ABSTRACT

Title of Dissertation: PUBLIC INTERVENTION AND HOUSEHOLD BEHAVIOR.

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How does the distribution of power within the household affect the nutrition of its members? In 1998, the largest social program in rural Mexico (PROGRESA) designed a random experiment for the purpose of evaluation. Exploiting the experimental nature of the data, I estimate calorie demand equations based on the predictions from different models of household behavior. There are three main findings in the first chapter. First, I reject the income pooling restriction and the Pareto-efficiency assumption for the nutrition decisions of families assuming that only the head of household and his spouse participate in decision-making. Second, I reject the income pooling restriction in the context of the extended family, for which all income earners contribute to decision-making. Third, I show that changing the wife's non-labor income has little effect on the levels of food consumption in households with two decision-makers. In the extended family setting, I find that, for a given level of household income, an increase in the number of income earners is associated with a decrease in calorie consumption. Yet, when a female household member starts earning income, family calorie consumption

increases. When it is a male household member who starts earning income, family calorie consumption decreases.

In the second chapter, we investigate heterogeneity in program impact for the Mexican social program PROGRESA, which is a means-tested conditional cash transfer program implemented in rural regions of the country. The “common effect” model in program evaluation assumes that all treated individuals have the same impact from a program. Does the program have the same effect on everyone? Will some groups benefit more from the program than others? The design of PROGRESA provides a theoretical motivation for exploring heterogeneity in program impacts. We examine the program targeting mechanism and find heterogeneity in the eligible population along the criteria used for beneficiary selection. We also investigate the overall heterogeneity of program impacts, which includes both observed and unobserved heterogeneity. Experimental data are sufficient to identify mean program impacts or impacts on subgroups, but do not identify unobserved heterogeneity in impacts. Using a non-parametric technique, we find evidence against the “common effect” model. This result does not rely on any assumption and thus is particularly strong evidence of heterogeneous treatment effects. Additional assumptions allow us to further analyze the distribution of impacts.

PUBLIC INTERVENTION AND HOUSEHOLD BEHAVIOR.

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Dedication

To my parents.

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1.1 Introduction

Many of the social programs currently in place in Latin America intentionally direct monetary transfers at the female head of the household (usually the mother). This design reflects the belief that mothers care more about family well-being than fathers do. Directing transfers to women also implicitly questions unitary preference household models. According to these models, it should not matter who in the household receives the transfer, since optimal choices for the allocation of resources are made subject to a pooled budget constraint. The income pooling restriction provides a testable implication of the unitary model.

There is substantial evidence against the unitary model (e.g. Thomas 1990 and Schultz 1990 in the context of developing countries; Bourguignon, Browning, Chiappori and Lechène 1993, Fortin and Lacroix 1997 and Phipps and Burton 1998 for developed countries). However, empirical evidence on the effect of the distribution of income on household decisions may suffer from issues of endogeneity, measurement error and the lack of support in the data for the joint distribution of incomes. Studies that exploit an exogenous change in the intrahousehold distribution of income to identify the effect of the distribution of income (e.g. Lundberg, Pollak and Wales 1997; Attanasio and Lechène 2002, Duflo 2003) constitute convincing pieces of evidence.

The collective model of the household (Chiappori 1988, Browning and Chiappori 1998) acknowledges that household members have different preferences and that a household should not be treated as a single unit. The distribution of power within the household influences allocations. The main assumption of this model is that the collective allocation of resources is Pareto-efficient. The model imposes testable restrictions on the way in which distribution factors can enter demand equations.

Much of the evidence on whether households attain Pareto-efficient outcomes is drawn from developed countries (e.g. France, Canada), where indeed households are found to make Pareto-efficient consumption decisions. However, using data from sub-Saharan Africa on agricultural production decisions for plots operated by men and women living in the same household, Udry (1996) rejects the Pareto-efficiency assumption underlying the collective models.

How does the distribution of power within the household affect the nutrition of its members? The purpose of this study is to test the income pooling restriction and the Pareto-efficiency assumption on the nutrition decisions of poor rural Mexican households. Both restrictions are tested for households assuming that only husbands and wives participate in decision-making. In addition, the income pooling restriction is tested in the context of the extended family, where any other household member who earns income contributes to decision-making.

I rely on data collected for the evaluation of the Mexican program for education, health and nutrition, PROGRESA. The PROGRESA program is a means-tested social program that sends cash transfers to rural households upon compliance with a defined set of requirements. A distinctive feature of the design of the evaluation of PROGRESA is that for a group of eligible households, transfers were postponed until the end of the evaluation period.[1] The sample consists of randomly selected treatment and control villages. The control group, which is denied transfers, is found to be similar to the treated group on all other aspects (Behrman and Todd 1999).[2]

My paper contributes to the existing literature in four ways. First, I apply the models of household behavior to the nutrition decisions. There are two reasons for focusing on nutrition decisions. First, food expenditures represent 75 percent of the household budget of these poor rural households. I consider a food calorie aggregate that better captures the nutritional status of households than food expenditures in rural regions where consumption out of own production is important.[3] Second, the higher the level of aggregation for the goods consumed, the more likely households will be found to attain Pareto-efficient outcomes. For example, households may be efficiently allocating resources towards the aggregate outcomes for nutrition, clothing and education but this could hide an inefficient allocation of resources within each of these categories.

Second, I correct for the endogeneity issues between spouses' incomes and calories by getting additional identifying restrictions for the spouses' incomes from a specific module of the survey that collects characteristics of the spouses' families at the time of marriage. Along with the treatment dummy variable from the experiment, this set

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[1] A random assignment of potential beneficiaries in treatment and control groups was conducted in order to evaluate the actual impacts of the program on a range of outcomes. These impacts are assessed using estimators commonly used in program evaluation (e.g. difference-in-difference estimator). Detailed data description and mean impact results are presented in a series of research reports (see http://www.ifpri.org/themes/progresa.htm).

[2] Behrman and Todd (1999) do find significant differences in the baseline at the individual and household levels. They argue that the differences appear to be statistically significant because of the large sample size. I include household-level and individual-level variables in the conditioning set to control for these differences. Recalling that the randomization is done at the locality level and not the individual or household level, this finding does not indicate a failure in the randomization process. Randomization of a social program at the individual or household level is usually found to be more disruptive than randomization at the locality level.

[3] As an example, as many as half of the households report consuming but not purchasing tortillas, a staple food, in the previous seven days.

of instruments is likely to lead to precise estimates of the effect of the distribution factors on the demand for calories because of the strong correlation between the instruments and the endogenous variables. It is possible that most studies fail to reject Pareto-efficiency because the effects of the distribution factors are imprecisely estimated.

Third, I test the unitary model of decision-making in the context of the extended family. For a given level of household income, changes in the number of income earners should not lead to changes in per capita calorie consumption if the unitary model holds. In this part, longitudinal data over three rounds are used.

Fourth, I present evidence on the extent to which inappropriately neglecting the distribution of power within the household affects inferences about the demand for nutrition. In response to the emerging literature on intrahousehold allocation and bargaining power, Rosenzweig (1986) pointed out that the collective models "have not provided clear directions as to how intrahousehold allocation *will* differ when some individual attains more bargaining strength" (p.236). I examine how changes in the distribution of income impact the levels of calories consumed in the household.

There are three main findings to this study. First, I reject the income pooling restriction and the Pareto-efficiency assumption for the nutrition decisions for families when only the head of household and his spouse are assumed to participate in decision-making. Second, I reject the unitary model in the context of extended family decision-making. Third, I show that changing the wife's income has little effect on the levels of food consumption in households with two decision-makers. In the extended households, I find that calorie consumption increases when a woman starts earning income and decreases when it is a man who starts earning income.

The paper is organized as follows. In the following section, I describe the PROGRESA program, the experimental evaluation sample and the sample used in this study. In the third section, I model the household nutrition decisions using the collective framework. In the fourth section, I present the empirical models, the tests of income pooling and the test of Pareto-efficiency for the allocation decisions with respect to nutrition. In the fifth section, I review the empirical issues and present the estimation strategy adopted. In the sixth part, I discuss the estimation results. In the last section, I conclude.

1.2 The Data

1.2.1 Description of the PROGRESA program

I rely on data collected for the evaluation of the Mexican program for education, health and nutrition, PROGRESA. The PROGRESA program targets poor rural households in Mexico. It has been implemented since 1998. At the end of 1999, it covered 2.6 million families, i.e., about 40% of all rural households and one ninth of all families in Mexico. In 1999, the program's annual budget was approximately $777 million, which corresponds to 0.2 percent of Mexico's GDP (Skoufias, 2001). In January 2002 the Inter-American Development Bank approved its largest loan ever to Mexico for expanding PROGRESA to urban areas of the country. Despite the recent important political changes, PROGRESA has been maintained under the new name Opportunidad.

A baseline census of households provides the information used to determine the eligibility status of the households. Basically, a poverty line is drawn and only households below the poverty line are classified as eligible for program benefits. On average, 78 percent of each locality's population are found to be poor, that is, eligible for the program benefits.

The program benefits are comprised of two components:

1. Educational grants given to families with children in the last three years of primary school and secondary school children. The grant amounts vary by grade and gender, with greater awards to girls and to the most advanced children. The grants are given upon attendance to school. A complex system of verification based on forms completed and signed by teachers and school directors ensures that the attendance requirement is met before sending money to the households.

2. All eligible households can benefit from a monetary transfer designed to help them improve their nutrition. They are encouraged to spend the money on food although not required to do so. In order to receive this cash transfer, they are required to make regular visits to health centers and to participate in health talks. Only one visit per year to a health center is required for adults, two to five visits a year for pregnant and breast-feeding women and two to seven visits a year for infants and children. In addition, nutritional in-kind supplements are provided to under-nourished children and infants and pregnant and breast-feeding women.

An important characteristic of the PROGRESA program with respect to this study is that transfers are made to the mother in the household. Two additional features are that (1) transfers to a household cannot go beyond a given maximum transfer amount regardless of the number of eligible school-age children, (2) transfers are sent to the beneficiaries every two months after checking their compliance with program requirements. For example, the average transfer from October 1998 to November 1999 is about 197 pesos per household per month, which is equivalent to 20% of the mean value of consumption of a poor household. [4]

1.2.2 Experimental design for the evaluation of PROGRESA

The evaluation study is designed as an experiment with localities randomly assigned to treatment and control groups. Only eligible households in treatment localities receive benefits after the start of the program in mid-1998. During the evaluation period, benefits are denied to eligible households in the control group localities. Non-eligible households in the treatment and control localities do not receive benefits. The sample includes 506 localities (320 assigned to the treatment group and 186 to the control group).

The evaluation dataset consists of repeated observations for about 24,000 households over 5 rounds of survey (baseline: October 1997 and March 1998; follow-ups: November 1998, June 1999 and November 1999). Data are collected to capture the multiple objectives of the program in terms of human capital investment and poverty alleviation. The last three surveys collect both household-level expenditure and individual-level labor activities and non-labor income (Table 1.1).[5] They also include individual-level information on schooling, migration and health status, and locality-level information on the availability of services and prices. In addition, a module was specially designed to get information on the status of women and on intrahousehold relations.[6] I also use administrative data on the actual PROGRESA transfers received by beneficiary households in the treatment group in 1998, prior to the November 1998 survey.

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[4] The figures are in November 1998 pesos and the value is approximately $20 U.S.
[5] All tables of chapter 1 are in the Appendix 1.
[6] For a description of the module on women's status and intrahousehold relations, see Adato *et al.* (2000).

1.2.3 Sampling frame

In this paper, I use two datasets: (i) the November 1998 cross-section focuses on households with two decision-makers, and (ii) longitudinal data from November 1998, June 1999 and November 1999 allows me to study decision-making in extended families.

The two decision-makers setting relies on the assumption that, even when household members other than the head of household and his spouse earn income, the sole two decision-makers are the head and his spouse.[7] This is a strong assumption, which is relaxed in the extended family setting. The extended family setting allows every household member who earns income to participate in decision-making.

The November 1998 survey is collected for 24,073 households (136,250 individuals). I select 20,925 households for which there is an intact couple. I restrict the sample to 17,382 households with a male head of household and his wife who are more than 15 years old and who report some income (labor earnings or non-labor income) and consumption data.

In the end of 1998, a new computation of the poverty line led to the inclusion in the eligible population of 899 households who were previously ineligible for program benefits. This process is referred to as the "densification" process. It affected all eligible localities enrolled in the PROGRESA program. I exclude these households from the analysis because they have not yet received the program benefits at the time of the November 1998 survey although they probably anticipate becoming eligible.

[7] Note that another way of defining two decision-maker families is to restrict the sample to households where only the husband and/or the wife earn income. Yet, this means restricting the sample based on a variable that may be affected by the PROGRESA treatment, namely having an additional earner in the family. Indeed, when comparing the fraction of the treatment and control groups that are dropped when applying this restriction to the sample, I find that respectively 34% of the control group households and 25% of the treatment group households are dropped. Thus, I do not apply this restriction to the data.

The partition between the treatment and the control group and the eligible and non-eligible groups for the remaining 16,483 households is given in Table 1.2. I restrict the sample further to the 9,223 eligible households.[8] This restriction is justified by the identification strategy used in the empirical section. Table 1.3 presents descriptive statistics for the variables in the selected sample.

Longitudinal data from November 1998, June 1999 and November 1999 are used to study decision-making for extended families. In all three rounds, data are collected on household nutrition, consumption, labor activities and income. In contrast to the 1998 cross-section sample, I keep all households for which consumption data are reported, including eligible and non-eligible households. There are a total of 18,790 households in each round. Changes in the number of individuals earning income are shown in Table 1.4. Table 1.5 shows other descriptive statistics for this dataset.

1.2.4 Expenditures, Nutrients and Incomes

Food expenditures include household level data on food outlays made in the seven days preceding the interview for 36 food items. The value of food consumed from own production in that same period of time is added to food outlays to obtain the value of food consumption. Food consumed from own production is valued by imputing either a household-level price or a locality level price when the household does not report any expenditures on the food consumed from own-production. Non-food expenditures are expenses reported on a weekly, monthly and semi-annual basis. Non-food expenses

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Instead, I impose the assumption that only the head of household and his spouse participate in decision-making.

[8] Because some variables have missing data, the sample sizes used in the estimation are sometimes smaller than 9,223. This is particularly true when I use the spouses' family background variables as exogenous variables.

reported on a weekly basis include transportation and tobacco. Monthly outlays include school tuition, health-related expenses, home cleaning, electricity and home fuel expenditures. Expenditures reported on a semi-annual basis include home and school supplies, clothes, shoes, toys and payments for special events. The value of consumption is computed as the sum of non-food expenditures and the value of food consumption.

The measure of food calories consumed is constructed from the 7-day recall food consumption data. First, the number of units of the 36 food items bought and consumed in the household are converted into kilograms. The second step is to calculate for each food item the "edible" kilograms of food. The edible kilograms are converted into kilocalories.[9] Lastly, instead of dividing this total household calorie consumption by the number of household members, an adjustment is made for the fact that some household members ate outside the home and some non-household members ate in the home during the study period. This adjustment consists in subtracting from household size the number of people having food outside and adding the number of people eating in the household. Thus, the aggregate food calories are represented by the daily per mouth measure of food calories consumed. I also consider aggregate calories consumed from four different food groups, i.e. calories from vegetables and fruits, calories from grains and cereals, calories from meat and meat products and calories from other food.

The income data are comprised of labor and non-labor incomes. Labor income is constructed using wages and the value of employer-provided benefits from all activities. Most of the individuals work as agricultural workers paid on a daily basis. The second

[9] All conversions are based on Mexican food tables from *Tablas de Valor Nutritivo de los alimentos de mayor consumo en Mexico* (1996).

most common type of occupation for heads of household is self-employment.[10] Respondents are probed several times in the questionnaire to elicit all labor earnings. In particular, a section of the survey directly concerns more informal types of activities such as sewing and craft-making, cooking and home-cleaning, building, repairs and driving. Non-labor income is comprised of pensions, bank interest, rents and other revenues, government transfers and remittances. I also include the actual PROGRESA transfers in the wife's non-labor income for beneficiary households in the treatment communities. The average monthly transfer actually received by the women is about 125 pesos (in 1998 figures). This figure is about three times less than the payment women are entitled to and which is computed using the program rules and household composition. Much of the difference between actual and hypothetical payments comes from operational problems in transfer delivery (Coady and Djebbari, 1999).

In the extended family analysis, any individual who earns some labor or non-labor income is considered as an income earner. The number of income earners changes when household members enter or exit the labor force, start or stop receiving non-labor income. The number of earners also changes when one income earner joins the household or leaves the household. This variation occurs at the same time as the variation in household size. In the estimations, I control for the change in the number of household members.

[10] Elsewhere in the questionnaire, detailed information on agricultural activities is collected. However, net profits computed as the difference between sales of agricultural products and expenses on inputs are negative for most of the respondents, which is not plausible. In addition, it is far from straightforward how to assign the agricultural profits to individuals. Therefore reported income from self-employment is the preferred measure of income from agriculture for non-wage-earner farmers.

1.3 Applying the models of household behavior to household nutrition

1.3.1 The unitary model of the household

Suppose households derive utility *U* not from nutrition *N* directly, but from the effect of nutrition on health *H*. The production of health is also assumed to be affected by household characteristics θ_h (e.g. innate health status, access to a sewage system and to electricity) and locality characteristics θ_l (e.g. access to health facilities). Nutrition itself depends on the consumption of food X_k. In addition, the choice of the diet determines the absorption of nutrients. Thus, nutrition *N* also depends on household-specific characteristics μ_h that include the education of each spouse. Suppose that the household consumes *K* different food items with price p_k along with a composite good *Z* (p_z = 1) and that total household income is *Y*. Households solve the following problem:

$$\begin{aligned}
&\underset{Z,X_1,\ldots,X_K}{\text{Max}}\ U(H,Z) \text{ subject to:}\\
&\sum_{k=1}^{K} p_k X_k + Z = Y,\\
&H = H(N,\theta_h,\theta_l),\\
&N = N(X_1,\ldots,X_K,\mu_h).
\end{aligned}$$

The demands for food are functions of prices, total household income and household and locality characteristics as given in equation (1):

$$\text{(1)} \qquad \text{for } k = 1,\ldots,K : X_k = X_k(p,Y,\theta_h,\theta_l,\mu_h).$$

The unitary model embodies an important assumption with regards to household preferences. A household is assumed to behave "as one". This occurs if all household members have the same preferences or if one household member imposes his preferences, acting as a dictator. The only economic justification for the unitary model is Becker's Rotten Kid Theorem (Becker, 1974), which holds under restrictive conditions

(Bergstrom, 1989). A testable implication of the unitary model is that, once you condition on household expenditures, individual incomes should not have any effect on demand. Consider the previous model assuming that only the husband and the wife participate in household decision-making. Let *WNLY* be wife's non-labor income. Testing the unitary model restriction consists in testing that *WNLY* has no effect on the demand for food X_k, after controlling for all other factors affecting X_k.[11] Thus, testing income pooling is based on the null hypothesis defined below:

$$\boldsymbol{H_o} : \forall k : \quad \frac{\partial X_k(p, Y, \theta_h, \theta_l, \mu_h)}{\partial WNLY} = 0.$$

Alternatively, if data on husband's earnings *HLY* are also available, testing the unitary model can be based on the following joint hypothesis:

$$\boldsymbol{H_o} : \forall k : \quad \frac{\partial X_k(p, Y, \theta_h, \theta_l, \mu_h)}{\partial WNLY} = \frac{\partial X_k(p, Y, \theta_h, \theta_l, \mu_h)}{\partial HLY} = 0.$$

Similar tests have been proposed in the literature for different *X* outcomes. The most common outcomes include a system of budget shares for different categories of goods[12] and health status of children.[13]

[11] I use woman non-labor income instead of woman total income because only 5 percent of the women in the sample earn any labor income. In contrast, more men earn labor income than non-labor income.

[12] Thomas (1993) finds a differential effect of individual incomes on budget shares for urban Brazil. Bourguignon, Browning, Chiappori and Lechène (1993) find similar evidence from French data using individual incomes and total household income. Hoddinott and Haddad (1995) using data from Cote-d'Ivoire show that the wife's share of income significantly affects budget shares. Doss (1996) finds that household budget shares in Ghana depend on the share of current assets held by women. Browning and Chiappori (1998) reject the unitary model of the household with Canadian data on budget shares and individual incomes. Attanasio and Lechène (2002) find that the wife's share of income significantly affects the budget shares with the same sample of Mexican households I use in this study.

[13] Thomas (1990) finds a differential effect of individual incomes on anthropometric measures for children and children's survival probabilities in urban Brazil. Haddad and Hoddinott (1994) find an effect of the wife's share of income on anthropometrics of Ivorian children. Thomas (1994) presents evidence for urban Brazil, urban Ghana and the US of an effect of parents' education on child health. Thomas, Contreras and Frankenberg (2002) provide evidence of a differential effect of assets brought at time of marriage by the father and the mother on child health. Duflo (2003) finds that the presence of an elderly woman eligible for an old-age pension plan is associated with a large impact on the health of girls residing in the same household. This effect is negligible for elderly men on both girls and boys residing in the same household.

Individual total incomes, non-labor incomes and labor earnings, current assets and assets at the time of marriage for each spouse are all factors that are assumed to affect the distribution of power within the household. According to the unitary model, none of these factors should affect the demand for goods. The choice of factors in each study strongly depends on the assumptions made on the issues prevailing in the estimation. Although individual incomes are likely to be related to the distribution of power within the household, it is difficult to argue that the distribution of income is exogenous to household demand for goods. Thus, some studies rely on context-specific features of the population under study to select the distribution factors that are likely to affect the distribution of power within the household, such as assets brought at time of marriage.[14]

1.3.2 The collective model of the household

In contrast to the unitary model, the collective model only imposes Pareto-efficiency on the allocation decisions of the individual household members. The outcome is Pareto-efficient if no one in the family can be made better off without making someone else worse off.

As before, I consider a model where only the head of household and his spouse participate in the decision-making. The utility functions U_f and U_m are assumed to satisfy the standard differentiability conditions. Both utility functions depend on the household health status. As before, health H production is affected by the nutritional status of the household N and by household and locality characteristics θ_h and θ_l. Nutrition itself

Concerning household nutrition, Thomas (1990) finds a differential effect of husband's income and wife's income on the per capita intakes of calories and proteins with data from urban Brazil.

[14] For example, in Indonesia, women are found to retain property rights to assets brought at time of marriage, which justifies the use of these assets as distribution factors (Thomas, Contreras and Frankenberg 2002).

depends on the consumption of K food items $X_1,\ldots, X_k$ and household characteristics μ_h. The family with total income Y optimally chooses to consume quantities $X_1,\ldots, X_k$ of K food items at prices $p_1,\ldots, p_k$ and a composite good Z, The collective model imposes that the basket of goods collectively chosen by the spouses is Pareto-optimal. The optimization problem can be written as follows:

$$
\begin{aligned}
\underset{Z,X_1,\ldots,X_k}{\text{Max}}\ & U_f(H,Z) \text{ subject to:}\\
& U_m(H,Z) \geq \overline{U}\\
& \sum_{k=1}^{K} p_k X_k + Z = Y,\\
& H = H(N,\theta_h,\theta_l),\\
& N = N(X_1,\ldots,X_K,\mu_h).
\end{aligned}
$$

In the dual problem, the household maximizes a weighted sum of the husband's utility and wife's utility. Let $\lambda(A^1, A^2)$ be the factor weighting the spouses' preferences in the household welfare function, where A^1 and A^2 are distribution factors affecting the distribution of power within the household. The factor $\lambda(A^1, A^2)$ is not a preference parameter. It does not affect the preferences of the decision-makers. It only weights their respective utility functions in the household objective function. The weight factor also depends on all the exogenous variables of the optimization problem. Yet, it is the additional distribution factors A^1 and A^2 that identify the collective model. Finding such factors that are independent of preferences, budget constraint and technology constraints but affect choices is a challenging task. The dual problem is as shown below:

$$
\begin{aligned}
\underset{Z,X_1,\ldots,X_k}{\text{Max}}\ & \lambda\, U_f(H,Z) + (1-\lambda)\, U_m(H,Z) \text{ subject to:}\\
& \sum_{k=1}^{K} p_k X_k + Z = Y,\\
& H = H(N,\theta_h,\theta_l),\\
& N = N(X_1,\ldots,X_K,\mu_h).
\end{aligned}
$$

The unitary model is a special case of the collective model wherein individual household members have the same preferences (i.e., $U_f(.) = U_m(.)$). In the collective model, the demands for food X_k are given by:

$$(2) \qquad \forall k: \quad X_k = X_k(p, Y, \lambda(p, A^1, A^2), \theta_h, \theta_l, \mu_h).$$

Pareto-efficiency can be empirically verified by testing the hypothesis that the ratio of the effects of the two distribution factors across pairs of goods is constant. This ratio would be equal to one if the unitary model holds. The idea is that the distribution factors, such as individual non-labor incomes, only affect consumption of a good through their effect on the factor weighting the utility function of each partner in the household objective function. The Pareto-efficiency assumption is justified if one considers the family as the place where household members play a repeated game and each household member knows the preferences of the other household members in the household.

For any two goods *k* and *l*, a testable implication of Pareto-efficiency[15] is captured by the null hypothesis:

$$\boldsymbol{H_0}: \forall k, l: \quad \frac{\partial X_k / \partial A^1}{\partial X_k / \partial A^2} = \frac{\frac{\partial X_k}{\partial \lambda} * \frac{\partial \lambda}{\partial A^1}}{\frac{\partial X_k}{\partial \lambda} * \frac{\partial \lambda}{\partial A^2}} = \frac{\partial \lambda / \partial A^1}{\partial \lambda / \partial A^2} = \frac{\partial X_l / \partial A^1}{\partial X_l / \partial A^2}.$$

Testing the null hypothesis of Pareto-efficiency requires selecting variables that represent the distribution factors A^1 and A^2. Bourguignon, *et al.* (1993) find that the ratio of the effects on commodity demands of each household member's individual income are constant across goods using data on French households in which both spouses work full time and have at most one child. Thomas and Chen (1994) provide similar evidence for households in Taiwan. Browning and Chiappori (1998) use the log of the ratio of wife's earnings to husband's earnings and the wife's log earnings as the two distribution factors to test for Pareto-efficiency in a budget shares system for Canadian households. They cannot reject Pareto-efficiency. Thomas, Contretas and Frankenberg (2002) provide evidence that Indonesian households make Pareto-efficient decisions with respect to children's health. They consider the value of assets brought to marriage by each spouse as the two distribution factors affecting the distribution of power within the household.

In contrast, using data from sub-Saharan Africa on agricultural production decisions for plots operated by men and women living in the same household, Udry (1996) rejects the Pareto-efficiency assumption underlying the collective models. He explains the inefficiency as the consequence of missing markets in land, labor and/or assets in Ghana. Similarly, expenditure patterns of households in Cote d'Ivoire are found to be Pareto-inefficient (Duflo and Udry, 2003). According to the authors, the allocation of resources is dictated by social norms. The inefficiency arises because of the lack of cooperation in the household generated by information asymmetries and/or enforcement problems. In addition, using Canadian data, Phipps and Burton (1998) find that income pooling can be rejected for certain goods but not for others. This suggests that the ratio of male and female income effects across pairs of goods is not constant.

Finally, non-Pareto-efficient decisions are consistent with a model in which each spouse is responsible for making decisions on different goods, as in the separate sphere bargaining model of Lundberg and Pollak (1993). In the noncooperative equilibrium, family public goods are under-supplied because of the lack of coordination between the

[15] In order to test for Pareto-efficiency, one needs to identify two factors that affect the distribution of power within the household. Testing income pooling only requires the use of one distribution factor.

individual household members. Yet, the supply of public goods is higher in the case where traditional gender roles serve as a focal point for the division of responsibilities than in a setting with independent optimizing individuals. Pareto-efficient outcomes could emerge from cooperative bargaining if the transaction costs related to the negotiation, monitoring and enforcement of the agreements between household members are low compared to the benefit of moving from the traditional gender role equilibrium to a Pareto-efficient equilibrium.

1.3.3 Decision-making in the extended family context

In the extended family setting, all income earners participate in the household decision-making. If the income pooling restriction holds in this context, a test of the unitary model is based on the result that, once total expenditures are controlled for, individual incomes should not affect the demand for goods. There is an issue in the extended family setting with adding many individual income variables to perform this test because the lack of variation in the income data could lead to very imprecise estimates for each individual income effect.

Yet, in the unitary model of household behavior, for a given budget constraint, the number of income earners should not affect the allocation of resources. Thus, I exploit this restriction in order to test the unitary model in the extended family context. All of the literature on intrahousehold bargaining focuses on families with two decision makers. Thus, this comes as a first test of the unitary model in the extended family context.

1.4 Empirical model

1.4.1 A setting with two decision-makers

The unitary model implies that how much each spouse contributes to the household should not matter, only total income matters. More generally, no distribution factor should enter the demand for calories once we control for total household expenditures, differences in tastes and household technology. A distribution factor is defined as a factor that shifts power within the household but does not affect household preferences or technology directly nor the budget line. Testing the Pareto-efficiency assumption requires the use of two distribution factors and several goods. Following Browning and Chiappori (1998), I estimate two models for families where only the head of the household and his spouse are assumed to participate in decision-making. I consider the extended family setting in the next sub-section.

The restricted collective model (2.1) includes only one distribution factor, the log of the wife's non-labor income. The unrestricted collective model (2.2) is applied to the demand for calories from four food groups, i.e. calories from vegetables and fruits, calories from cereals and grains, calories from meat and meat products and calories from other food. Model (2.2) includes two distribution factors, the log of the wife's non-labor income and the log of the husband's labor income.[16] The unrestricted collective model allows testing of the Pareto-efficiency assumption for the nutrition decisions. Apart from the distribution factors, the equation describing the household demand for calorie

[16] The use of natural logarithms allows interpreting the coefficients as elasticity measures. The logarithmic transformation is useful when the distribution of a variable in level is highly skewed, which is the case for expenditures, income and calorie consumption.

consumption includes the log of the per capita value of consumption.[17] Value of consumption is the preferred proxy for household wealth because it fluctuates less than current income.[18] Note that one peso is added to both the wife's income and the husband's income. The restricted collective model is as follows:

$$(2.1) \qquad \ln Cal = \alpha + \beta \ln PCE + \gamma \ln WNLY + Z\theta + \varepsilon,$$

$\ln Cal$ is the log of per mouth household calorie consumption,
$\ln PCE$ is the log of per capita value of consumption,
$WNLY$ is the log of the wife's non labor income,
Z is a vector of household and locality characteristics,
ε is the error term,
$\alpha, \beta, \gamma, \theta$ are parameters to be estimated.

The models underlying equations (1) and (2) require that the prices of goods be included in a demand function. Prices of the ten most commonly consumed food items, i.e. tomatoes, onions, maize tortillas, noodle, rice, beans, eggs, coffee, sugar and vegetable oil, are included in the Z vector.[19] Household size and household composition are included to capture the effect of economies of scale. The number of individuals in different age and gender groups and the logarithm of household size capture household composition. Households derive utility not from nutrition directly, but from the effect of nutrition on health. Thus, I add household and village characteristics that affect the production of health and nutrition (e.g. presence at the locality level of health facilities, access to a sewage system and to electricity, husband's years of education and wife's

[17] The value of consumption is denoted lnPCE and β is referred to as the expenditure elasticity of calories for the sake of brevity. Strauss and Thomas (1990) estimate the shape of the calorie expenditure curve non-parametrically using data from urban Brazil. Bhalotra and Attfield (1998) carry out a semi-parametric estimation of the income-nutrition relationship for rural Pakistan. Both find that the calorie expenditure curve is non-linear in the sense that the elasticity of calories decreases with household wealth. The PROGRESA program targets the poorest households thus the sample is likely to be homogeneous in terms of income. In this case, non-linearities are likely to be nonexistent.
[18] Seasonality needs to be taken into account when measuring income, especially agricultural income. Households are also more likely to smooth consumption over time than income (see Deaton 1997, pp.26-32).

years of education). Husband's education and wife's education are also likely to capture taste differences between households. If they are not included, these variables would be absorbed into the error term and would likely generate spurious correlation between the error term and the income variables.

The unrestricted collective model for calories consumed from four different food groups is obtained by adding husband's earnings as an additional distribution factor.

The unrestricted model is as follows:

$$(2.2)\ \forall k: \qquad \ln Cal^k = \alpha^k + \beta^k \ln PCE + \gamma_1^k \ln WNLY + \gamma_2^k \ln HLY + Z\theta^k + \varepsilon^k,$$

$\ln Cal$ is the log of per month household calorie consumption from food group i,
$\ln PCE$ is the log of per capita value of consumption,
$\ln WNLY$ is the log of the wife's non labor income,
$\ln HLY$ is the log of the husband's labor income,
Z is a vector of household and locality characteristics,
ε is the error term,
$\alpha, \beta, \gamma, \theta$ are parameters to be estimated.

In order to test for the Pareto-efficiency assumption, distribution factors should enter all equations for the four food groups in the same way. However, note that for any differentiable monotonic function $\phi(.)$ of A^i, the Pareto-efficiency condition stated above remains unchanged:

$$\forall k,l: \quad \frac{\partial X_k / \partial\phi(A^1)}{\partial X_k / \partial A^2} = \frac{\partial X_l / \partial\phi(A^1)}{\partial X_l / \partial A^2},$$
$$\forall k,l: \quad \frac{\partial X_k / \partial A^1}{\partial X_k / \partial\phi(A^2)} = \frac{\partial X_l / \partial A^1}{\partial X_l / \partial\phi(A^2)}.$$

Thus, the test result should be the same whether husband’s income and wife’s income enter the equation in levels or in logarithmic form.

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[19] At least 50% of the sample reported some consumption of each of these food items.

1.4.2 The extended family setting

In the extended family set-up, all income earners participate in decision-making. That is, extended families include any households where the head and/or his spouse is likely to share with other household members the decision-making on the allocation of total household income because the other household members contribute to household income.

From one round of the survey to the next, some individual household members start earning income and others quit their job, are fired, retire or lose their non-labor income. For a given level of household income, changes in the number of income earners should not affect changes in per capita calorie consumption. The estimation is based on a first-difference model that relates changes in calorie consumption to changes in the number of income earners. The first-differencing transformation is denoted by Δ, and is equal to the change from period *(t-1)* to period *t* of the variables of interest. In the transformation, the first round of data is lost. The empirical model is based on the estimation of equation (2.3):

$$(2.3) \qquad \Delta \ln Cal = \alpha + \beta \, \Delta \ln PCE + \gamma \, \Delta NEARNERS + \Delta Z \, \theta + \Delta \varepsilon,$$

$\ln Cal$	is the log of per mouth household calorie consumption,
$\ln PCE$	is the log of per capita value of consumption,
NEARNERS	is the number of individuals earning some income,
Z	is a vector of household characteristics,
ε	is the error term,
$\alpha, \beta, \gamma, \theta$	are parameters to be estimated.

In addition, I distinguish between the effect of an additional female earner from the effect of an additional male earner, as well as the effect of an additional household member who starts earning income from the effect of an additional household member who stops earning income. I estimate the following equation:

(2.4) $$\Delta \ln Cal = \alpha + \beta\, \Delta \ln PCE + \gamma_{me}\, \Delta Nmenter + \gamma_{fe}\, \Delta Nfenter + \gamma_{md}\, \Delta Nmdrop + \gamma_{fd}\, \Delta Nfdrop + \Delta Z\, \theta + \Delta\varepsilon,$$

$\ln Cal$	is the log of per mouth household calorie consumption,
$\ln PCE$	is the log of per capita value of consumption,
$\Delta Nmenter$	is the number of male who starts earning some income,
$\Delta Nmdrop$	is the number of male who stops earning some income,
$\Delta Nfenter$	is the number of female who starts earning some income,
$\Delta Nfdrop$	is the number of female who stops earning some income,
Z	is a vector of household characteristics,
ε	is the error term,
$\alpha, \beta, \gamma_g, \theta$	are parameters to be estimated, $g = me, fe, md, fd$.

1.4.3 Testing models of household behavior

1.4.3.1 Testing the unitary model

In the two decision-makers setting, the restricted collective model (2.1) allows me to test whether the unitary model holds for total calories consumed by testing whether, once you condition on household expenditures, the wife's non-labor income affects the quantity of calories consumed by the household. The test is based on the null hypothesis:

$$\boldsymbol{H_0}: \quad \gamma = 0.$$

If applied to the demand for calories from the four different food groups, the restricted collective model (2.1) can also be used to test the unitary model restriction for the demands for calories from each food group.

Alternatively, the unrestricted collective model (2.2) provides a basis for testing the unitary model restriction for the calories consumed from any food group *k* using the following hypothesis:

$$\boldsymbol{H_0}: \quad \gamma_1^k = \gamma_2^k = 0.$$

In the extended family framework, testing the unitary model restriction consists in testing the hypothesis that changes in the number of income earners do not affect on the change in calorie demand conditional on the change in consumption, based on equation (2.3):

$$\boldsymbol{H_0}: \quad \gamma = 0.$$

Alternatively, if based on equation (2.4), testing the unitary model consists in testing the following null hypothesis:

$$\boldsymbol{H_0}: \quad \gamma_{fd} = \gamma_{fe} = \gamma_{md} = \gamma_{me} = 0.$$

1.4.3.2 Testing the Pareto-efficiciency assumption

Testing the Pareto-efficiency assumption requires the estimation of the unrestricted collective model (2.2) for the various food groups. A Wald test for non-linear restrictions is computed on any single pair of goods (k, l) for the following null hypothesis:

$$\boldsymbol{H_0}: \quad \frac{\gamma_1^k}{\gamma_2^k} = \frac{\gamma_1^l}{\gamma_2^l}.$$

Testing ratios of coefficients can be problematic when denominators are close to zero. An alternative version of this test for any single pair of goods (k, l) is specified as:

$$\boldsymbol{H_0}: \quad \gamma_1^k \times \gamma_2^l - \gamma_2^k \times \gamma_1^l = 0.$$

In order to test for Pareto-efficiency in a joint manner for the calories consumed from the different food groups, a Wald test is computed for the following non-linear restrictions:

$$\begin{aligned} \boldsymbol{H_0}: \quad & \gamma_1^1 \times \gamma_2^2 - \gamma_2^1 \times \gamma_1^2 = 0, \\ & \gamma_1^2 \times \gamma_2^4 - \gamma_1^4 \times \gamma_2^2 = 0, \\ & \gamma_1^3 \times \gamma_2^4 - \gamma_1^4 \times \gamma_2^3 = 0. \end{aligned}$$

Under the null hypothesis, the Wald test is asymptotically distributed as a chi-square with three degrees of freedom. Rejecting the joint null hypothesis is a rejection of the Pareto-efficiency assumption underlying the collective model. This is the most general test for the Pareto-efficiency assumption but it is based on a non-linear hypothesis. The power of the Wald test may be limited, especially when an efficient estimator is not used. The precision of 2SLS estimates is related to the correlation between the endogenous variable and the instruments, as shown below:

$$\text{Est.}Var(\gamma^{IV}) = s^2[X'W(W'W)^{-1}W'X]^{-1},$$

γ^{IV} is the IV estimate for the effect of the distribution factors,
s^2 is the estimate for σ^2,
X is the set of exogenous variables,
W is the set of instrumental variables.

A strong correlation between X and W is associated with more precise IV estimates of the effects of the distribution factors. In the opposite, the use of weak instruments may lead to a failure to reject the null hypothesis of Pareto-efficiency when it is false.

1.5 Empirical issues and estimation strategy

1.5.1 Estimation issues

1.5.1.1 Total expenditures

Estimating the expenditure elasticity of calories is problematic because of the likely endogeneity between expenditures and calories consumed (Bouis and Haddad 1992; Bouis 1994). There are three possible sources of endogeneity. First, when the total value of consumption is used to capture household wealth, then any measurement error in the food quantity data can be found in food calories (the dependent variable) as well as in the value of consumption (the explanatory variable). A problem of common measurement error arises. Second, data usually over-estimate consumption for rich households, because they include consumption by non-household members (e.g. employees) and usually under-estimate consumption for poor households, whose members often eat on the workplace (e.g. agricultural workers, domestic workers). The error term in the equation for calories consumed is then correlated with household wealth. Third, another potential endogeneity issue can rise from the existence of a feedback effect from nutrition to income, as described in the efficiency wage literature (Stiglitz 1976).

Two main approaches are taken to address the endogeneity problem. First, when panel data are available, one can specify a model with fixed effects or random effects estimates that allow for unobserved, time-invariant family heterogeneity (Behrman and Deolalikar 1987). Second, the value of consumption is instrumented using individual incomes or assets and household characteristics. Household or individual income is used if the main source of endogeneity comes from measurement error because household or individual incomes are highly correlated with total expenditures and uncorrelated with the measurement error in calories. Once we control for expenditures, income should not affect demand and can be excluded and used as an instrument. If endogeneity also arises from unobserved variables correlated with expenditures, then household assets can be used as instruments. Subramanian and Deaton (1996) review the empirical evidence on the food income elasticity. They show that the OLS estimate for the income elasticity of calories, after adjusting the calorie aggregate to account for household members eating outside the home and non-household members eating with the household, is in the range of 0.3-0.4.

1.5.1.2 Individual incomes

Three main empirical issues arise in estimating the effect of the intrahousehold distribution of income.

First, it is difficult to argue that the individual incomes are exogenous with respect to the outcome of interest. In the context of developed countries, researchers usually avoid this issue by restricting their sample to couples where both partners are full-time workers. If most of the variation in labor supply occurs between occupation, controlling for the occupations of husbands and wives becomes a mean to hold labor supply constant across households. In the case of developing countries, restricting the sample to couples where both spouses work full-time would lead to very selective samples.

The endogeneity issue concerns labor income and to a lesser extent non-labor income. Earnings are the product of a wage rate and the number of hours worked. It is plausible that decisions on labor supply and consumption are made jointly. Differential effects of husband's earnings and wife's earnings would thus be consistent with the unitary model when wife's income picks up something else. Phipps and Burton (1998) find that expenditures on restaurant meals respond more strongly to the wife's income than to the husband's income. In their review of the literature on bargaining and distribution in marriage, Lundberg and Pollak (1996) interpret this finding as a price effect. [20] In addition, non-labor income is often comprised of pensions which represent the outcome of past labor supply choices.

[20] The opportunity cost of the wife's time accounts for a large part of the cost of home prepared meals, which are a substitute for restaurant meals.

Second, the differential effect of male and female incomes can be driven by different measurement errors in male and female incomes. This issue raises concerns about the interpretation of the results (Haddad 1999).

Third, the data are usually concentrated in certain regions of the joint distribution of husband's income and wife's income. In particular, in most developing countries datasets, as in the PROGRESA control localities, women have little income. Some combinations of male and female income, such as high female income-low male income, are unlikely to be found in the data. Thus, a problem of support arises. This means that there is little variation to parametrically identify and single out the effects of husband's income and wife's income, making identification of the effects difficult. Because PROGRESA gives income to women in the treatment group, this dataset is useful for examining the role of the mother's income in household decisions.

In sum, studying the effect of the distribution of income on household decisions is complicated by issues of endogeneity, measurement error and the lack of support in the data for the joint distribution of incomes.

In addition to instrumenting for income, one way to get around these problems is to exploit exogenous changes in income. Lundberg, Pollak and Wales (1997) [21] and Duflo (2003) [22] exploit natural experiment settings characterized by an exogenous change in the intrahousehold distribution of income. These studies show that the distribution of

[21] Lundberg, Pollak and Wales (1997) exploit a natural experiment in the UK where child allowance benefits were redistributed to women to test for income pooling. However, along with a change in the distribution of income within marriage, a change in policy instruments, i.e. from tax relief to the father to direct child allowance to the mother, also occurred.

[22] Duflo (2003) exploits a natural experiment setting in South Africa. She finds that pensions received by women had a large impact on the anthropometric status of girls living in the same household and none on that of boys. In contrast, pensions received by men have no effect on either boys or girls. The differential effect on children's health of the old age pension in South Africa according to the gender of the recipient is strong evidence against the unitary model of the household.

income within the household impacts decisions and constitute convincing pieces of evidence. Although PROGRESA allocates benefits to women in randomly selected localities, in the ideal experiment for this study, PROGRESA would have allocated cash transfers to men in some randomly selected villages and to women in other randomly selected villages. In general, studies based on natural experiments do not provide direct substantive evidence on the extent to which changes in spouses' income translate into changes in consumption levels because the policy changes include more than just a change in income.

Using a model with family-specific fixed effect, Thomas, Contreras and Frankenberg (2002) find evidence of a differential effect of male and female value of assets brought at time of marriage on girls' health outcomes and boys' health outcomes in Indonesia. This study only exploits within-household variation but obtains estimates that are robust to measurement error. The authors find a differential effect of higher wife's assets for girls and boys, which provides evidence on gender discrimination. However, the authors fail to assess the magnitude of the effect of wife's assets on the levels of the children's health measures. Instead, what they estimate is the effect on the gap between health outcomes for boys and girls of an additional rupiah in the hands of the mother.

1.5.2 Estimation strategy

In this paper, I treat the value of consumption and the distribution factors in model (2.1) and model (2.2) as endogenous.

In the unitary model, I use two sets of instruments for the value of consumption: (i) dwelling characteristics, and (ii) dwelling characteristics along with total household income. The dwellings attributes I focus on are access to piped water and the type of material the floor is made of. Definitions of the variables and descriptive statistics are given in Table 1.3.

In the restricted collective model, women's non-labor income is also endogenous. The identification strategy is based on the targeting of benefits to mothers in treatment group localities. Eligible mothers in the treatment group localities receive cash payments from the program that are not provided to women in the control group localities during the evaluation period. Thus, belonging to the treatment group results in higher household expenditures and higher women’s income and is uncorrelated, by design, with the error term in the nutrition demand equations (2.1). Belonging to the treatment group qualifies as a valid instrument for the log of wife’s non-labor income and for expenditures. As in the unitary model specification, dwelling characteristics and total household income are used as exclusion restrictions in order to identify the effect of expenditures.

Similarly, in the unrestricted collective model, the targeting of benefits to women in the treatment group, household total income and dwelling characteristics are used as instruments for expenditures and for the wife's non-labor income. Additional variables from a specific module on the spouses' families at time of marriage are used as instruments for spouses' incomes. They include indicators of the families' social status at the time of marriage. Belonging to families of higher social status is likely to result in higher income and is unlikely to be correlated to the error term in the nutrition demand equation (2.2).

In the extended family framework, the first-difference model (2.3) corrects for any unobserved fixed effect (Wooldridge, 2002). Yet, measurement error, a feedback effect from nutrition to household expenditures or the number of income earners, or the omission of any time-varying variable related to these variables could lead to inconsistent estimates. A test of the strict exogeneity assumption is based on the fact that no subset of the explanatory variables, in levels, should enter the first-difference specification. Consider the following:

$$\Delta \ln Cal = \alpha + \beta\, \Delta \ln PCE + \beta' \ln PCE + \gamma\, \Delta NEARNERS + \gamma' \, NEARNERS + \Delta Z\, \theta + \Delta \varepsilon,$$

$$\boldsymbol{H_0} : \beta' = \gamma' = 0.$$

Note that the test is based on the assumption that expenditures enter linearly in the equation. Rejecting the null hypothesis provides evidence of a contemporaneous correlation between the explanatory variable and the error term. If the test rejects the null, one can estimate the first-difference equation (2.3) using 2SLS rather than OLS to correct for the endogeneity. A set of valid exclusion restrictions includes any of the other explanatory variables in levels, provided that there are sufficiently correlated with the endogenous variables.

Using the same Mexican dataset I use in this study, Attanasio and Lechène (2002) test the income pooling restriction in a budget shares system that includes food, alcohol and tobacco, transportation, services, woman's clothing, men's clothing, girl's clothing and boy's clothing. Yet, on average, 97% and 95% of all households respectively report zero expenditure for alcohol and tobacco. The authors do not correct for the mass point at zero in the estimation of the budget shares system. Given the strength of censoring at zero, it is likely that the test of income pooling is based on inconsistent estimates. They exploit the exogenous change in women's income created by the targeting of the program's benefit to mothers within randomly selected treatment localities to instrument both total expenditures and the wife's share of total income as outlined above. Yet, they restrict the sample to household containing no more than two married adults and any number of children. If the PROGRESA treatment affects the number of adult members co-residing in the household, then using the PROGRESA treatment variable as an instrument is inappropriate because it is correlated with the error term in the outcome equation. Along with the random allocation of benefits between treatment group women and control group women, the authors use village-level agricultural wages as an additional instrument in order to identify the system. Since the PROGRESA villages strongly rely on agriculture, high village-level wages are likely to occur in villages who experience a good harvest. People who live in these villages are also likely to have higher food consumption than people living in villages that have experienced low agricultural yields. Thus, if the village-level aggregates captures local conditions that affects nutrition in the village, using them as an identifying restriction would lead to inconsistent estimates. The authors reject the income pooling restriction in the budget shares system. They do not test the Pareto-efficiency assumption.

Rubalcava, Teruel and Thomas (2002) estimate a budget shares system similar to Attanasio and Lechene (2002) using the PROGRESA data. One of the main differences is that they split food budget share into budget shares for four categories of food, i.e. budget share for vegetables, fruits, tortilla and beans, and meat. The other equations in the budget shares are similar to the ones in Attanasio and Lechene (2002). The other main difference is methodological. The authors estimate the effect of the PROGRESA actual transfer amount controlling for total family expenditures using OLS on three separate samples, i.e. all treatment and control households, only eligible households in the treatment and control groups, and only eligible households in treatment group. They do not deal with the endogeneity problems in income or expenditures. Instead, they find that the estimates of the effect of PROGRESA cash transfers are similar in magnitude in the three samples. This finding leads them to interpret the PROGRESA transfer effect as the effect of a change in women's bargaining power rather than the effect of non-linearities in the Engel curve, or the effect of other components of PROGRESA such as the conditionality of the grant or the nutrition education from mandatory health talks. Yet, the actual PROGRESA transfer amounts sent to households are a reflection of household choice to participate and comply with program requirements. Thus, this variable, unlike the treatment dummy from the experiment, is no longer exogenous to consumption shares. Finding close estimates from three different samples does not guarantee that any of the estimates are consistent, especially when endogeneity problems are not dealt with.

1.5.3 Potential caveats

1.5.3.1 The effect of belonging to the treatment group on nutrition

Belonging to the treatment group could impact nutrition decisions through the health talks that the households are required to attend in order to receive benefits, and not just through the income effect. The health talks are held in local clinics by nurses and primary health-care practitioners. Nutrition is discussed among 25 other themes related to health such as hygiene, immunization and family planning. Emphasis is on preventive health care. Nutrition-related lectures include how to detect early malnutrition, how to get safe food, water treatment, and how to treat diarrhea by oral rehydration. In addition, beneficiaries' consumption could be affected by the receipt of nutritional in-kind supplements. These are given to eligible households with young malnourished children, expecting mothers and breastfeeding mothers.

First, I assess the effect of *TREATED* on nutrition in the unitary model by separately testing the hypothesis that *TREATED* is a valid identifying exclusion as in Baum, Schaffer and Stillman (2003). This test allows assessing the validity of the instruments one by one. Thus, it complements the standard test of over-identifying restrictions. In the collective models, after conditioning on individual income, I use the same test to assess the validity of *TREATED* as an instrument. Second, although it is difficult to disentangle the impacts of the different components of the program on nutrition, some information is specific to the potential effect of health lectures on nutrition. As suggested by the themes covered during the health talks, the impact might not be so much in term of the diversity of the diet than in terms of hygiene related diet quality, e.g how to store food to avoid contamination by germs. Furthermore, qualitative evidence from field trips suggests that because of over-crowding in the room where talks are given, the impact of health talks on nutrition is likely to be minor. Third, by splitting the sample into two sub-samples "with young children" and "without young children", I compare the results from the tests of over-identifying restrictions. The idea is that the instrument *TREATED* is more likely to have an effect on family nutrition beyond its effect on the endogenous regressors for the sub-sample "with young children" than on the sub-sample "without young children".

1.5.3.2 Simultaneity in children schooling and the take-up of the program

Since school grant money is only given to women whose children are regularly attending school, the decision to take-up the transfer and decision to invest in children's education are made jointly. For this reason, rather than the take-up of benefits, I use the treatment indicator, i.e. an indicator of "the offer to treat". Non-participation by eligible treatment households in the program could lead to a weak correlation in the first stage equation between the endogenous variables and the instrument. Any other heterogeneity in treatment could only affect the strength of the first stage correlation. Belonging to the treatment group is an indicator of the offer to treat, which is an exogenous variable with respect to household choice. Whether households decide to send children to school or not could only affect the findings because of a weak correlation in the first stage equation between the offer to treat dummy and the endogenous variables, which can be checked using a standard t-test. The issue of simultaneity between the children schooling decision and the take-up of the program is more problematic in studies that include education spending as a separate equation in the system, such as Attanasio and Lechene (2002) and Rubalcava, Teruel and Thomas (2002).

1.5.3.3 Other considerations

Another potential instrument for expenditures and the distribution of income is the amount households are entitled to receive. The advantage of this instrument is that it has more variability than the dummy variable identifying households in the treatment group. However, a problem with using this variable is that the grant awards that constitute the larger part of the transfer vary with the gender and degree of advancement of children. Thus, the amount households are entitled to receive varies with household composition and is therefore collinear with other variables one would like to include in the right-hand side of the calories equation. Family composition presumably affects nutrition in a direct way through economies of scale. Omitting the family composition variables would lead to a correlation between the benefit amount levels and the error terms in model (2.1) and model (2.2). This would lead to biased estimates. Thus, instead of using the PROGRESA cash transfer amount as an instrument, I use the treatment dummy from the experiment.

1.6 Results and discussion

This section is organized as follows. First, I discuss the estimation results for the calories equations derived from the different models of household behavior in the setting with two decision-makers. I discuss the validity of the instruments used in the two-stage least squares estimation. Second, I examine the results in the extended family context. Third, I test the unitary model restriction and the Pareto-efficiency assumption for the nutrition decisions. Fourth, I discuss the substantive effect of changing the wife's income on the levels of calories consumed.

1.6.1 Estimation results in a setting with two decision-makers

1.6.1.1 The unitary model

According to the unitary model, income is pooled in the household. Thus, the unitary model acts as a benchmark.

In the unitary model, the expenditure elasticity of total calories is found to be in the range 0.27-0.32. The expenditure elasticity is higher for calories from meat and meat products and calories from vegetables and fruits. These estimates are obtained by treating the value of consumption as endogenous. As mentioned in section 5, the main sources of endogeneity are (i) common measurement error, (ii) spurious correlation arising from a systematic error in accounting for the number of individuals sharing the meals, and (iii) a feedback effect from nutrition to the permanent income measure. The common measurement error is the most likely endogeneity problem to occur because the same data are used to generate the dependent variables and the expenditures aggregate. The spurious correlation is corrected for using a strategy similar to Subramanian and Deaton (1996). Instead of dividing the value of consumption by household size, I divide it by the actual number of individuals who regularly eat in the household and obtain a *per mouth* value of consumption. In order to correct for endogeneity from common measurement error, I use per capita household income as an instrument. It is highly correlated with the value of consumption and uncorrelated with the measurement error. Because endogeneity can also arise from a feedback effect, I use dwelling characteristics as instruments. They are also correlated with the value of consumption but unlikely to be correlated with the unobserved determinants of nutrition.

Thus, I present 2SLS estimates of the unitary model for three specifications that differ in terms of the instruments used and in the source of endogeneity that is corrected for. In Table 1.6, I only use as an instrument per capita household income. In Table 1.7, I only use the dwelling characteristics as instruments. In Table 1.8, I use both sets of instruments and test the over-identifying restrictions. I cannot reject the null hypothesis that the instruments are orthogonal to the error term. In Table 1.9, I present the first stage regressions for the three different specifications. In all three cases, the test of joint significance of the instruments in the first stage produces an F-statistic above 10.[23]

If the unitary model holds, then the treatment indicator *TREATED* should be a valid exclusion restriction in the calorie equations. The test by Baum, Schaffer and Stillman (2003) allows testing this hypothesis. Interestingly enough, I find that, in general, the treatment variable is not a valid exclusion restriction in the unitary model. This is the case in all of the demand equations, except for meat and meat products. The results are consistent across the three specifications and for all equations except for calories from meat and meat products (Table 1.10). It questions the unitary household model since it suggests that PROGRESA benefits are likely to have an impact on nutrition beyond the income effect. Since benefits are targeted to women, these results could be consistent with a collective model of household behavior in which receiving PROGRESA benefits alters the balance of power within the household in favor of the women.

1.6.1.2 The collective models

Table 1.11 presents the 2SLS results from the estimation of the restricted collective model. Table 1.12 contains results for the unrestricted collective model.

In the restricted collective model, the estimated expenditure elasticity of total calories is 0.28. In the unrestricted collective model, this value is 0.47. As previously, the expenditure elasticity is higher for calories from meat and meat products and calories from vegetables and fruits. In general, calorie consumption increases as the wife's non-labor income increases, with the exception of the calories consumed from meat and meat products for which the effect is negative although insignificant. The effects of changes in the wife's non-labor income on calorie consumption are similar in the restricted and unrestricted collective models (Table 1.11 at line 2 and Table 1.12 at line 3). In addition, in the unrestricted collective model, husband's earnings are found to have a positive but insignificant effect on the consumption of vegetables and fruits and a negative and significant effect on the consumption of meat and meat products. In the other equations, the husband's earnings elasticity is found to be small (less than 0.01) and insignificant.

[23] According to Staiger and Stock (1997), an F-statistic below 10 indicates weak instruments.

Per mouth calorie consumption of meat and meat products and vegetables and fruits increases with household size conditional on household composition. Consumption of other food decreases with household size conditional on household composition. In addition, total calories consumption is negatively associated with household size when controlling for household composition. Family composition, captured by the number of household members in different age and gender groups, significantly affects nutrition. Nutrition also depends on the spouses' years of schooling. Taken jointly, availability of health care in the village and access to electricity and sewage have a significant effect on the calorie demand equations. The coefficients of prices of food items are also jointly significant.

In the restricted collective model, as in the unitary model, expenditures are treated as endogenous and instrumented using per capita income and dwelling characteristics. In addition, because wife's non-labor income is treated as endogenous, I use the treatment dummy as an additional exclusion restriction. The last two lines of Table 1.11 provide information on the validity of the instruments. Taken jointly, all instruments are found to be valid exclusion restrictions for all equations using a test of over-identifying restrictions. Moreover, although in the unitary model, the treatment dummy cannot be used as an exclusion restriction, I cannot reject the null hypothesis that this variable is a valid instrument in the restricted collective model, except for the meat consumption. The first stages are presented in Table 1.13. Note that the strong correlation between the instruments and the wife's non-labor income is mainly due to the treatment dummy variable.

I compare tests of the exogeneity of the treatment dummy for households with young children (age 4 and below) and households without young children. In both cases, I cannot reject exogeneity of the treatment dummy using the Baum, Schaffer and Stillman (2003) test for one exclusion restriction. This suggests that the effect of belonging to the treatment group on household nutrition is mainly an income effect, even for families with young children who are eligible for an in-kind nutritional supplement (Table 1.14).

In the unrestricted collective model, expenditures, wife's non-labor income and husband's earnings are treated as endogenous. As previously, I use per capita income, dwelling characteristics and the treatment dummy as instruments. In addition, I use spouses' characteristics at time of marriage as additional exclusion restrictions. The last two lines of Table 1.12 provide information on the validity of the instruments. Taken jointly, all instruments are found to be valid exclusion restrictions for all equations using a test of over-identifying restrictions. As in the restricted collective model, I cannot reject the null hypothesis that the treatment dummy variable is a valid instrument in the unrestricted collective model, except for the meat consumption. Finally, the first stages are presented in Table 1.15. Again, the strong correlation between the instruments and the wife's non-labor income is mainly due to the treatment dummy variable.

Three conclusions can be drawn from the previous results. First, reviewing the test results for the instruments used in the estimation of the unitary model and the collective models suggests that the instruments are valid ones. Thus, the estimated effects of expenditures and individual incomes are consistent. Second, the strong correlation between the individual incomes and the instruments used for the estimation of the collective models allows the effects of the distribution factors on calorie consumption to be precisely estimated. Third, holding expenditures constant, increasing wife's non-labor income has a positive effect on total calorie consumption and increasing husband's earnings has a negligible effect on total calorie consumption but a differential effect on calories from different groups of food.

1.6.2 Estimation results in the extended family setting

In the extended family setting, I estimate a first-difference model for equation (2.3) and equation (2.4). In the underlying model, the dependent variable is the per mouth amount of calories consumed in the household for both equations. The explanatory variables common to both equations include the per capita value of consumption, household size and household composition and other time-constant household characteristics, such as husband’s and wife’s education. The equation (2.3) includes the number of individuals in the household earning any income. In equation (2.4), I include as explanatory variables the number of male household members who started earning income, the number of male household members who stopped earning income, the number of female household members who started earning income, and the number of female household members who stopped earning income between two successive periods. The first-difference transformation corrects for any fixed unobserved effect. All time-invariant explanatory variables are differenced away in the context of the first-difference model.

In order to test the strict exogeneity assumption, I include in the first-difference OLS regression the potentially endogenous variables in levels. The first column of Table 1.16 shows that, in equation (2.3), the number of income earners has a significant effect on the change of calories consumed, indicating an endogeneity problem. Yet, the value of consumption is insignificant. The second column of Table 1.16 shows that, in equation (2.4), the number of male income earners and the number of female income earners have a significant effect on the change in calories consumed. Thus, I estimate the first-difference model with 2SLS using exclusion restrictions to instrument for the change in the number of income earners in equation (2.3), and to instrument for the number of males and females who started or stopped earning income between two successive periods in equation (2.4).

As mentioned above, any of the explanatory variables in levels can act as valid exclusion restrictions, provided that they are sufficiently correlated to the endogenous variable. Among these, the number of 15 to 19 year olds is found to be sufficiently correlated with the change in the number of income earners. This is consistent with teenagers being the additional income earners in the family. I use the value of per capita household income as additional exclusion restriction in equation (2.3). As expected, this variable is strongly correlated with the change in the number of income earners (Table 1.17). I test whether this additional variable is a valid instrument using the test of orthogonality for one exclusion restriction. I cannot reject the exogeneity of this particular variable. Using a standard test of over-identification, I cannot reject the exogeneity of all the instruments taken jointly (Table 1.18).

In equation (2.4), I instrument for four endogenous variables, i.e. the number of male household members who started earning income, the number of male household members who stopped earning income, the number of female household members who started earning income, and the number of female household members who stopped earning income between two successive periods. I use the same identifying restriction as in the equation (2.3). In addition, I use the treatment dummy, the number of 20-34 years old female household members and the number of 20-34 years old male household members. These young adults constitute a pool of income earners in the family. The use of the treatment dummy as an exclusion restriction increases the precision of the 2SLS estimates. Table 1.17 shows that the instruments are strongly correlated with the endogenous variables. Using a test of exogeneity for a single instrument, I cannot reject the exogeneity of the treatment dummy. In addition, I cannot reject the exogeneity of all the instruments taken jointly. These test results are reported at the bottom of Table 1.18, in the second column.

The 2SLS estimation results for equation (2.3) are shown in the first column of Table 1.18. I find that a 1 percent change in the per capita value of consumption is associated with a 0.46 percent change in the per mouth amount of calories consumed. An additional income earner is associated with a decrease in calorie consumption on the order of 9 percent.

Yet, the effect of an additional income earner on calorie consumption varies with the gender of the household member. In addition, the effect of a household member who starts earning income is not symmetric to the effect of a household member who stops earning income, as shown in the second column of Table 1.18. When a female household member starts earning income, family calorie consumption increases by 19 percent. When it is a male household member who starts earning income, family calorie consumption decreases by 10 percent. When a female household member stops earning income, family calorie consumption decreases by 15 percent. When it is a male household member who stops earning income, family calorie consumption increases by 17 percent.

1.6.3 Testing the unitary model restriction and the Pareto-efficiency assumption for the nutrition decisions

1.6.3.1 Testing the unitary model in the two decision-makers setting

Table 1.19 provides results from testing the unitary model restriction in the restricted collective model (column 1) and the unrestricted collective model (column 2) for total calories and calories from each food group. The unitary model is rejected in all cases except for the consumption of meat and meat products in both specifications.

1.6.3.2 Testing the unitary model restriction in the extended family setting

In the extended family setting, I find in all specifications a significant effect of the change in the number of income earners on changes in household calorie consumption, holding changes in household expenditures constant. In particular, changes in the number of male income earners have a stronger effect on the level of calorie consumption than changes in the number of female income earners (see Table 1.18). Thus, in the extended family setting, the income pooling restriction does not hold. Controlling for total household expenditures, the number of individuals in the household who are earning some labor income matters for the calorie consumption decision. The unitary model is rejected in the case of the extended family.

1.6.3.3 Testing the Pareto-efficiency assumption

Using the unrestricted collective model with two distribution factors, I reject the Pareto-efficiency assumption for two pairs of goods. These consist of the pair "vegetables and fruits calories / cereals and grains calories" and the pair "cereals and grains calories / other food calories". The results are presented in Table 1.20.

If the Pareto-efficiency assumption is tested jointly over all pairs of goods, the joint hypothesis has a chi-square statistic equal to 7.14 that corresponds to a p-value of 0.094. I reject at the 10% level the Pareto-efficiency assumption on the allocation of resources between spouses with respect to the calorie consumption decisions.

1.6.4 How is nutrition affected by changing the spouse's income?

After providing statistical evidence of an effect of the distribution of power within the household on nutrition, I discuss the substantive importance of this effect.

According to the estimates from the restricted collective model, a 100 percent change in the wife's non-labor income is associated with a one percent change in total calories consumed. The effect is a little higher for calories from vegetables and fruits, calories from cereals and grains and calories from other food, but still in the 1 percent range. Changing the wife's non-labor income has no effect on meat consumption.

Results from the unrestricted collective model are consistent with these findings. In addition, the effect of husband's earnings on vegetable and fruits consumption, meat consumption and consumption of other food is found to be equal to the effect of wife's income. The negative effect of husband's earnings on consumption of cereals and grains more than offsets the positive effect of wife's income. Yet, changes in husband's income have no effect on total calorie consumption.

Overall, I find that the effects of changes in spouses' incomes are small in a substantive sense. This is consistent with recent results in the literature: "The key issue in the context of testing models of decision-making is their [statistical] significance"

(Thomas 2002). Does this finding imply that the unitary model may hold approximately? I discuss this question further below.

First, the tests of income pooling and Pareto-efficiency that are used in this study require the inclusion of distribution factors in the model equation. However, in practice, the number and nature of the distribution factors that determine the distribution of power within the household are not clearly defined. As mentioned in Section 3, although the distribution of income is likely to affect the distribution of power within the household, other factors could also have a role.[24] Omitting these factors might not affect the testing of the unitary model restriction and the Pareto-efficiency assumption. Yet, these other factors might have a greater impact on the distribution of power within the household than spouses' incomes.

Second, most women in the sample have low income. A 100 percent change in income is a small amount in absolute terms. Thus, the support problem may play a role in the low elasticity found in this study.

Third, the unitary model is also rejected in the extended family settings, i.e. for households where individuals other than the head of household or his spouse can be earning some labor income. I find that when a women starts earning some income, family calorie consumption increases. It decreases when a man starts earning income.

1.7 Conclusion

How does the distribution of power within the household affect the nutrition of its members? I explore this question using a unique dataset collected for the evaluation of

[24] For example, current assets and assets brought at time of marriage have been found to influence the distribution of power within the household in developing countries.

the largest social program in rural Mexico. In this sample, poor households in randomly selected control localities did not receive program benefits. I exploit data from this social experiment to (1) test models of household decision-making, (2) provide some insights on how much nutrition is affected by changes in the distribution of income within the household and in the number of income earners in the household.

Focusing on the nutrition decision, I consider two settings. The first relies on the assumption that, even when household members other than the head of household and his spouse earn income, the sole two decision-makers are the head and his spouse. This is a strong assumption, which is relaxed in the extended family setting. The extended family setting allows every household member who earns income to participate in decision-making.

I reject the income pooling restriction underlying the unitary model of the household in a setting with two decision-makers. In addition, I reject the Pareto-efficiency assumption underlying the collective model of the household for the calorie consumption decisions. There is an allocation of resources towards consumption of the various food groups that is Pareto-superior to the one collectively chosen in the household. This result is consistent with the separate sphere bargaining model (Lundberg and Pollak 1993) for which gender roles assign responsibility to each partner for certain decisions. The results are also consistent with the existence of information asymmetries at the household level, or problems in the enforcement of household agreements. Yet, I find that doubling the wife's non-labor income is associated to only minor changes in calorie levels –around one percent.

In the extended family setting, with possibly more than two income earners, I find that, for a given level of household income, changes in the number of income earners lead to changes in per capita calorie consumption. This leads to a rejection of the unitary model income pooling restriction in the case of the extended family. I find that calorie consumption increases when a woman starts earning income and decreases when it is a man who starts earning income.

From a policy standpoint, the rejection of the income pooling restriction suggests that the intrahousehold distribution of power over resources affect household decision-making. When I relax the assumption that the head of household and his spouse are the sole two decision-makers, I find that the implications of the unitary model do not hold for extended families in poor rural regions of Mexico. In addition, in the extended family setting, when a woman starts earning income, family calorie consumption increases by 19 percent. This is in contrast to the findings in the two decision-makers setting, for which changes in food consumption associated with changes in the intrahousehold allocation of income are found to be small. This difference highlights the importance of taking into account the characteristics of households in poor countries in the empirical modelling of household decision-making in order to guide policy.

Chapter 2: Heterogeneous program impacts in PROGRESA

2.1 Introduction

The most commonly used estimators in program evaluation provide information on the program mean impact. Yet, this is only a narrow answer to the question of how well a program works. Exploring heterogeneity in program impacts provide information on the distributional effects of policy interventions in a way that goes beyond mean impacts. It can also help to go inside the "black box" of mean program impacts and learn more about how policies generate their mean effects (see Heckman and Smith 1995 for a discussion of the main criticism towards experimental methods).

The purpose of this paper is to investigate the importance of heterogeneity in program impacts for the Mexican program for education, health and nutrition PROGRESA. Does the program have the same effect on everyone? Will some groups benefit more from the program than others? Most of the existing literature on heterogeneity of treatment effects mainly looks at the heterogeneous effect that vary with observed characteristics, i.e. the heterogeneity on subgroups of the population. We explore the heterogeneity of impacts as a function of the criteria used by PROGRESA to select beneficiaries. Yet, we also investigate the overall heterogeneity of program impacts, which includes both observed and unobserved heterogeneity. Four other studies explore similar aspects of the distribution of impacts in the US context (Heckman, Smith and Clements 1997, Black, Smith, Berger and Noel 2003, Bitler, Gelbach and Hoynes 2004, and Biddle, Boden and Reville 2003). This is the first paper that investigates the heterogeneity in program impacts for a policy intervention in a developing country.

In the next section, we describe the Mexican program for education, health and nutrition PROGRESA. The Mexican social program PROGRESA is a means-tested conditional cash transfer program. It targets the poorest households in the most remote rural regions of Mexico. The PROGRESA program is designed as a cash transfer payment given to the mother in the household upon compliance with a defined set of requirements (e.g. regular child attendance to school and frequent visits to health centers). Thus, the program has both a short-term poverty reduction objective and a longer-term objective in terms of investment in human capital. We describe the data and the experimental design. The latter consists of an experiment with randomly assigned treatment and control localities. Within the treatment localities, eligible households are offered the program benefits. Within the control localities, eligible households do not receive the benefits during the two-year evaluation period. Data collected in November 1998, i.e. six months after the start of the program, as well as data collected in June 1999 and in November 1999 are used in this study. By sending conditional cash transfers to households, the PROGRESA program's main objectives are to reduce household poverty, to increase household food consumption, to encourage investment in human capital. Thus, the outcomes of interest include household total expenditures and value of consumption, household food expenditures and value of food consumption, and the children's time spent in schooling activities, income-generating activities and domestic activities. A summary of the evaluation of PROGRESA can be found in Skoufias (2001).

In the third section, we present the theoretical framework for our investigation. Based on the program benefit scheme and on how well the program was implemented, we discuss the case for heterogeneous program impacts versus the "common effect" model in PROGRESA. Then, we analyze the heterogeneity in program impacts on household welfare, consumption and time allocation that is raised by the conditionality of the cash transfer.

In the fourth section, we explore the heterogeneity of impacts along observable characteristics. We focus on the variation in impacts as a function of the two criteria used by PROGRESA to select beneficiaries, i.e. a village marginality index a household poverty index. Both indices are constructed by the program officials using information collected prior to the intervention (Skoufias *et al.*, 1999). This analysis allows us to assess the effectiveness of the targeting mechanism. Are the poorest households in the most marginal villages getting a greater program impact than less poor households from less marginal places? In order to answer this question, we estimate treatment effects on subgroups, which is the most common way to investigate the distributional impacts of a program. We estimate program impacts along the targeting criteria for all the outcomes of interest.

In the two subsequent sections, we investigate the overall heterogeneity of program impacts, which includes both observed and unobserved heterogeneity. Experimental data are sufficient to identify mean program impacts or impacts on subgroups, but do not identify unobserved heterogeneity in impacts.

In the fifth section, we derive a lower bound for the total variance of impacts using results from classical probability theory. We test whether the lower bound is significantly different from zero. This allows us to test whether the total effect of the program is homogeneous over both observed and unobserved characteristics. We explicitly decompose the total variance in impacts into a part that is systematically related to the targeting criteria and a part that is not. It allows us to examine whether, once systematic variation in impacts is removed, the variance in impacts is still different from zero. Exploring unobserved heterogeneity is a methodological innovation in the paper. Yet, additional assumptions are required to further analyze the distribution of impacts. We assume perfect positive dependence between untreated outcome levels and treated outcome levels. This assumption holds if the ranks of the households are unaffected by the program. It allows us to estimate quantile treatment effects (QTE). The QTE estimation provides information on how the impact varies at different quantiles of the untreated distribution. We explore the QTE distributions of both the total impacts and the unobserved impacts for the selected outcomes.

In the sixth section, moments of the distribution of impacts are identified under the assumption that the untreated outcome levels and program impacts are independent. This assumption means that households do not anticipate gains from the program at the time they decide to participate in the program, which is likely to occur when households are randomly assigned to a treatment and a control group. We test this assumption and we identify the first two moments of the distribution of impacts using a parametric random coefficient model. We also identify the first two moments of the distribution of both the total impacts and the unobserved impacts under a weaker independence assumption. We assume a normal distribution and estimate the distribution of impacts. Alternatively, when the independence assumption holds, we identify the first four moments of the distribution of impacts using a more flexible parametric approach. We approximate the distribution of impacts assuming that it belongs to the Pearson family of distribution.

We find evidence against the "common effect" assumption, which is robust to the assumption adopted. From the analysis on subgroups, we find evidence that there is variation in impacts along the targeting criteria in the treated population, that the geographic targeting is effective but the beneficiary selection on household poverty within poor localities is not. The last two findings are consistent with the findings in Skoufias *et al.* (2001) on the program targeting mechanism.

The bounding analysis provides evidence that the total variance of impacts is different from zero. This result does not rely on any assumption and thus is particularly strong evidence of heterogeneous treatment effects. Moreover, we find that, even once the systematic variation in impacts is removed, the unobserved part of the variance of impacts is different from zero. This is further evidence of heterogeneity in program impacts, which is not due to systematic variation along observable characteristics. We also find from the bounding analysis that the experimental data are consistent with a large range of impact distributions. From the QTE estimation, we learn that the program impact on wealth and nutrition is lower for households who were at a lower level of wealth and nutrition prior to the intervention in the last two rounds. This is consistent with the theoretical prediction that treatment effect on wealth and nutrition are higher for the households whose cost of complying with the program requirements is the lower. The impacts are positive at each decile of the untreated distribution of outcomes. In the first round, the program impact does not vary with the untreated outcome distribution. We also find that the unobserved impacts distribution follow a similar pattern along the untreated outcome distribution.

Under the assumption of independence between gains from the program and the untreated outcome levels and when impacts follow a normal distribution, we find that the fraction of the treated population with a positive impact ranges is higher for the latest rounds. This finding suggest that in the first round, some households may have supported the cost of the program requirements without receiving the program benefits because of implementation problems. Yet, for most of the outcomes of interest, the assumption of independence between program impacts and untreated outcome levels fails to hold. This is consistent with a selective compliance to program requirements in the eligible population.

2.2 The PROGRESA program and data

2.2.1 Description of the program

The PROGRESA program targets poor rural households in Mexico. It has been implemented since 1998. At the end of 1999, it covered 2.6 million families, i.e., about 40% of all rural households and one ninth of all families in Mexico. In 1999, the annual program budget was approximately $777 million, which corresponds to 0.2% of Mexico's GDP (Skoufias, 2001). In January 2002, the Inter-American Development Bank approved its largest loan ever to Mexico for expanding PROGRESA to urban areas of the country. Despite the important recent political changes, PROGRESA remains in place, under the new name Opportunidad.

The actual targeting of PROGRESA involves two stages: (1) the selection of the localities where PROGRESA operates, (2) the selection of beneficiary households within the selected localities. The most remote localities with a minimum of infrastructure (e.g.

at least one primary school and a health center) are selected. Within each selected locality, the selection of households is based on pre-program survey information on household wealth that includes income and relevant household characteristics. A household is eligible to the program if it falls below a poverty line defined by income and other relevant socio-economic attributes. On average, 78% of each locality population is found to be poor, i.e. eligible for the program benefits. The program benefits are conditional income transfers and they are comprised of two components:

3. Educational grants given to families with children in the last three years of primary school and secondary school children. The grant amounts vary by grade and gender in favor of girls and of the most advanced children and reflect opportunity costs. The grants are given upon attendance to school.[25] A complex system of verification based on forms completed and signed by teachers and school directors ensures that the attendance requirement is met before sending money to the households.

4. All selected households can benefit from a monetary transfer designed to help them improve their nutrition. This component is commonly called the food cash transfer. But, although households are encouraged to spend the money on food, they are not required to do so. In order to receive this cash transfer, they are required to make regular visits to health centers and to participate to health talks. Only one visit per year to a health center is required for adults, two to five visits a year for pregnant and breast-feeding women and two to seven visits a year for infants and children. In addition, in-kind nutritional supplements are provided to under-nourished children and infants and to pregnant and breast-feeding women.

The average transfer from October 1998 to November 1999 is about 197 pesos per household per month[26], which is equivalent to 20% of the mean value of consumption of a poor household. An additional requirement of PROGRESA is that households withdraw from other assistance programs that share similar objectives with PROGRESA, such as Ninos de Solidaridad, DICONSA, LICONSA and INI. [27]

2.2.2 The experimental design

The evaluation sample is designed as an experiment with randomization of localities into treatment and control groups. The randomization is at the village level rather than at the household level in order to avoid contamination bias. Only eligible households in treatment localities actually receive any transfer. Eligible households in control localities are denied these transfers until the end of the evaluation period in 2000.[28] The random assignment of localities in treatment and control groups was conducted in order to evaluate the impacts of the program on a range of outcomes.[29]

Behrman and Todd 1999 evaluate the randomness of the sample and find that the treatment and control groups mean outcomes at the locality level are similar before the intervention. However, they find small differences at the household and individual level.

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[25] Children are required to attend school with an 85% monthly attendance rate. If a child fails to meet this requirement, families stop receiving the educational grant for that child, but remain eligible for other program benefits.

[26] The figures are in November 1998 pesos and the value is approximately $20 U.S.

[27] Ninos de Solidaridad provide educational grants. DICONSA maintains subsidized prices for basic food items. LICONSA provide poor families with one free kilogram of tortillas and subsidize the price of milk. INI is targeted to indigenous people and provide lodging and food or educational grants to students.

[28] Extending the PROGRESA program to all the eligible population had to be done by phases because of the size of the program. For a random subset of the villages, the incorporation to the program was postponed for two years. Thus, this subset of the population acts as a control group.

[29] These impacts are assessed using estimators commonly used in program evaluation (e.g. the difference-in-difference estimator). Detailed data description and mean program impacts are presented in a series of research reports (see http://www.ifpri.org/themes/progresa.htm).

Note also that the existing alternative assistance programs do not operate anymore in all of the PROGRESA localities, i.e. the treatment and control localities. Thus, the difference between treatment localities average outcomes and control localities average outcomes gives an estimate of the mean program impact.

All households in the selected localities are interviewed before and at several points in time after the start of the program. The evaluation dataset consists of repeated observations (panel data) for 24,000 households from 506 localities (320 assigned to the treatment group and the remaining 186 to the control group) over five rounds of survey (baseline: October 1997 and March 1998; follow-ups: November 1998, June 1999 and November 1999).

In this study, I use data from the three follow-up rounds, i.e. November 1998, June 1999 and November 1999, because no reliable consumption data is collected before November 1998. These three rounds are used as cross-sections, i.e. I use data from all households for each of the rounds. I restrict the sample to the eligible households. I supplement the datasets with information collected before the implementation of the program in October 1997. I also supplement them with data from a specific time allocation module collected only once in November 1999. A description of the data used in this study is available in the data appendix (Appendix 3).

2.2.3 The outcomes of interest

The outcomes of interest in the empirical section include the wealth of the household measured by household total expenditures and by the value of consumption of all goods and services (Deaton, 1997). Improving household nutrition is a key objective of the program. Household food expenditures and the value of food consumption provide quantitative measures of nutrition. In addition, we examine the time children spent in schooling activities, income-generating activities and domestic activities using a detailed module on time allocation. We denote income-generating activities as all of the activities involving work outside the house, including wage labor (for an employer, on one's own firm/farm with salary or other paid casual work) and non-wage labor (as an aide, on one's own firm/farm and other non-paid casual work). Domestic activities include all activities that take place at home and could have been realized by someone else the household would have hired, such as cleaning the house, washing, sewing and ironing clothes, shopping for the household, preparing meals and washing dishes, fetching water or wood, disposing garbage, taking care of animals and fields, looking after children including taking them to school, or looking after elderly or sick people. A third category of activity consists of attendance at school and time spent studying outside the classroom.[30] In addition to data on the individual and household outcomes, individual and household characteristics are collected as well.

2.3 A theoretical discussion

In a paper on the welfare reform in the US, Bitler, Gelbach and Hoynes (2004) find evidence of program impact heterogeneity which is consistent with the theoretical predictions of a standard labor supply model. In this section, we first discuss the case for heterogeneous program impacts versus the "common effect" model in PROGRESA based on the program benefit scheme and on how well the program was implemented. Then, we

[30] Finally, leisure time is a residual, i.e. the remaining time in a day after subtracting time spent for income generating activities, domestic activities and school. Observations for which leisure time was less or equal to 8 hours were deleted from the sample.

analyze the heterogeneity in program impacts emerging from the conditionality of the cash transfer.

Households with different preferences, budget sets and production sets are likely to respond differently to a homogeneous treatment. Yet, the design of the program can address this issue by varying the treatment. For example, in the case of Mexico, one expects parents to be less likely to send daughters to school than sons. The PROGRESA program addresses the problem by providing a larger grant amount for girls sent to school. In addition, one expects that the older the child is, the higher his or her opportunity cost of time in the labor market. Again, the program anticipates this source of heterogeneity and provides larger grants for secondary school children. The opportunity cost of time of children is also likely to be higher for households who hold or operate land. Girls who live in large households with many younger children are also more likely to be employed at home. Mothers whose opportunity cost of time in domestic or labor activities is higher are likely to bear a high cost from participating in the program, which would lower the overall impact of the program on their households.

Yet, the program does not control for all potential sources of heterogeneity by varying the treatment. In addition, it is unclear whether the program payment schedule could exactly balance out differences in costs. Thus, we can expect to find heterogeneity in program impacts although the program treatment already varies from a household to another. Finally, although the program started in Spring 1998, 27% of the eligible households in the treatment group did not receive any cash benefits by March 2000 according to administrative data. This is mostly due to administrative problems in the implementation of the program. If these households bore the cost of participating in the program without receiving the program benefits, they are likely to have experienced a loss in welfare and a decrease in consumption. Households who sent children to school and complied with program requirements have experienced delays in the receipt of the cash transfer due to delays in the verification of the requirements or in the delivery of the monetary transfers. Coady and Djebbari (1999) assess the early stages of the implementation of the program.

In the remaining of the section, we discuss the effect of the conditionality of the educational grant on household behavior and welfare. This analysis complements the study by Skoufias and Parker (2001). Skoufias and Parker use a standard labor supply model and find that the effect of the conditionality on children's time allocation depends on households preferences and initial wealth level.

In a simplified version of the standard economic model of time allocation, a household composed of one adult and one child optimally chooses a composite good z, the child's schooling time t_s, and the adult's and child's leisure time l_a, l_c. The indirect utility function $V(.,.)$ is as follows:

$$V(p,Y) = \underset{X}{Max}\ \{U(X) \quad s.t. \sum pX = Y = I + (w_a + w_c)\, T\}\text{ , where } X\ =\ (z,\ t_s,\ l_c,\ l_a).$$

The vector of prices associated with X is denoted p, household full income Y is composed to household non-labor income I and household value of the common time endowment T at prices w_a and w_c. Suppose that in a first stage, household optimal choices are limited to $(z,\ l_c,\ l_a)$.

Like the indirect utility function, the partial indirect utility function is increasing in Y. For a given Y, the partial indirect utility function is first increasing, and then decreasing in t_s: it admits a maximum in t_s. We denote by t_s^* the optimal schooling time:

$$t_s^* = \underset{t_s}{ArgMax}\, V(p, t_s, Y) = \underset{z, t_s, l_c, l_a}{ArgMax}\, \{U(X) \quad s.t. \sum pX = Y\}.$$

Program participants must be at least as well-off taking up the program than not taking it up. Yet, the increase in participant welfare can reflect an increase in consumption or an increase in leisure time. Because households have to send children to school a minimum of $\bar{t}$ hours, the welfare effects of a pure income transfers vs. tied income transfers are analyzed using unconstrained and constrained optimization problems:

$$V^U(p, Y) = \underset{X}{Max}\, \{U(X) \quad s.t. \sum pX = Y\} = \underset{t_s}{Max}\, V^U(p, t_s, Y),$$
$$V^C(p, Y) = \underset{X}{Max}\, \{U(X) \quad s.t. \sum pX = Y and\ t_s \geq \bar{t}\} = \underset{t_s}{Max}\, V^C(p, t_s, Y).$$

One can distinguish between four types of households based on the constrained and unconstrained partial indirect utility functions. Figure 1 shows how these two functions vary with t_s. Denote by A the optimal schooling time without transfer, B the optimal schooling time with a pure unconditional transfer and C the optimal schooling time with a conditional transfer. Let g be the transfer amount.

1. Type I households are better off accepting a tied income transfer than refusing it because point C is above point A. Without any aid (point A), these households were already sending their children to school at least the minimum of $\bar{t}$ hours. The effect of the program on the child's schooling time is thus minor. Furthermore, the optimal schooling time is the same whether or not the transfer is conditional (B = C):

$$V^C(p, Y) = V^U(p, Y) < V^C(p, Y + g) = V^U(p, Y + g)\,,$$
$$t^{U*}(p, Y) = t^{C*}(p, Y) > \bar{t}\,,$$
$$t^{U*}(p, Y + g) = t^{C*}(p, Y + g) > \bar{t}\,.$$

For Type I households, the main effect of the program consists of the pure income effect of the cash transfer. We can expect type I households to have higher expenditures after taking-up the program.

2. Type II households also are better off accepting a tied income transfer than refusing it (point C is above point A). Before receiving the aid, these households choose to send children to school less than the minimum of $\bar{t}$ hours (point A). When they get the educational grants, the children attend school more than the minimum required attendance level. Yet, whether they receive a conditional or an unconditional transfer does not affect the optimal schooling time (B = C).

$$
\begin{aligned}
&V^C(p,Y) < V^U(p,Y) < V^C(p,Y+g) = V^U(p,Y+g), \\
&t^{U*}(p,Y) < t^{C*}(p,Y) = \bar{t}, \\
&t^{U*}(p,Y+g) = t^{C*}(p,Y+g) > \bar{t}.
\end{aligned}
$$

Like households of type I, type II households only benefit from the pure income effect of the cash transfer. With the additional PROGRESA income, children's time spent in school is increased above the minimal attendance requirement level. We can also expect higher expenditures for these households.

3. Type III households are similar to type II households in that (1) they are better off accepting a tied income transfer than refusing it, (2) before receiving the aid, children were sent to school less than the minimum of $\bar{t}$ hours. However, the optimal schooling time these households would have chosen for their children if given an unconditional transfer would have been smaller than the optimal schooling chosen with a tied income transfer. Point C is to the right of point B. For type III households, the conditionality of the grant matters. Type III households are affected both by the

pure income effect of the cash transfer and the effect of a lower price of schooling driven by the attendance requirement. This price effect has the standard substitution and income effect, which reinforces the pure income effect of the transfer.

$$V^C(p,Y) < V^U(p,Y) < V^C(p,Y+g) < V^U(p,Y+g),$$
$$t^{U*}(p,Y) < t^{C*}(p,Y) = \bar{t},$$
$$t^{U*}(p,Y+g) < t^{C*}(p,Y+g) = \bar{t}.$$

With the PROGRESA conditional transfer, families can exactly meet the minimum attendance requirement. Children spend more time studying than without a transfer. Yet, at income level $(Y+g)$, type III households may have to bear the cost of children's foregone labor earnings due to the implicit reduction in labor time $\bar{t} - t^{U*}(p,Y+g)$ that results from the conditionality of the grant. Thus, the grant impact on household expenditures may be negative for these households. We expect the food cash transfer benefit to have a positive effect on household expenditures. The net effect on expenditures could be either positive or negative.

4. The last type of household (type IV) looks like the type II and III households in that they were sending children to school less than the minimum numbers of hours targeted by the program prior to the intervention. In addition, like type III households, the conditionality of the grant affects their potential welfare. However, it also affects the outcome of their choice. When compliance to program requirements is demanded, in their choice between (i) complying and receiving the grant, (ii) not complying and not receiving a grant, these households choose the latter. They are better off not taking the conditional grant than taking it. Point A is above point C. These households are likely to be the poorest or at least the ones who rely the most on child

labor. The costs of complying with the program requirements include the foregone earnings of their children. With an unconditional grant, although their demand for schooling would not have attained the minimum of $\bar{t}$ hours, it would still have been greater than it actually is when facing the trade-off implicit in the conditional grant. Point B is at the right of point A.

$$
\begin{aligned}
&V^{C}(p,Y) < V^{C}(p,Y+g) < V^{U}(p,Y) < V^{U}(p,Y+g),\\
&t^{U*}(p,Y) < t^{C*}(p,Y) = \bar{t},\\
&t^{U*}(p,Y+g) < t^{C*}(p,Y+g) = \bar{t}.
\end{aligned}
$$

Type IV households are not getting the educational grants. The only effect of the program on children's time allocation and household expenditures is through the food cash transfer component.

Two points can be made from this analysis. First, although the treatment is heterogeneous, we cannot rule out the case of heterogeneity in program impacts. Problems in program implementation are likely to be at the origin of lower or even temporary negative program impacts for some participants. Second, identifying the different types of households allows us to form predictions concerning the heterogeneity of program impacts. In particular, we find that impacts on expenditures are greater for households who are meeting or almost meeting program requirements prior to the intervention. We expect program impacts on child's schooling for these households to be small or zero. These are likely to be the richest households among the eligible ones or households with young children. We expect to find very small program impacts on expenditures for households whose costs of participating are the highest. These impacts could even be negative for households who meet program requirements but would have still rely on child labor under an unconditional scheme. Yet, program impacts on child's schooling would be the largest for these households. We also expect small or zero program impacts on child's schooling for households who cannot meet program requirements. These are likely to be the poorest households who greatly rely on child labor.

Finally, note that we restricted the previous analysis to households with a single child. In Mexican households with more than a child, young children and boys are more likely to be sent to school than older children and girls. Thus, program impacts on schooling (labor) of older children or girls could even be negative (positive).

2.4 Heterogeneity of impacts and the targeting of the program

2.4.1 Empirical model

If the goal of the targeting is to increase the efficiency of the program then the treatment should be allocated to those for which the impact is the largest.[31] We explore the heterogeneity of impacts as a function of the two criteria used by PROGRESA to select beneficiaries, i.e. a village marginality index and a household poverty index. Both indices are constructed by the program officials using information collected prior to the intervention (Skoufias *et al.*, 1999). Both indices are included in the datasets.

The village marginality index is constructed using village-level information on the illiteracy rate of heads of households, on access to basic infrastructure (running water, a drainage system, electricity), on housing characteristics (ratio of household members to rooms in the house, frequency of houses where floor are made of dirt) and the importance

[31] See section 5.7 of Berger, Black and Smith (2000) on optimal targeting of unemployment insurance in the US.

of agricultural activities in the village. The higher the value of the marginality index, the more remote is the village. The household poverty index takes into account household characteristics, family assets and per capita income. The higher the poverty score is, the poorer the household is. In each eligible village, households are classified as poor and non-poor based on this score.

We allow the treatment effect to vary with the village marginality index and the household poverty index.[32] If the targeting mechanism is effective, then the poorest households in the most marginal villages get a greater program impact than less poor households from less marginal places.

In order to test this hypothesis, we estimate program impacts on household total expenditures and value of consumption, household food expenditures and value of food consumption, and the children's time spent in schooling activities, income-generating activities and domestic activities (all outcomes are designed by *Y* in the equations below).

The treatment group households (*T = 1*) are compared to the control group households (*T= 0*). We include interaction terms between the poverty index (*Pindex*) and the village marginality index (*Vindex*) and the treatment indicator (*T*).

We control for household or individual characteristics in order to obtain more precise estimates. In addition, this should correct for any differences not accounted for by the randomization of localities into treatment and control groups. When estimating program impacts on nutrition and wealth, the control variables include household composition and characteristics of the head of household. For impacts on children's time allocation, the control variables include the child's age, parent's education, the age of the

[32] A preliminary semi-parametric analysis suggested that a linear function is a good approximation of the relationship between the impacts and the poverty and village indices.

mother and the father, whether the head of household is a female or a male, whether the head of household speaks an indigenous language and variables measuring the demographic composition of the household. Control variables are designed by *X*.

We estimate the following equation.

$$
\begin{aligned}
(1) \qquad & Y = \alpha + \alpha_1 * Pindex + \alpha_2 * Vindex + \alpha_3 * Pindex * Vindex + \\
& \quad \beta * T + \beta_1 * T * Pindex + \beta_2 * T * Vindex + \beta_3 * T * Pindex * Vindex + X\delta + \varepsilon.
\end{aligned}
$$

First, we test whether there is any program impact on the outcome by evaluating the following joint hypothesis:

$$H_0 : \beta = \beta_1 = \beta_2 = \beta_3 = 0.$$

Then, we test whether the program impact along the poverty and the village marginality criteria is the same for all households by testing the following hypothesis:

$$H_0 : \beta_1 = \beta_2 = \beta_3 = 0.$$

Rejecting this null hypothesis is evidence of heterogeneous program impacts along the program targeting criteria. In order to test whether the impacts are decreasing or increasing along the household poverty and village marginality indices, we examine the sign of the coefficients on the interaction terms in equation (1). We expect the sign of the coefficient on the interaction terms to be positive for the wealth, nutrition and education outcomes, and negative for children labor and domestic activities. We also estimate the fraction of the treated population with a positive program impact for each of the outcomes.

2.4.2 Results and discussion

Table 2.1 shows the estimation results for equation (1) for the wealth and nutrition variables.[33] We reject the null hypothesis that the treatment does not vary with the targeting criteria for all outcomes and rounds. The rejection is stronger for the interaction with the village marginality index and weaker for the interaction with the poverty index. We find that the signs of the coefficients on the interaction terms are positive. This is evidence that the program impacts on wealth and nutrition are greater for those who initially live in the most remote villages. The coefficient on the interaction term between treatment and village marginality index decreases with time. The same holds for the coefficient on the interaction term between the treatment indicator and the two indices, which decreases with time. This means that the overall difference in program impacts between households who initially live in the most remote areas and those who initially live in the less marginal areas is getting smaller over time. Table 2.2 shows that the program has a significant impact on the wealth and nutrition aggregates in all rounds and that the fraction with a positive program impact on wealth and nutrition is also increasing over time. In the second and third round, the program impact on wealth and nutrition is positive for more than 90 percent of the treated group.

Results from the impact of the program on time allocation outcomes for boys of primary school-age and secondary school-age are presented in Table 2.3. We do not find any program impact on schooling for primary school-age boys. The overall program impact on time spent in domestic chores is insignificant as well. Yet, the fraction of eligible young boys with a positive program impact on time spent in domestic activities is

[33] All tables of results for chapter 2 are in Appendix 2.

unexpectedly high. We find evidence of a program impact on their labor activities. This impact varies with the program targeting criteria. The probability of participating in the labor market and time spent working is the lowest for the children who live in the most remote villages. Much of the variation in program impacts comes from the interaction with the village marginality index. Program impacts do not vary much with the initial poverty level of households.

For secondary school-age boys, there is a significant program impact on both schooling and labor activities. The program does not have an impact on their domestic activities. We find evidence that the program impact on participation in school and time spent studying is smaller for children who live in the most remote villages. Furthermore, only 63 percent of eligible boys experience a positive impact on schooling time. This is not the expected finding. It is plausible that for the most remote villages, the decision to invest in education beyond primary school is less attractive than in less remote areas. We also find that participation in labor activities decreases more rapidly in the most remote areas than in the least remote areas for eligible households. Yet, the program impact on the time spent working does not vary with the targeting criteria. Nor do program impacts on any of the outcomes vary with the initial poverty level of households.

Table 2.4 shows the program impacts on girls' time allocation outcomes. For primary school-age girls, the program impacts participation in domestic activities and time spent in domestic chores. But, the overall impact of the program on either schooling or labor is insignificant. We find evidence of variation of program impacts along the targeting criteria for the domestic activities of primary school-age girls. As expected, the program impacts along the targeting criteria are negative for this outcome. This means that young girls from the most marginal areas are benefiting from a larger reduction in domestic activities because of the program than young girls from less marginal areas. Yet, the fraction of eligible young girls with a positive impact of the program on their domestic activities is unexpectedly high. Note also that program impacts do not vary with the initial poverty level of households.

Finally, PROGRESA has a significant impact on schooling and domestic activities for secondary school-age girls. Program impacts on schooling vary with the targeting criteria, but program impacts on domestic chores are independent of the initial poverty and marginality indices. We find that the coefficient on the interaction between treatment and the poverty and marginality indices is negative for the participation in school equation of secondary school-age girls. The sign is also negative for the time spent studying. This is not the expected finding. This means that the program impact is smaller for poorest girls from the most remote villages.

Overall, we find evidence that the program impacts vary with the program targeting criteria for most outcomes. In particular, program impacts depend on the initial place of living. We find that program impacts do not vary much with the initial poverty level of households. In an analysis of the PROGRESA program targeting, Skoufias *et al.* (2001) compare the current targeting mechanism to alternative selection models. They also find that program impacts could have been achieved using the geographic targeting alone. [34] As expected, we find that households from the most marginal villages get a greater program impact on wealth and nutrition than households from less marginal places. Similarly, children from the most remote villages are found to have a larger reduction on work and domestic activities from the program. However, we also find that the program does not affect the schooling of primary school-age children. In addition, both secondary school-age boys and girls have a smaller program impact on their schooling time when they live in the most remote villages. Low impacts on schooling in the primary grades are consistent with the program spending money to "buy the base" (see Todd and Wolpin, 2003, for a similar finding). On average, secondary school-age children from the most remote areas may be getting a lower program impact because many of them chose not to attend school and thus do not get the PROGRESA grant.

2.5 Heterogeneity of impacts and perfect positive dependence.

The existing literature on heterogeneity of treatment effects mainly looks at the heterogeneous effect that vary with observed characteristics, i.e. the heterogeneity on subgroups of the population. In the case of PROGRESA, other papers found evidence of differential impacts on child's schooling and labor for primary school-age children vs. secondary school-age children, for girls vs. boys, for drop-outs children vs. children continuing through school (see Skoufias 2001 for a synthesis of the results).

In this section, we investigate the overall heterogeneity of program impacts, which includes both observed and unobserved heterogeneity. First, we quantify the total variance of impacts and test whether the total effect of the program is homogeneous over both observed and unobserved characteristics. Experimental data are sufficient to identify mean program impacts or impacts on subgroups, but do not identify unobserved heterogeneity in impacts. Second, we assume perfect positive dependence between untreated outcome levels and treated outcome levels in order to explore other distributional aspects of program impacts. Perfect positive dependence occurs when the

[34] They also discuss the social cost raised by targeting within poor villages.

ranks of the households are unaffected by the program. This assumption means that a high (low) rank household in the untreated state remains a high (low) rank household after the treatment.

2.5.1 Testing non-parametrically for heterogeneous program impacts

The main issue in program evaluation is a problem of "missing data". One cannot simultaneously observe the outcome of interest in the case of program participation (Y_1) and in the case of non-participation to the program (Y_0) for a given individual. Let T denote participation in the program, with $T = 1$ if a person participates and $T = 0$ otherwise. Because localities are randomly assigned in treatment and control groups, the experiment provides information on the marginal distributions of the outcome, i.e. $F_1(y_1 \mid T=1)$, the participants' outcomes, and $F_0(y_0 \mid T=1)$, what the participants outcomes would have been had they participated (Heckman, Smith and Clements 1997). Although the joint distribution of outcomes is never observed, it can be bounded using classical probability inequalities due to Fréchet (1951) and Hoeffding (1940). Bounds for the correlation between Y_1 and Y_0 can be estimated, and thus the variance of the impact $\Delta = Y_1 - Y_0$ can also be bounded using the Fréchet-Hoeffding inequalities.[35] One can then test whether the minimum variance is statistically different from zero. Rejecting this hypothesis implies that the program impact is heterogeneous for the population covered by the experiment. The Fréchet-Hoeffding bounds are as follows:

$$
\begin{aligned}
Max[F_1(y_1 \mid T=1) + F_0(y_0 \mid T=1) - 1, 0)] &\leq F(y_1, y_0 \mid T=1) \\
&\leq Min[F_1(y_1 \mid T=1), F_0(y_0 \mid T=1)].
\end{aligned}
$$

[35] Cambanis *et al.* (1976) showed that if $k(Y_1, Y_0)$ is superadditive (or subadditive) then the extreme values of $E(k(Y_1, Y_0) \mid T = 1)$ are obtained by the upper- and lower bounding distributions.

The upper-bound distribution corresponds to the case of perfect positive dependence between Y_0 and Y_1, i.e. the case for which the two marginal distributions are matched in ascending order. The lower-bound distribution in the Fréchet-Hoeffding inequalities corresponds to the case of perfect negative dependence, i.e. the case for which the marginal distributions are matched in the reverse order. Since the sample sizes of the treatment group and control group are different, we use the percentiles of each distribution. In each case, we calculate the outcomes correlation $r_{Y_0 Y_1}$ and derive the bounds for $Var(\Delta) = Var(Y_1) + \text{var}(Y_0) - 2r_{Y_0 Y_1}\sqrt{Var(Y_0)Var(Y_1)}$. We then test whether the lower bound of the variance, which is derived from the Fréchet-Hoeffding upper-bound distribution, equals to zero. In the common effect model (homogeneous program impacts), the impact is constant and $Var\ (\Delta) = 0$. Thus, rejecting the null hypothesis that the lower bound of the variance is zero implies a rejection of the common effect model and provides evidence of heterogeneous program impacts, under the assumption of perfect positive dependence. In addition, the Fréchet-Hoeffding bounds give an estimate of the range of values for the variance of impacts.

After decomposing the variance of impacts into the systematic variance of impacts along observable characteristics and the unobserved variance in impacts, we compute the Fréchet-Hoeffding bounds for the unobserved part of the variance in impacts. We test whether the lower bound of this variance is equal to zero. A rejection of this null hypothesis is further evidence of heterogeneous program impacts, not accounted for in the systematic variation of impacts along observable characteristics.

2.5.2 A semi-parametric analysis

The standard common effect estimator assumes that all treated households receive the same impact from the program. When comparing the distributions of the outcome variable for the treatment group and the control group, the treatment group distribution is only shifted by a constant factor. [36] We consider a model of heterogeneous impacts on the distribution that is estimated by quantile regression assuming the ranking property holds.

The advantage of quantile regression is that the impact of the program on different quantiles of the outcome of interest does not have to be constant. Thus, the estimation of quantile treatment effects allows testing the hypothesis that the treatment effect is the same for all points of the initial distribution of the outcome by testing whether the impacts are the same across quantiles of the control distribution. [37]

Quantile treatment effects (QTE) are a special case of quantile regression of the conditional mean of Y given $X = x$ where X is a discrete variable indicating whether the observation belongs to the treatment group or the control group. The quantile regression estimator minimizes a weighted sum of absolute residuals (Koenker and Bassett, 1978). Other experimental evaluations have used QTE, e.g. Heckman, Smith and Clements (1997), and Abadie, Angrist and Imbens (2002).

We present the estimates of the QTE, which are impacts conditional on the percentiles of Y_0 for the targeted outcomes: (1) wealth, proxied by the per capita value of consumption, (2) per capita value of food consumption. [38] The results on program

[36] Note that the common effect model also assumes that household rankings with respect to the outcome of interest in the treated and untreated states are unaffected by the program.

[37] However, quantile treatment effects estimation does not provide information on the quantiles of the treatment effect distribution.

[38] QTE are also estimated for per capita expenditures and per capita food expenditures.

impacts are estimated for November 1998, June 1999 and November 1999. Since the program started to send benefits in the summer 1998 and data on consumption is first collected in November 1998, the three cross-sections consist of post-program samples. Thus, we investigate quantile treatment effects in a simple difference model.

Instead of estimating QTE on the outcomes *Y*, we also estimate QTE on $\tilde{Y}$, which are obtained by removing the effect of household characteristics *X* from the outcomes *Y*. Estimating QTE on $\tilde{Y}$ is similar to estimating experimental mean program impacts conditioned on X. Note that QTE estimation on *Y* and QTE estimation on $\tilde{Y}$ are both consistent, but the latter is more efficient.

$$Y = \alpha_0 + \sum_{i=1}^{K} X_i \alpha_i + \nu,$$
$$\tilde{Y} = y - [\hat{\alpha}_0 + \sum_{i=1}^{K} X_i \hat{\alpha}_i].$$

In addition, we estimate QTE on $\tilde{Y}$ once systematic variation in impacts along program targeting criteria is removed. We compare the QTE unobserved heterogeneity in impacts with the QTE total heterogeneity in impacts. Do unobserved impacts vary along the percentiles of the untreated outcome in the same way as total impacts vary along the percentiles of the untreated outcome?

What should we expect from the bounding and QTE analyses? Both the Fréchet-Hoeffding lower bound of the variance of impacts and the QTE analysis correspond to the case of perfect positive dependence. The Fréchet-Hoeffding bounding analysis can inform us on the existence of heterogeneity in program impacts. The QTE estimation provides information on how the impact varies at different points of the untreated distribution, e.g. the first decile, the median, the last decile. The estimation of quantile treatment effects also allows us to estimate the variance of impacts over the quantiles of the untreated outcome distribution, which should be close to the lower-bound of the variance estimated using the Fréchet-Hoeffding inequalities.

Based on the theoretical framework, we expect the treatment effect on wealth and nutrition to be higher for the households whose cost of complying with the program requirements is the lower. In particular, children are required to attend school on a regular basis and their foregone earnings are an additional cost for households that relied on child labor before the start of the program. Whether unobserved treatment effects on wealth and nutrition follow this pattern is an open question.

2.5.3 Results and discussion

Table 2.5 provides evidence of the heterogeneity of program impacts on per capita expenditures, per capita value of consumption, per capita food expenditures and per capita value of food consumption using the Fréchet-Hoeffding inequalities. The standard errors are obtained from the bootstrap (Efron and Tibshirani, 1994). We find that program impacts standard deviations can range anywhere from 4 to 12 pesos (minimum) to 130 to 260 pesos (maximum). These values can be compared to the average monthly per capita cash transfer amount that eligible households are entitled to receive when they fulfill program requirements, i.e. 32 pesos. [39] The lower bound is substantively small compared to the average untreated outcome level (Table 2.5, last column). In all cases, we reject the null hypothesis that the minimum standard deviation is equal to zero at the 1% significance level. Using this non-parametric technique, we find that the experimental data are consistent with a large range of impact distributions.

[39] The average household size is 6.

In Table 2.2, we found that the systematic heterogeneity in impacts along the targeting criteria is significantly different from zero. Table 2.6 shows that the Fréchet-Hoeffding lower bound unobserved impacts standard deviation is different from zero for all the outcomes considered and in all rounds. Thus, this is evidence that the heterogeneity in impacts found in Table 2.5 is not only due to the systematic heterogeneity along program targeting criteria.

Figure 2-7 present the difference in quantiles from the two marginal distributions of the wealth and nutrition aggregates conditional on a set of observable characteristics for each round. The associated 90 percent pointwise confidence intervals are obtained from the bootstrap with 200 replications. Overall, the impacts are positive at each decile of the untreated distribution of outcomes.

For both outcomes, the difference overall increases from the lowest percentile to the highest percentile of the control group distribution. It suggests the program impact on wealth and nutrition is lower for households who were at a lower level of wealth and nutrition prior to the intervention.

In November 1998, the program impact on per capita value of consumption conditioned on X varies from about 4 pesos for the lowest decile of the untreated distribution to 8 pesos for the higher decile, i.e. increases by a factor two (Figure 2). This impact is low compared to the amount a PROGRESA household is eligible to receive upon compliance with program requirements. The impact at the median is about 7 pesos, compared to a mean impact of 8 pesos. This suggests that some households may not be getting the maximum benefit amount in November 1998, either because they have not fulfill all program requirements or because of implementation problems.

In June 1999, the impact ranges from 9 pesos to 28 pesos, with a median at 19 pesos and a mean at 17 pesos (Figure 3). Although the impact at the highest decile is close to the average per capita benefit amount, the impact at the first decile is still low. Thus, it is possible that the poorest households are still not getting the maximum benefit amount in June 1999. In November 1999, the impact ranges from 11 pesos to 22 pesos, with a median and mean at 16 pesos (Figure 4).

The mean impact between November 1998 and November 1999 has increased by a factor two, while the impact at the lowest decile and the impact at the highest decile almost increased by a factor three in a year period. The impacts on per capita food consumption are similar in magnitude to the impacts of per capita consumption (Figures 5-7). This suggests that PROGRESA affects household total consumption mainly through higher food consumption.

Table 2.7 shows evidence of a variation in program impacts from the QTE analysis, except for the first round for which impacts are homogenous along the quantiles of the untreated distribution. In addition, Table 2.7 shows the program impact standard deviation from the QTE estimation. When the program impacts are found to be heterogeneous, impact standard deviations are similar to the lowest range of values estimated using the Fréchet-Hoeffding bounds.

Results from the decomposition of program impacts into systematic impacts and unobserved impacts are given in Table 2.8. We find significant standard deviation in systematic impacts. Yet, the standard deviation of unobserved impacts is also significantly different from zero. Moreover, unobserved differences between the treatment and control group outcomes are increasing along the percentiles of the untreated outcome distribution except for the first round of data (Figures 8-13). This suggests that the variation in total impacts shown in Figures 2-7 are partly driven by the variation in the unobserved impacts.

2.6 Heterogeneous program impacts under the assumption of independence between program impacts and untreated outcome levels

In this section as in the previous section, we also investigate the overall heterogeneity of program impacts, which includes both observed and unobserved heterogeneity. In this section, the main assumption is that the untreated outcome levels and program impacts are independent. This assumption means that households do not anticipate gains from the program at the time they decide to participate in the program. This is likely to occur when households are randomly assigned to a treatment and a control group. How plausible is the assumption in the case of PROGRESA? In the Mexican program, random assignment is not at the household level but at the village level and treatment group households can choose not to fulfill program requirements. We test the independence assumption. When this assumption holds and under additional assumptions, we can plot the distribution of impacts.

Note that if the independence assumption holds, then program impacts should not vary along the quantiles of the untreated distribution. Yet, since program impacts are found to vary with the untreated distribution, the QTE results imply that the assumption of independence between program impacts and the untreated outcome levels does not hold. Yet, if the assumption of perfect positive dependence does not hold, then the variation in impacts along the quantiles of the untreated distribution is not consistent.

2.6.1 The Hildreth-Houck random coefficient model

As previously, denote by Y_0 the outcome in the untreated state, and Y_1 the outcome in the treated state. Any individual can only be observed in one or the other state. The observed outcome Y is a function of Y_0, Y_1 and the treatment indicator *T*. Suppose that the treatment effect varies for each household. We denote the household-specific program impacts by β_i. We assume that program impacts and untreated outcome levels Y_0 are uncorrelated. We have:

$$(2) \qquad Y_i = (1-T_i)\,Y_{i0} + T_i\,Y_{i1} = Y_{i0} + T_i\,(Y_{i1} - Y_{i0}).$$

Denote by $\hat{Y}_0$ the expected outcome for the control group given a vector of individual characteristics *X*, i.e. :

$$\hat{Y}_0 = E(Y \mid X, T=0).$$

We have from equation (2) that:

$$\begin{aligned} T_i = 1 \quad &\Rightarrow \quad Y_{i1} = \hat{Y}_0 + \overline{\beta} + v_{i1}, \\ T_i = 0 \quad &\Rightarrow \quad Y_{i0} = \hat{Y}_0 + v_{i0}, \end{aligned}$$

where $\overline{\beta}$ is the mean treatment effect, v_{i0} and v_{i1} are respectively the individual deviations of Y_{i0} and of Y_{i1} with respect to their means.

Thus, we have from equation (2):

$$Y_i = \hat{Y}_0 + T_i\,(\overline{\beta} + (v_{i1} - v_{i0})) + v_{i0}.$$

The household-specific program impacts β_i are as follows:

$$\beta_i = \overline{\beta} + (v_{i1} - v_{i0}).$$

Household-specific program impacts are such that:

$$\mathrm{E}(\beta_i) = \overline{\beta}, \quad \beta_i = \overline{\beta} + \sigma_i.$$

We assume that $\mathrm{E}(\sigma_i) = 0, \quad \mathrm{E}(\nu_{i0}) = 0.$

Equation (2) becomes:

$$(3) \qquad Y_i = \hat{Y}_0 + T_i \ \beta_i + \nu_{i0}.$$

Equation (3) has the structure of the Hildreth-Houck random coefficient model (1968). The independence between program impacts and untreated outcome levels implies that σ_i are independent of ν_{i0}.

Let $\varepsilon_i = T_i\sigma_i + \nu_{i0}$. Then equation (3) can be written as follows:

$$Y_i = \hat{Y}_0 + T_i \ \overline{\beta} + \varepsilon_i.$$

The Hildreth-Houck model is a heteroscedastic error structure model:

$$Var(\varepsilon_i \mid T_i = 0) = Var(\nu_{i0}),$$
$$Var(\varepsilon_i \mid T_i = 1) = Var(\sigma_i + \nu_{i0}).$$

Under the assumption that σ_i and ν_{i0} are uncorrelated, we have:

$$Var(\sigma_i + \nu_{i0}) = Var(\sigma_i) + Var(\nu_{i0}).$$

Once we assume that program impacts and the untreated outcome levels are independent, testing for heterogeneous program effect consists in testing that the variance of the error term depends on whether the household belongs to the treatment or the control group. This is done using the Breusch-Pagan test for heteroscedasticity and the LR test for groupwise heteroscedasticity (Judge *et al.* 1985). We treat the existence of negative values for $(Var(\sigma_i + \nu_{i0}) - Var(\nu_{i0}))$ as a test of the assumption of independence between program impacts and untreated outcome levels. We also estimate the difference in the variance of the OLS residuals for the treatment and the control groups. Under the assumptions of the random coefficient model, this difference should represent the variance of impacts.

In addition, we estimate the standard deviation of impacts when impacts are decomposed into systematic impacts and unobserved impacts. Finding significant impacts along observable characteristics that are correlated with the untreated outcome is evidence that the independence assumption is false for the population as a whole. We provide estimates of the standard deviation of total and unobserved impacts under the less restrictive assumption that $Y_0 \perp \beta_i \mid Z, T \times Z$.

2.6.2 Additional assumptions on the distribution of program impacts

If we assume that the impacts are normally distributed, then the estimation of the mean and variance of impacts is sufficient to plot the distribution of impacts. It also allows us to compute the percentage of the treated population that experienced a positive impact from the program.

Alternatively, we assume that the distribution of program impacts belongs to the Pearson family of distributions. This family of distributions includes as special cases the normal, chi-square, beta and gamma distributions. It only allows for one mode but includes bell curve shapes, as well as J-shaped or U-shaped curves (Kendall and Stuart, 1963). From the theoretical model that assumes that untreated outcome levels and gains from the program are uncorrelated, we have: $Y_{0i} + \beta_i = Y_{1i}$.

Since we assume that Y_{0i} and β_i are uncorrelated, we can estimate by deconvolution the four first moments of the distribution of impacts using the moments of the distribution of Y_1 and the moments of the distribution of Y_0.[40] Note that the estimation of the four moment of the distribution of impacts by deconvolution allows to further test the independence assumption. Finding a negative value for the estimated fourth moment is an indication of failure of the independence assumption. The first four moments are then used to approximate the distribution of impacts, assuming that it belongs to the Pearson family of distributions (see also Biddle, Boden and Reville 2003 who approximate the distribution of the effects of work-related injury on the subsequent earnings by a Pearson distribution). Using the estimated first four moments, we test whether the estimated distribution exists. It can be shown that all frequency distributions satisfy the following relation between the second moment about the mean μ_2, the third moment about the mean μ_3 and the fourth moment about the mean μ_4:

$$\beta_2 - \beta_1 - 1 > 0,$$
$$\beta_1 = \frac{\mu_3^2}{\mu_2^3} \text{ and } \beta_2 = \frac{\mu_4}{\mu_2^2}.$$

Rejection is interpreted as a failure of the independence assumption. When the independence assumption is not rejected, this method allows us to obtain a density function for the distribution of impacts from the Pearson family of distributions.

2.6.3 Results and discussion

All outcomes previously analyzed are per capita measures. We also estimate the random coefficient model using per capita outcomes although we do not report the estimates. [41] For per capita measures of wealth and nutrition, we find negative value for

[40] The derivation of the first four moments from deconvolution are in Appendix 4.
[41] Estimates are available upon request.

the estimated $(Var(\sigma_i+v_{i0})-Var(v_{i0}))$. Thus, we reject the assumption of independence between program impacts and untreated outcome levels for per capita measures. These results are consistent with the variation in impacts along the quantiles of the untreated distribution found from the QTE analysis under the assumption of perfect positive dependence.

In Table 2.9, we present tests of heterogeneity in the variance of the error term using the Hildreth-Houck random coefficient model for household total wealth and nutrition outcomes. The first column in Table 2.9 provides the Breusch-Pagan test results and the second column provides the LR test for groupwise heteroscedasticity results. The results are consistent in both columns. We reject the null hypothesis of no heterogeneity in most cases. As in the case of the semi-parametric estimation, rejection is less frequent when using the first round of post-program data. The third column of Table 2.9 show the estimated standard deviation of impacts associated with a standard error. The estimated standard deviation of impacts is higher in the second round. The program impacts standard deviations vary from 31 to 178 pesos for household wealth and from 50 to 150 pesos for nutrition. Recall that these impacts concern household outcome levels and therefore cannot be directly compared to the impacts on per capita outcomes that are reported in the previous analyses. Yet, they can be compared to the average monthly benefit that a household is untitled to receive upon compliance with program requirements, i.e. about 200 pesos.

Using the estimated mean and variance of impacts and assuming a normal distribution, we plot the distribution of impacts on household consumption (Figure 14) and food consumption (Figure 15). The last column of Table 2.9 shows that the percentage of the treated population with a positive impact ranges from 63% to 81%. It is higher for the latest rounds. If we believe that the normality assumption holds, then this finding suggest that, in the first round, household may have supported the cost of the program requirements without receiving the program benefits because of implementation problems. Recall that in the empirical section, we consider the effect of the offer to treat which is likely to be a lower bound for the effect of the program on the treated population because some households may not be getting the program benefits.

In Table 2.10, we show the standard deviation of impacts in the case of the less restrictive independence assumption, i.e. when impacts are allowed to vary along the targeting criteria. The systematic standard deviation of impacts and the unobserved deviation of impacts sums up to the total standard deviation of impacts under the assumption that $Y_0 \perp \beta_i \mid Z, T \times Z$. The total standard deviation of impacts in Table 2.10 is larger than the standard deviation of impacts in Table 2.9, which is derived under the stronger independence assumption $Y_0 \perp \beta_i$. When we assume that impacts follow a normal distribution, we find that 56% to 74% of the eligible households experience a positive impact under the weaker assumption that $Y_0 \perp \beta_i \mid Z, T \times Z$. This is always lower than the percentage with a positive impact under the stronger independence assumption.

We estimate the first four moments of the distribution of impacts. We find negative values for the estimated fourth moment for many of the outcomes, except consumption in November 1998 and November 1999, food expenditures in November 1998 and November 1999 and food consumption in November 1999. This result is interpreted as a failure of the independence assumption. Furthermore, the first four moments for food expenditures in November 1999 do not correspond to a well-behaved frequency distribution. Thus, we are left with estimating Pearson distributions for the program impacts on four of the outcome variables. Yet, the third column in Table 2.9 shows that the standard deviation is not significantly different from zero for one of these four outcomes, i.e. November 1998 consumption.

We find that for three of the four outcomes, i.e. November 1998 consumption, June 1999 food expenditures and November 1999 food consumption, the distribution of impacts is approximated by an L-shaped Gamma distribution (Figures 16, 18 and 19). First, this shape is clearly not similar to that of a normal distribution as assumed in Figure 14 and 15, although both shapes belong to the Pearson family. Second, all households have a positive program impacts on consumption and food consumption and expenditures. Third, these distributions of impacts have a vertical asymptote at less than 100 pesos. Most of the households have a small program impact and a few households have a large impact. Finally, the distribution of impacts on November 1999 consumption is approximated by a Type IV Pearson distribution (Figure 17). This distribution is less skewed than the estimated Gamma distribution for the other outcomes. Moreover, about 30 % of the population experienced negative impacts.

Failure of the independence assumption for most of the outcomes of interest cast doubt on the validity of the independence assumption. Some eligible households are likely to choose not to comply with the children school attendance requirement although they would still receive the food cash transfer. Thus, failure of the independence assumption is consistent with selective compliance to program requirements of the eligible population.

2.7 Conclusion

In this paper, we assess the importance of heterogeneity in impacts from the PROGRESA program. The PROGRESA experimental data help measure the effect of the "offer to treat". This commonly used experimental estimator under-estimates the value of the mean program impact for those who actually take-up program benefits. Finding a positive value is sufficient evidence that the program works.

Yet, we find evidence that program impacts are not uniformly distributed in the treated population as do Heckman, Smith and Clements (1997), Abadie, Angrist and Imbens (2002), Black, Smith, Berger and Noel (2003), Biddle, Boden and Reville (2003), Bitler, Gelbach and Hoynes (2004) for different treatment effects in the US context.

Theoretical predictions based on the PROGRESA benefit scheme indicate that program impacts on wealth and nutrition are higher for households whose cost of complying with program requirements is lower. Program impacts on education are higher when the conditionality of program is binding. The conditionality of the program can also drive some households to take-up only parts of the program benefits, which leads to heterogeneity in impacts. In addition, program impacts on welfare are likely to be small or even negative because of failures in the implementation of the program.

First, we find evidence that the program selection mechanism is only partially effective in capturing the heterogeneity in program impacts. The geographic targeting is effective but we find little if any benefit from targeting within the poor villages. The proportion of beneficiaries with a positive program impact on wealth and nutrition ranges from 63% to 99% depending on the outcome considered. This proportion is the lowest in the first six months after the start of the program. This is consistent with the fact that some households may have bear the cost of complying without receiving the benefits because of early implementation delays in sending the cash transfers. We also find that secondary school-age children from the most remote areas get the lowest program impact. This is consistent with the fact that many of them may have chosen not to attend school and do not get the PROGRESA grant.

Second, we investigate the overall heterogeneity of program impacts, which includes both unobserved and systematic heterogeneity. Methodologically, experimental data help identify subgroups effects such as heterogeneity along the targeting criteria, but do not directly identify unobserved heterogeneity in impacts. Using the Fréchet-Hoeffding inequalities from classical probability theory, we find evidence against the homogeneous impact assumption underlying most of the impact evaluation research as do Heckman, Smith and Clements (1997). This result does not rely on any assumption and thus is particularly strong evidence of heterogeneous treatment effects. We also find evidence of heterogeneity in program impacts, which is not due to systematic variation along observable characteristics. Yet, many distributions of impacts are consistent the estimated variance of impacts from the bounding analysis.

Additional assumptions are required to analyze the distribution of impacts. We consider two assumptions. The first one concerns perfect positive dependence between potential outcomes in the treatment and non-treatment state. The second relates to whether program participants anticipate the gains from the program. The second assumption is that program impacts and untreated outcome levels are independent.

First, we estimate distribution of program impacts along the quantiles of the untreated distribution under the perfect positive dependence assumption. As expected, we find that program impacts on wealth and nutrition are greater for the households who were at a higher level of wealth and nutrition prior to the intervention. We also find that the variation in total impacts along the quantiles of the untreated distribution are partly driven by the variation in the unobserved impacts along the quantiles of the untreated distribution. Second, we reject the independence assumption for most of the outcomes. This is consistent with a selective take-up of specific program component in the eligible population, as predicted in the analysis of the conditionality of the program. Yet, we estimate the full distribution of impacts for the outcomes for which the independence assumption holds, assuming it belongs to the flexible Pearson family of distributions. Most of the distributions are L-shaped, which means that most of the families get a small impact and a few families get a large program impact.

Appendices

Appendix 1: Tables of chapter 1

Table 1.1: Variables in the PROGRESA sample.

Unit of observation	**Variable**
Individual and household level	Household composition, education, health, paid and non-paid labor, farm activities, income, expenditures, living conditions, assets, decision-making within the household.
Locality level	Availability of services, main economic activities, all prices (including wages)
Module on the status of women and intrahousehold relations	Assets at marriage of the spouses, education of their parents and wealth of their families, current decision-making patterns

Table 1.2: Partition of households in the restricted sample between eligible / non-eligible and treatment / control groups.

	Treatment localities	Control localities	Total
Eligible households	**Transfers distributed after August 1998** 5,823 households	**No transfer until the end of the evaluation period** 3,400 households	9,223 households
Non-eligible households	**No transfer** 4,314 households	**No transfer** 2,946 households	7,260 households
Total	10,137 households	6,346 households	16,483 households

Table 1.3: Descriptive Statistics for the November 1998 cross-section.

	Variable name	# of Obs.	Mean	S.D.
=1 if treatment group	treated	9223	0.63	0.48
P.C. calorie consumption	pcc	8319	2072	869
P.C. value of consumption	pce	9028	181	107
P.C. family income	pcy	9136	201	122
P.C. calorie consumption from vegetables and fruits	pccveg	9184	41	36
P.C. calorie consumption from grains and cereals	pccgrn	9048	1612	949
P.C. calorie consumption from meat and meat products	pccmea	9178	109	121
P.C. calorie consumption from other food	pccotf	9176	329	178
=1 if wife earns any non-labor income	anywny	9223	0.62	0.49
=1 if wife earns any labor income	anywly	9223	0.05	0.22
=1 if wife earns any income	anywty	9223	0.64	0.48
=1 if husband earns any non-labor income	anyhny	9223	0.34	0.47
=1 if husband earns any labor income	anyhly	9223	0.88	0.32
=1 if husband earns any income	anyhty	9223	0.97	0.17
# of household income earners other than heads	Nosce	9223	0.41	0.76
Wife's non-labor income if any	wnonlaby	5719	140	114
Wife's labor income if any	wlaby	451	711	392
Wife's total income if any	wty	5873	190	233
Husband's non-labor income if any	hnonlaby	3135	151	243
Husband's labor income if any	hlaby	8159	853	343
Husband's total income of any	hty	8955	826	389
=1 if house floor made of cement	cement	9223	0.26	0.44
=1 if house has access to piped water	pipes	9223	0.28	0.45
=1 if wife lives in the same village as she lived in before her marriage	sameloc	8053	0.64	0.48
=1 if husband's father wore shoes at the time of marriage	f2shoes	8053	0.61	0.49
=1 if husband's father had some primary school education	f2sprim	8053	0.29	0.45
=1 if husband's mother had some primary school education	m2sprim	8053	0.20	0.40
=1 if wife's father wore shoes at the time of marriage	fshoes	8053	0.61	0.49
=1 if wife's father had some primary school education	fsprim	8053	0.36	0.48
=1 if husband own a house at the time of marriage	hhouse	8053	0.17	0.38

Table 1.3: continued

Variable description	Variable name	# of Obs.	Mean	S.D.
# of household members	hhsz	9221	6.60	2.53
# of children below age 4	ch4	9223	1.15	1.11
# of children 5-10	ch510	9223	1.44	1.15
# of boys 11-14	m1114	9223	0.42	0.64
# of girls 11-14	f1114	9223	0.40	0.62
# of boys 15-19	m1519	9223	0.35	0.62
# of girls 15-19	f1519	9223	0.35	0.61
# of men 20-34	m2034	9223	0.51	0.57
# of women 20-34	f2034	9223	0.62	0.58
# of men 35-54	m3554	9223	0.50	0.51
# of women 35-54	f3554	9223	0.46	0.51
# of men 55 or more	m55p	9223	0.19	0.40
# of women 55 or more	f55p	9223	0.17	0.40
Husband's # of years of schooling	h_edu	9223	2.93	2.64
Wife's # of years of schooling	w_edu	9223	2.74	2.68
=1 if husband's an ag-worker	agworker	9223	0.74	0.44
=1 if village has access to electricity	elec	9223	0.66	0.47
=1 if village has a sewage system	sewage	9223	0.12	0.33
=1 if some permanent health care facilities in the village	healthf	9223	0.84	0.36
=1 if mobile health squad in the village	mobilehf	9223	0.80	0.40
Median local price per kg of tomatoes	mp_tom	9223	10.7	1.5
Median local price per kg of onions	mp_on	9223	7.0	1.5
Median local price per kg of potatoes	mp_pot	9223	7.2	1.6
Median local price per kg of oranges	mp_orng	9223	3.6	3.1
Median local price per kg of plantains	mp_plat	9223	3.4	1.0
Median local price per kg of maize tortillas	mp_tort	9223	3.4	0.7
Median local price per kg of corn	mp_corn	9223	3.0	0.8
Median local price per kg of noodles	mp_ndle	9223	2.2	0.6
Median local price per kg of rice	mp_rice	9223	6.6	1.1
Median local price per kg of beans	mp_bean	9223	11.2	2.0
Median local price per kg of chicken	mp_chic	9223	21.6	3.5
Median local price per kg of eggs	mp_egg	9223	10.3	1.8
Median local price per kg of coffee	mp_cof	9223	10.0	3.0

Note: The variable name of the Log. of a variable starts with letter "l", e.g. Log(pce) is lpce.

Table 1.4: Changes in the number of earners in the longitudinal data.

Number of earners:	From November 1998 to June 1999	From June 1999 to November 1999
Did not change	48%	48%
Increased	19%	36%
Decreased	32%	16%

Table 1.5: Descriptive statistics for the longitudinal sample.

Variable description	Name	N	Mean	S.D.
Change in log. Of per mouth calories consumption	clpcc	36732	-0.016	0.61
Change in log. Of per capita value of consumption	clpce	36786	-0.011	0.58
Change in the # of household members who earn income	cnearners	38389	0.095	0.89
Change in the # of male earners in the household	cnmearners	38389	0.046	0.77
Change in the # of female earners in the household	Cnfearners	38389	0.050	0.46
# of household members who stopped earning income	Ndrop	38389	0.248	0.52
# of household members who started earning income	Nenter	38389	0.343	0.59
# of female household members who stopped earning income	Nfdrop	38389	0.082	0.27
# of female household members who started earning income	Nfenter	38389	0.132	0.34
# of male household members who stopped earning income	Nmdrop	38389	0.179	0.38
# of male household members who started earning income	Nmenter	38389	0.214	0.41
Change in household size	chhsz	38389	-0.040	0.44
Change in the # of children below age 4	cch4	38389	-0.036	0.44
Change in the # of children age 5 to 10	cch510	38389	-0.011	0.41
Change in the # of males age 11 to 14	cm1114	38389	0.005	0.30
Change in the # of females age 11 to 14	cf1114	38389	0.003	0.30
Change in the # of males age 15 to 19	cm1519	38389	-0.009	0.32
Change in the # of females age 15 to 19	cf1519	38389	-0.017	0.35
Change in the # of males age 20 to 34	cm2034	38389	-0.026	0.33
Change in the # of females age 20 to 34	cf2034	38389	-0.018	0.31
Change in the # of males age 35 to 54	cm3554	38389	-0.002	0.22
Change in the # of females age 35 to 54	cf3554	38389	0.003	0.22
Change in the # of males age 55 or more	cm55p	38389	0.003	0.18
Change in the # of females age 55 or more	Cf55p	38389	0.003	0.18
Per capita calorie consumption	pcc	37655	2263	1080
Per capita value of consumption	pce	37646	201	126
Per capita household income	pcy	34556	246	162
# of household income earners	nearners	38389	1.6	0.8
# of male household income earners	nmearners	38389	1.1	0.7
# of female household income earners	nfearners	38389	0.6	0.6
# of household members	hhsz	38383	5.8	2.7
=1 if treatment group	treated	38389	0.61	0.48
Number of males age 20 to 34	m2034	38389	0.5	0.6
Number of female age 20 to 34	f2034	38389	0.5	0.6
Number of males age 15 to 19	m1519	38389	0.4	0.6
Number of females age 15 to 19	f1519	38389	0.3	0.6

Note: Changes relates to the period between November 1998 and June 1999, and the period between June 1999 and November 1999. Descriptive statistics for the variables in levels are reported for the June 1999 and November 1999 pooled cross-sections.

Table 1.6: 2SLS estimation of the unitary model - lpce instrumented using lpcy.

	(1) lpcc	(2) lpccveg	(3) lpccgrn	(4) lpccmea	(5) lpccotf
lpce	0.319*** (0.080)	1.590*** (0.235)	0.248** (0.120)	1.200*** (0.239)	0.214* (0.129)
lhhsz	-0.159*** (0.055)	0.469*** (0.159)	-0.144* (0.086)	0.221 (0.172)	-0.592*** (0.088)
ch4	0.009 (0.007)	0.016 (0.016)	-0.001 (0.009)	0.007 (0.017)	0.021** (0.010)
ch510	-0.001 (0.007)	-0.020 (0.016)	-0.012 (0.010)	-0.013 (0.017)	-0.008 (0.011)
m1114	0.023** (0.009)	-0.070*** (0.023)	0.020 (0.013)	-0.027 (0.024)	0.008 (0.014)
f1114	0.020** (0.009)	-0.080*** (0.022)	0.024* (0.013)	-0.023 (0.023)	0.023* (0.013)
m1519	0.013 (0.009)	-0.054** (0.021)	0.010 (0.012)	-0.021 (0.023)	0.021 (0.013)
f1519	0.013 (0.008)	0.009 (0.020)	0.005 (0.012)	0.019 (0.021)	-0.005 (0.013)
m2034	0.024** (0.010)	-0.056** (0.025)	0.028* (0.014)	-0.038 (0.026)	0.027* (0.015)
f2034	0.001 (0.010)	-0.026 (0.025)	0.002 (0.015)	-0.045* (0.026)	0.022 (0.014)
m3554	0.012 (0.013)	-0.087*** (0.033)	0.015 (0.019)	-0.065* (0.033)	0.028 (0.019)
f3554	0.011 (0.012)	-0.023 (0.030)	0.014 (0.017)	-0.002 (0.030)	0.033* (0.018)
m55p	0.011 (0.014)	-0.056 (0.035)	0.001 (0.020)	0.015 (0.035)	0.018 (0.021)
f55p	0.014 (0.012)	0.004 (0.030)	0.007 (0.018)	0.002 (0.031)	0.024 (0.017)
h_edu	-0.008*** (0.002)	0.005 (0.004)	-0.013*** (0.002)	-0.004 (0.004)	0.001 (0.003)
w_edu	-0.007*** (0.002)	-0.003 (0.004)	-0.010*** (0.003)	0.005 (0.005)	-0.001 (0.003)
agworker	0.009 (0.010)	-0.028 (0.028)	0.021 (0.015)	-0.024 (0.028)	0.006 (0.017)
elec	-0.039*** (0.009)	-0.005 (0.021)	-0.048*** (0.013)	-0.025 (0.021)	-0.037*** (0.013)
sewage	-0.019 (0.013)	-0.008 (0.032)	-0.029 (0.020)	-0.005 (0.032)	0.044** (0.017)
healthf	0.008 (0.014)	-0.017 (0.038)	0.002 (0.022)	0.016 (0.040)	-0.001 (0.022)
mobilehf	0.007 (0.010)	-0.062** (0.025)	0.022 (0.014)	-0.010 (0.025)	0.003 (0.016)
mp_tom	-0.002 (0.004)	-0.022** (0.009)	-0.001 (0.005)	-0.030*** (0.010)	0.019*** (0.005)
mp_on	0.002 (0.003)	-0.006 (0.008)	0.003 (0.005)	0.011 (0.009)	-0.014*** (0.005)
mp_pot	0.013*** (0.003)	-0.001 (0.007)	0.016*** (0.005)	0.006 (0.008)	0.010** (0.005)
mp_orng	-0.011*** (0.002)	0.012*** (0.004)	-0.017*** (0.002)	-0.001 (0.004)	0.003 (0.002)

Table 1.6: continued

	(1) lpcc	(2) lpccveg	(3) lpccgrn	(4) lpccmea	(5) lpccotf
mp_plat	-0.005	-0.012	-0.009	-0.008	-0.001
	(0.004)	(0.009)	(0.006)	(0.010)	(0.006)
mp_tort	0.008	-0.027**	0.006	-0.006	0.039***
	(0.006)	(0.013)	(0.009)	(0.013)	(0.007)
mp_corn	0.028***	0.061***	0.029***	0.033**	0.034***
	(0.006)	(0.015)	(0.010)	(0.016)	(0.011)
mp_ndle	0.002	0.050***	-0.010	0.082***	-0.005
	(0.007)	(0.019)	(0.011)	(0.025)	(0.012)
mp_rice	-0.011***	0.005	-0.012**	-0.031***	0.016***
	(0.004)	(0.009)	(0.005)	(0.009)	(0.006)
mp_bean	0.002	-0.001	0.009***	-0.033***	0.010***
	(0.002)	(0.005)	(0.003)	(0.006)	(0.003)
mp_chic	-0.005***	-0.006*	-0.004***	0.009***	-0.012***
	(0.001)	(0.003)	(0.002)	(0.004)	(0.002)
mp_egg	0.001	-0.019***	0.002	-0.018***	0.010***
	(0.002)	(0.005)	(0.003)	(0.006)	(0.004)
mp_cof	-0.004**	-0.002	-0.006***	-0.010***	-0.007***
	(0.001)	(0.003)	(0.002)	(0.003)	(0.002)
Constant	6.268***	-4.848***	6.274***	-1.384	5.154***
	(0.508)	(1.492)	(0.768)	(1.552)	(0.819)
Observ.	8163	8657	8794	8196	8816

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 1.7: 2SLS estimation of the unitary model - lpce is instrumented using dwelling characteristics.

	(1) lpcc	(2) lpccveg	(3) lpccgrn	(4) lpccmea	(5) lpccotf
lpce	0.277*** (0.060)	1.389*** (0.136)	0.232*** (0.085)	1.392*** (0.156)	0.183** (0.089)
lhhsz	-0.189*** (0.046)	0.341*** (0.112)	-0.170** (0.070)	0.324** (0.129)	-0.609*** (0.071)
ch4	0.010 (0.006)	0.012 (0.015)	0.001 (0.009)	0.016 (0.017)	0.020** (0.010)
ch510	0.001 (0.007)	-0.021 (0.016)	-0.010 (0.010)	-0.007 (0.017)	-0.009 (0.010)
m1114	0.027*** (0.009)	-0.059*** (0.021)	0.023* (0.013)	-0.030 (0.023)	0.009 (0.013)
f1114	0.023*** (0.009)	-0.075*** (0.021)	0.027** (0.012)	-0.025 (0.022)	0.023* (0.013)
m1519	0.014* (0.009)	-0.049** (0.020)	0.012 (0.012)	-0.025 (0.022)	0.021* (0.013)
f1519	0.013 (0.008)	0.013 (0.019)	0.005 (0.012)	0.016 (0.021)	-0.005 (0.012)
m2034	0.026*** (0.010)	-0.053** (0.024)	0.030** (0.014)	-0.040 (0.025)	0.030** (0.015)
f2034	0.002 (0.010)	-0.020 (0.024)	0.005 (0.015)	-0.052** (0.026)	0.022 (0.014)
m3554	0.016 (0.013)	-0.080** (0.031)	0.019 (0.019)	-0.073** (0.033)	0.033* (0.019)
f3554	0.013 (0.012)	-0.019 (0.028)	0.016 (0.017)	-0.001 (0.031)	0.032* (0.018)
m55p	0.016 (0.014)	-0.049 (0.033)	0.006 (0.020)	0.009 (0.035)	0.021 (0.020)
f55p	0.013 (0.012)	0.002 (0.029)	0.008 (0.018)	0.005 (0.031)	0.022 (0.017)
h_edu	-0.008*** (0.002)	0.005 (0.004)	-0.013*** (0.002)	-0.005 (0.004)	0.001 (0.003)
w_edu	-0.007*** (0.002)	-0.002 (0.004)	-0.010*** (0.003)	0.004 (0.005)	-0.001 (0.003)
agworker	0.006 (0.010)	-0.041* (0.023)	0.019 (0.014)	-0.007 (0.025)	0.004 (0.015)
elec	-0.035*** (0.009)	-0.004 (0.021)	-0.045*** (0.012)	-0.033 (0.022)	-0.034*** (0.013)
sewage	-0.017 (0.013)	0.005 (0.029)	-0.028 (0.019)	-0.012 (0.030)	0.045*** (0.016)
healthf	0.005 (0.013)	-0.042 (0.028)	0.001 (0.019)	0.035 (0.032)	-0.003 (0.019)
mobilehf	0.008 (0.010)	-0.055** (0.023)	0.022 (0.014)	-0.014 (0.025)	0.005 (0.015)
mp_tom	-0.003 (0.003)	-0.028*** (0.007)	-0.001 (0.005)	-0.025*** (0.009)	0.018*** (0.005)
mp_on	0.002 (0.003)	-0.008 (0.007)	0.002 (0.005)	0.016** (0.008)	-0.014*** (0.005)
mp_pot	0.013*** (0.003)	0.001 (0.007)	0.016*** (0.004)	0.003 (0.007)	0.010** (0.004)

Table 1.7: continued.

	(1) lpcc	(2) lpccveg	(3) lpccgrn	(4) lpccmea	(5) lpccotf
mp_orng	-0.012***	0.009**	-0.018***	0.001	0.003
	(0.001)	(0.004)	(0.002)	(0.004)	(0.002)
mp_plat	-0.005	-0.011	-0.009	-0.006	-0.001
	(0.004)	(0.009)	(0.006)	(0.010)	(0.006)
mp_tort	0.008	-0.026**	0.005	-0.006	0.040***
	(0.006)	(0.013)	(0.009)	(0.013)	(0.007)
mp_corn	0.029***	0.065***	0.029***	0.026*	0.034***
	(0.006)	(0.013)	(0.009)	(0.015)	(0.010)
mp_ndle	0.004	0.052***	-0.009	0.079***	-0.003
	(0.007)	(0.018)	(0.010)	(0.025)	(0.011)
mp_rice	-0.011***	0.004	-0.012**	-0.033***	0.017***
	(0.004)	(0.009)	(0.005)	(0.009)	(0.006)
mp_bean	0.002	-0.001	0.009***	-0.034***	0.010***
	(0.002)	(0.005)	(0.003)	(0.006)	(0.003)
mp_chic	-0.005***	-0.005	-0.005***	0.009***	-0.012***
	(0.001)	(0.003)	(0.002)	(0.004)	(0.002)
mp_egg	0.001	-0.020***	0.002	-0.018***	0.010***
	(0.002)	(0.005)	(0.003)	(0.006)	(0.003)
mp_cof	-0.004***	-0.003	-0.006***	-0.010***	-0.007***
	(0.001)	(0.003)	(0.002)	(0.003)	(0.002)
Constant	6.533***	-3.554***	6.390***	-2.604**	5.346***
	(0.386)	(0.870)	(0.547)	(1.014)	(0.571)
Observ.	8231	8728	8875	8268	8890

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
IVs: CEMENT, PIPES

Table 1.8: 2SLS estimation of the unitary model - lpce is instrumented using lpcy and dwelling characteristics.

	(1) lpcc	(2) lpccveg	(3) lpccgrn	(4) lpccmea	(5) lpccotf
lpce	0.294***	1.457***	0.240***	1.354***	0.191***
	(0.049)	(0.121)	(0.070)	(0.132)	(0.073)
lhhsz	-0.172***	0.393***	-0.149**	0.316***	-0.605***
	(0.042)	(0.107)	(0.064)	(0.120)	(0.065)
ch4	0.009	0.012	-0.001	0.011	0.020**
	(0.006)	(0.015)	(0.009)	(0.017)	(0.010)
ch510	-0.001	-0.021	-0.012	-0.012	-0.009
	(0.007)	(0.016)	(0.010)	(0.017)	(0.011)
m1114	0.024***	-0.065***	0.021	-0.033	0.009
	(0.009)	(0.021)	(0.013)	(0.023)	(0.013)
f1114	0.020**	-0.077***	0.025**	-0.027	0.023*
	(0.009)	(0.021)	(0.012)	(0.022)	(0.013)
m1519	0.013	-0.052**	0.010	-0.025	0.021*
	(0.009)	(0.021)	(0.012)	(0.022)	(0.013)
f1519	0.013	0.011	0.005	0.016	-0.004
	(0.008)	(0.019)	(0.012)	(0.021)	(0.013)
m2034	0.024**	-0.053**	0.028*	-0.042	0.028*
	(0.010)	(0.024)	(0.014)	(0.026)	(0.015)
f2034	0.001	-0.024	0.003	-0.050*	0.022
	(0.010)	(0.025)	(0.015)	(0.026)	(0.014)
m3554	0.013	-0.081***	0.016	-0.072**	0.029
	(0.013)	(0.031)	(0.019)	(0.033)	(0.019)
f3554	0.011	-0.022	0.014	-0.004	0.033*
	(0.012)	(0.029)	(0.017)	(0.031)	(0.018)
m55p	0.012	-0.050	0.001	0.008	0.019
	(0.014)	(0.033)	(0.020)	(0.035)	(0.020)
f55p	0.013	0.001	0.007	0.002	0.024
	(0.012)	(0.030)	(0.018)	(0.031)	(0.017)
h_edu	-0.008***	0.005	-0.013***	-0.004	0.001
	(0.002)	(0.004)	(0.002)	(0.004)	(0.003)
w_edu	-0.007***	-0.002	-0.010***	0.004	-0.001
	(0.002)	(0.004)	(0.003)	(0.004)	(0.003)
agworker	0.007	-0.038*	0.021	-0.014	0.004
	(0.010)	(0.023)	(0.014)	(0.024)	(0.014)
elec	-0.038***	-0.003	-0.048***	-0.028	-0.037***
	(0.009)	(0.021)	(0.012)	(0.022)	(0.013)
sewage	-0.017	-0.001	-0.029	-0.014	0.046***
	(0.012)	(0.029)	(0.019)	(0.030)	(0.016)
healthf	0.005	-0.033	0.001	0.035	-0.003
	(0.012)	(0.028)	(0.018)	(0.031)	(0.018)
mobilehf	0.008	-0.056**	0.022	-0.015	0.003
	(0.010)	(0.023)	(0.014)	(0.024)	(0.015)
mp_tom	-0.003	-0.026***	-0.001	-0.026***	0.018***
	(0.003)	(0.007)	(0.004)	(0.008)	(0.005)
mp_on	0.002	-0.008	0.002	0.014*	-0.014***
	(0.003)	(0.007)	(0.004)	(0.008)	(0.005)
mp_pot	0.013***	0.001	0.016***	0.004	0.010**
	(0.003)	(0.007)	(0.004)	(0.007)	(0.004)
mp_orng	-0.012***	0.010***	-0.017***	0.001	0.003
	(0.001)	(0.004)	(0.002)	(0.004)	(0.002)

Table 1.8: continued.

	(1) lpcc	(2) lpccveg	(3) lpccgrn	(4) lpccmea	(5) lpccotf
mp_plat	-0.005 (0.004)	-0.012 (0.009)	-0.009 (0.006)	-0.007 (0.010)	-0.001 (0.006)
mp_tort	0.008 (0.006)	-0.027** (0.013)	0.006 (0.009)	-0.006 (0.013)	0.039*** (0.007)
mp_corn	0.029*** (0.006)	0.064*** (0.013)	0.029*** (0.009)	0.028* (0.015)	0.035*** (0.010)
mp_ndle	0.003 (0.007)	0.054*** (0.018)	-0.010 (0.010)	0.078*** (0.024)	-0.004 (0.011)
mp_rice	-0.011*** (0.004)	0.005 (0.009)	-0.012** (0.005)	-0.031*** (0.009)	0.016*** (0.006)
mp_bean	0.002 (0.002)	-0.001 (0.005)	0.009*** (0.003)	-0.033*** (0.006)	0.010*** (0.003)
mp_chic	-0.005*** (0.001)	-0.005 (0.003)	-0.004*** (0.002)	0.009** (0.004)	-0.012*** (0.002)
mp_egg	0.001 (0.002)	-0.020*** (0.005)	0.002 (0.003)	-0.018*** (0.006)	0.010*** (0.003)
mp_cof	-0.004*** (0.001)	-0.003 (0.003)	-0.006*** (0.002)	-0.010*** (0.003)	-0.007*** (0.002)
Constant	6.421*** (0.313)	-4.011*** (0.774)	6.328*** (0.455)	-2.378*** (0.868)	5.300*** (0.474)
Observ.	8163	8657	8794	8196	8816
test of OIR for all IVs chi2(2) (p-value)	1.79 (0.40)	1.05 (0.60)	2.95 (0.22)	3.17 (0.20)	0.58 (0.74)

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
IVs: LPCY, CEMENT, PIPES

Table 1.9: First stage regression in the case of the unitary model.

	(1) lpce	(2) lpce	(3) lpce
lpcy	0.082*** (0.009)		0.078*** (0.009)
pipes		0.042*** (0.011)	0.042*** (0.011)
cement		0.140*** (0.011)	0.138*** (0.011)
lhhsz	-0.523*** (0.044)	-0.626*** (0.044)	-0.556*** (0.044)
ch4	-0.025*** (0.008)	-0.020** (0.008)	-0.020** (0.008)
ch510	-0.009 (0.008)	-0.007 (0.008)	-0.007 (0.008)
m1114	0.038*** (0.011)	0.039*** (0.011)	0.038*** (0.011)
f1114	0.027** (0.011)	0.027** (0.011)	0.025** (0.011)
m1519	0.004 (0.011)	0.025** (0.010)	0.007 (0.010)
f1519	0.008 (0.010)	0.015 (0.010)	0.008 (0.010)
m2034	0.005 (0.013)	0.029** (0.012)	0.012 (0.012)
f2034	0.022* (0.012)	0.024** (0.012)	0.020 (0.012)
m3554	0.029* (0.016)	0.052*** (0.016)	0.037** (0.016)
f3554	0.012 (0.015)	0.017 (0.015)	0.014 (0.015)
m55p	0.029* (0.017)	0.047*** (0.016)	0.033** (0.016)
f55p	-0.012 (0.016)	-0.011 (0.015)	-0.012 (0.015)
h_edu	0.003 (0.002)	0.001 (0.002)	0.001 (0.002)
w_edu	0.007*** (0.002)	0.005*** (0.002)	0.005** (0.002)
agworker	-0.081*** (0.011)	-0.060*** (0.011)	-0.073*** (0.011)
elec	0.016 (0.011)	0.008 (0.011)	-0.001 (0.011)
sewage	0.065*** (0.015)	0.060*** (0.015)	0.056*** (0.015)
healthf	-0.112*** (0.013)	-0.109*** (0.013)	-0.098*** (0.013)
mobilehf	0.035*** (0.012)	0.032*** (0.012)	0.029** (0.012)
mp_tom	-0.027*** (0.003)	-0.026*** (0.003)	-0.025*** (0.003)
mp_on	-0.017*** (0.004)	-0.015*** (0.004)	-0.012*** (0.004)

Table 1.9: continued.

	(1) lpce	(2) lpce	(3) lpce
mp_pot	0.012***	0.012***	0.008**
	(0.003)	(0.003)	(0.003)
mp_orng	-0.011***	-0.009***	-0.010***
	(0.002)	(0.002)	(0.002)
mp_plat	-0.006	-0.003	-0.005
	(0.005)	(0.005)	(0.005)
mp_tort	-0.006	0.001	-0.005
	(0.007)	(0.006)	(0.006)
mp_corn	0.038***	0.033***	0.034***
	(0.008)	(0.007)	(0.007)
mp_ndle	0.029***	0.032***	0.028***
	(0.009)	(0.009)	(0.009)
mp_rice	-0.002	-0.003	-0.003
	(0.005)	(0.005)	(0.005)
mp_bean	0.001	0.004	0.004
	(0.003)	(0.003)	(0.003)
mp_chic	0.004***	0.003**	0.004***
	(0.001)	(0.001)	(0.001)
mp_egg	-0.001	-0.003	-0.001
	(0.003)	(0.003)	(0.003)
mp_cof	-0.004**	-0.003*	-0.003*
	(0.002)	(0.002)	(0.002)
Constant	5.812***	6.284***	5.781***
	(0.100)	(0.078)	(0.099)
Observations	8947	9028	8947
R-squared	0.25	0.26	0.26
F-test: IVs jointly significant	75.26	96.55	89.21

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 1.10: Testing the exogeneity of the treatment dummy in the unitary model

Equation	c-stat (p-value)
P.C. calorie consumption	13.11 (0.0003)
P.C. calorie consumption from vegetables and fruits	8.46 (0.0036)
P.C. calorie consumption from grains and cereals	14.29 (0.0001)
P.C. calorie consumption from meat and meat products	0.09 (0.75)
P.C. calorie consumption from other food	9.62 (0.0019)

Table 1.11: 2SLS estimation of the restricted collective model.

	(1) lpcc	(2) lpccveg	(3) lpccgrn	(4) lpccmea	(5) lpccotf
lpce	0.278*** (0.050)	1.428*** (0.123)	0.214*** (0.073)	1.364*** (0.135)	0.174** (0.075)
lwnonlaby	0.007*** (0.002)	0.013*** (0.005)	0.011*** (0.003)	-0.002 (0.005)	0.009*** (0.003)
lhhsz	-0.182*** (0.043)	0.375*** (0.107)	-0.165** (0.065)	0.323*** (0.121)	-0.616*** (0.065)
ch4	0.008 (0.006)	0.012 (0.015)	-0.002 (0.009)	0.012 (0.017)	0.020** (0.010)
ch510	-0.001 (0.007)	-0.022 (0.016)	-0.013 (0.010)	-0.012 (0.017)	-0.009 (0.011)
m1114	0.024*** (0.009)	-0.065*** (0.021)	0.021 (0.013)	-0.033 (0.023)	0.009 (0.013)
f1114	0.021** (0.009)	-0.077*** (0.021)	0.025** (0.013)	-0.027 (0.022)	0.023* (0.013)
m1519	0.013 (0.009)	-0.053** (0.021)	0.010 (0.012)	-0.025 (0.022)	0.021 (0.013)
f1519	0.013 (0.008)	0.011 (0.019)	0.005 (0.012)	0.016 (0.021)	-0.004 (0.013)
m2034	0.024** (0.010)	-0.054** (0.024)	0.027* (0.014)	-0.042 (0.026)	0.027* (0.015)
f2034	0.003 (0.010)	-0.020 (0.024)	0.006 (0.015)	-0.050* (0.026)	0.025* (0.014)
m3554	0.014 (0.013)	-0.080** (0.031)	0.017 (0.019)	-0.072** (0.033)	0.030 (0.019)
f3554	0.012 (0.012)	-0.021 (0.029)	0.015 (0.017)	-0.005 (0.031)	0.034* (0.018)
m55p	0.013 (0.014)	-0.049 (0.033)	0.002 (0.020)	0.008 (0.035)	0.020 (0.020)
f55p	0.013 (0.012)	0.001 (0.029)	0.006 (0.018)	0.002 (0.031)	0.023 (0.017)
h_edu	-0.008*** (0.002)	0.005 (0.004)	-0.013*** (0.002)	-0.004 (0.004)	0.001 (0.003)
w_edu	-0.007*** (0.002)	-0.002 (0.004)	-0.010*** (0.003)	0.003 (0.005)	-0.001 (0.003)
agworker	0.007 (0.010)	-0.038* (0.023)	0.020 (0.014)	-0.013 (0.024)	0.004 (0.014)
elec	-0.036*** (0.009)	0.001 (0.021)	-0.045*** (0.013)	-0.028 (0.022)	-0.035*** (0.013)
sewage	-0.021* (0.012)	-0.007 (0.029)	-0.034* (0.018)	-0.013 (0.030)	0.041*** (0.016)
healthf	0.005 (0.012)	-0.034 (0.028)	0.001 (0.018)	0.036 (0.031)	-0.003 (0.018)
mobilehf	0.006 (0.010)	-0.059** (0.023)	0.021 (0.014)	-0.015 (0.024)	0.002 (0.015)
mp_tom	-0.004 (0.003)	-0.027*** (0.007)	-0.002 (0.005)	-0.025*** (0.008)	0.018*** (0.005)
mp_on	0.003 (0.003)	-0.005 (0.007)	0.005 (0.004)	0.014* (0.008)	-0.012*** (0.005)
mp_pot	0.012*** (0.003)	-0.001 (0.007)	0.015*** (0.004)	0.004 (0.007)	0.009** (0.004)

Table 1.11: continued.

	(1) lpcc	(2) lpccveg	(3) lpccgrn	(4) lpccmea	(5) lpccotf
mp_orng	-0.012*** (0.001)	0.008** (0.004)	-0.019*** (0.002)	0.001 (0.004)	0.002 (0.002)
mp_plat	-0.005 (0.004)	-0.013 (0.009)	-0.010 (0.006)	-0.007 (0.010)	-0.001 (0.006)
mp_tort	0.009 (0.006)	-0.024* (0.013)	0.008 (0.009)	-0.006 (0.013)	0.041*** (0.007)
mp_corn	0.028*** (0.006)	0.063*** (0.013)	0.029*** (0.009)	0.028* (0.015)	0.035*** (0.010)
mp_ndle	0.003 (0.007)	0.054*** (0.018)	-0.010 (0.010)	0.078*** (0.024)	-0.004 (0.011)
mp_rice	-0.011*** (0.004)	0.004 (0.009)	-0.012** (0.005)	-0.031*** (0.009)	0.016*** (0.006)
mp_bean	0.001 (0.002)	-0.002 (0.005)	0.008** (0.003)	-0.033*** (0.006)	0.010*** (0.003)
mp_chic	-0.006*** (0.001)	-0.006* (0.003)	-0.005*** (0.002)	0.009** (0.004)	-0.013*** (0.002)
mp_egg	0.001 (0.002)	-0.020*** (0.005)	0.002 (0.003)	-0.018*** (0.006)	0.010*** (0.004)
mp_cof	-0.003** (0.001)	-0.002 (0.003)	-0.006*** (0.002)	-0.010*** (0.003)	-0.006*** (0.002)
Constant	6.520*** (0.321)	-3.836*** (0.788)	6.486*** (0.469)	-2.441*** (0.886)	5.401*** (0.485)
Observ.	8163	8657	8794	8196	8816
test of OIR for all IVs chi2(2) (p-value)	1.02 (0.59)	1.04 (0.49)	1.91 (0.38)	3.05 (0.21)	1.01 (0.60)
test of OIR for treated chi2(1) (p-value)	1.02 (0.31)	0.58 (0.44)	1.82 (0.17)	2.99 (0.08)	0.85 (0.35)

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
IVs: LPCY, TREATED, PIPES, CEMENT.

Table 1.12: 2SLS estimation of the unrestricted collective model.

	(1) lpcc	(2) lpccveg	(3) lpccgrn	(4) lpccmea	(5) lpccotf
lpce	0.476*** (0.087)	1.010*** (0.208)	0.462*** (0.115)	1.530*** (0.226)	-0.021 (0.135)
lhlaby	-0.004 (0.005)	0.016 (0.013)	-0.005 (0.007)	-0.028** (0.014)	0.006 (0.008)
lwnonlaby	0.005** (0.002)	0.016*** (0.005)	0.005* (0.003)	-0.001 (0.006)	0.012*** (0.004)
lhhsz	-0.061 (0.066)	0.215 (0.159)	0.044 (0.088)	0.450*** (0.173)	-0.686*** (0.103)
ch4	0.012 (0.008)	-0.016 (0.018)	0.007 (0.010)	0.008 (0.020)	0.009 (0.012)
ch510	-0.005 (0.008)	-0.037** (0.018)	-0.006 (0.010)	-0.030 (0.020)	-0.006 (0.012)
m1114	0.016 (0.010)	-0.047* (0.025)	0.023* (0.014)	-0.042 (0.027)	0.005 (0.016)
f1114	0.020** (0.010)	-0.085*** (0.024)	0.027** (0.013)	-0.053** (0.026)	0.025 (0.016)
m1519	0.008 (0.010)	-0.074*** (0.024)	0.004 (0.013)	-0.049* (0.026)	0.014 (0.015)
f1519	0.004 (0.010)	0.007 (0.023)	0.002 (0.013)	-0.007 (0.025)	-0.011 (0.015)
m2034	0.027** (0.011)	-0.040 (0.026)	0.023 (0.015)	-0.052* (0.029)	0.035** (0.017)
f2034	-0.013 (0.012)	-0.020 (0.028)	-0.019 (0.016)	-0.069** (0.030)	0.015 (0.018)
m3554	0.013 (0.015)	-0.076** (0.035)	0.009 (0.019)	-0.051 (0.038)	0.032 (0.023)
f3554	-0.001 (0.014)	-0.018 (0.033)	-0.007 (0.018)	-0.025 (0.036)	0.027 (0.021)
m55p	0.011 (0.016)	-0.019 (0.038)	0.005 (0.021)	0.002 (0.042)	0.018 (0.025)
f55p	0.006 (0.014)	-0.027 (0.034)	0.003 (0.019)	-0.032 (0.037)	0.028 (0.022)
h_edu	-0.009*** (0.002)	0.007 (0.005)	-0.011*** (0.003)	-0.007 (0.005)	0.003 (0.003)
w_edu	-0.007*** (0.002)	-0.001 (0.005)	-0.009*** (0.003)	0.004 (0.005)	-0.003 (0.003)
agworker	0.029* (0.017)	-0.116*** (0.040)	0.045** (0.022)	0.040 (0.044)	-0.032 (0.026)
elec	-0.038*** (0.010)	-0.014 (0.023)	-0.045*** (0.013)	-0.019 (0.025)	-0.034** (0.015)
sewage	-0.027** (0.013)	0.057* (0.032)	-0.046** (0.018)	-0.014 (0.035)	0.038* (0.021)
healthf	0.015 (0.015)	-0.063* (0.037)	0.019 (0.020)	0.017 (0.040)	-0.017 (0.024)
mobilehf	0.006 (0.011)	-0.042 (0.027)	0.014 (0.015)	0.002 (0.029)	-0.001 (0.017)
mp_tom	0.004 (0.004)	-0.034*** (0.009)	0.006 (0.005)	-0.025*** (0.010)	0.023*** (0.006)
mp_on	0.005 (0.003)	-0.014* (0.008)	0.008* (0.005)	0.015* (0.009)	-0.013** (0.005)

Table 1.12: continued.

	(1) lpcc	(2) lpccveg	(3) lpccgrn	(4) lpccmea	(5) lpccotf
mp_pot	0.010*** (0.003)	-0.005 (0.008)	0.012*** (0.004)	-0.003 (0.008)	0.007 (0.005)
mp_orng	-0.010*** (0.002)	0.001 (0.004)	-0.014*** (0.002)	0.003 (0.005)	0.001 (0.003)
mp_plat	-0.010** (0.004)	-0.021** (0.011)	-0.016*** (0.006)	0.004 (0.011)	0.002 (0.007)
mp_tort	0.006 (0.007)	-0.033** (0.016)	0.003 (0.009)	-0.014 (0.017)	0.038*** (0.010)
mp_corn	0.029*** (0.007)	0.061*** (0.016)	0.029*** (0.009)	0.020 (0.017)	0.025** (0.010)
mp_ndle	-0.001 (0.008)	0.073*** (0.020)	-0.012 (0.011)	0.091*** (0.022)	-0.019 (0.013)
mp_rice	-0.015*** (0.004)	0.004 (0.010)	-0.018*** (0.006)	-0.020* (0.011)	0.023*** (0.007)
mp_bean	-0.002 (0.003)	0.003 (0.006)	-0.001 (0.003)	-0.029*** (0.007)	0.009** (0.004)
mp_chic	-0.006*** (0.001)	0.001 (0.003)	-0.007*** (0.002)	0.007* (0.004)	-0.010*** (0.002)
mp_egg	0.004 (0.003)	-0.018*** (0.006)	0.005 (0.003)	-0.015** (0.007)	0.006 (0.004)
mp_cof	-0.005*** (0.002)	0.001 (0.004)	-0.005** (0.002)	-0.009** (0.004)	-0.007*** (0.002)
Constant	5.323*** (0.545)	-1.425 (1.303)	4.955*** (0.723)	-3.392** (1.420)	6.536*** (0.847)
Observ	6311	6311	6311	6311	6311
Test of OIR for all IVs chi2(7) (p-value)	9.52 (0.21)	8.82 (.26)	6.57 (.47)	10.25 (.17)	10.82 (.14)
Test of OIR for treated chi2(1) (p-value)	.37 (.53)	1.29 (.25)	.27 (.60)	4.02 (.04)	2.67 (.10)

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
IVs: LPCY, TREATED, PIPES, SAMELOC, F2SHOES, FSHOES, F2SPRIM, M2SPRIM, FSPRIM, HHOUSE.

Table 1.13: First stage regressions in the case of the restricted collective model.

	(1) lpce	(2) lwnonlaby
treated	0.049*** (0.010)	4.507*** (0.021)
lpcy	0.075*** (0.009)	0.028 (0.024)
pipes	0.038*** (0.011)	0.081*** (0.023)
cement	0.137*** (0.011)	0.041* (0.023)
lhhsz	-0.559*** (0.044)	-0.078 (0.104)
ch4	-0.020** (0.008)	0.001 (0.017)
ch510	-0.007 (0.008)	0.024 (0.019)
m1114	0.039*** (0.011)	0.129*** (0.023)
f1114	0.026** (0.011)	0.111*** (0.024)
m1519	0.007 (0.010)	0.041* (0.023)
f1519	0.008 (0.010)	0.040* (0.022)
m2034	0.011 (0.012)	0.007 (0.028)
f2034	0.022* (0.012)	-0.072** (0.029)
m3554	0.038** (0.016)	0.011 (0.034)
f3554	0.014 (0.015)	0.002 (0.033)
m55p	0.034** (0.016)	0.037 (0.040)
f55p	-0.012 (0.015)	0.058* (0.034)
h_edu	0.001 (0.002)	0.011** (0.005)
w_edu	0.005** (0.002)	0.005 (0.005)
agworker	-0.072*** (0.011)	-0.118*** (0.028)
elec	0.004 (0.011)	0.159*** (0.023)
sewage	0.050*** (0.015)	0.039 (0.030)
healthf	-0.096*** (0.013)	0.026 (0.029)
mobilehf	0.026** (0.012)	-0.069*** (0.020)
mp_tom	-0.026*** (0.003)	-0.018*** (0.007)
mp_on	-0.010*** (0.004)	-0.023*** (0.009)

Table 1.13: continued.

	(1) lpce	(2) lwnonlaby
mp_pot	0.007**	0.028***
	(0.003)	(0.007)
mp_orng	-0.011***	0.002
	(0.002)	(0.003)
mp_plat	-0.005	-0.034***
	(0.005)	(0.008)
mp_tort	-0.002	0.009
	(0.006)	(0.013)
mp_corn	0.034***	0.070***
	(0.007)	(0.015)
mp_ndle	0.029***	0.081***
	(0.009)	(0.017)
mp_rice	-0.004	0.008
	(0.005)	(0.010)
mp_bean	0.003	0.014**
	(0.003)	(0.006)
mp_chic	0.003**	-0.021***
	(0.002)	(0.004)
mp_egg	-0.001	0.010
	(0.003)	(0.007)
mp_cof	-0.003*	-0.011***
	(0.002)	(0.003)
Constant	5.792***	0.030
	(0.099)	(0.251)
Observations	8947	9136
R-squared	0.27	0.85
F-test: IVs jointly significant	72.76	12225

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 1.14: Testing the exogeneity of the treatment dummy in the unrestricted collective model using two samples – without young children (age 4 and below), and with young children (age 4 and below).

Equation	without young children c-stat (p-value)	with young children c-stat (p-value)
P.C. calorie consumption	0.01 (0.90)	0.10 (0.74)
P.C. calorie consumption from vegetables and fruits	2.13 (0.14)	0.16 (0.68)
P.C. calorie consumption from grains and cereals	0.007 (0.93)	0.30 (0.58)
P.C. calorie consumption from meat and meat products	1.55 (0.21)	1.61 (0.20)
P.C. calorie consumption from other food	0.04 (0.82)	1.98 (0.15)

Table 1.15: First stage regressions in the case of the unrestricted collective model.

	(1) lpce	(2) lhlaby	(3) lwnonlaby
treated	0.051*** (0.011)	-0.324*** (0.040)	4.549*** (0.022)
lpcy	0.072*** (0.010)	1.824*** (0.045)	0.013 (0.025)
pipes	0.046*** (0.012)	0.035 (0.046)	0.080*** (0.024)
sameloc	-0.029*** (0.010)	0.060 (0.038)	0.015 (0.020)
f2shoes	0.023 (0.016)	-0.032 (0.057)	-0.018 (0.028)
f2sprim	-0.002 (0.012)	-0.061 (0.047)	-0.046* (0.025)
m2sprim	0.012 (0.014)	0.028 (0.051)	0.012 (0.026)
fshoes	0.050*** (0.016)	-0.076 (0.057)	0.066** (0.029)
fsprim	0.031*** (0.011)	0.075* (0.042)	0.002 (0.021)
hhouse	0.033** (0.013)	-0.062 (0.048)	-0.042 (0.026)
lhhsz	-0.554*** (0.047)	1.770*** (0.196)	-0.040 (0.105)
ch4	-0.023*** (0.008)	0.057* (0.034)	-0.012 (0.017)
ch510	-0.006 (0.009)	0.009 (0.037)	0.018 (0.019)
m1114	0.038*** (0.011)	-0.145*** (0.045)	0.113*** (0.023)
f1114	0.033*** (0.011)	-0.085* (0.045)	0.104*** (0.024)
m1519	0.003 (0.011)	-0.452*** (0.049)	0.032 (0.023)
f1519	0.004 (0.011)	-0.181*** (0.045)	0.038* (0.023)
m2034	0.013 (0.013)	-0.365*** (0.060)	0.015 (0.029)
f2034	0.020 (0.013)	-0.262*** (0.060)	-0.084*** (0.029)
m3554	0.040** (0.017)	-0.241*** (0.078)	0.011 (0.036)
f3554	0.008 (0.016)	-0.389*** (0.068)	-0.020 (0.033)
m55p	0.040** (0.017)	-0.536*** (0.080)	0.043 (0.042)
f55p	-0.018 (0.016)	-0.571*** (0.072)	0.031 (0.035)
h_edu	-0.001 (0.002)	0.059*** (0.008)	0.006 (0.005)
w_edu	0.004 (0.002)	-0.034*** (0.009)	0.004 (0.005)
agworker	-0.080*** (0.012)	1.513*** (0.062)	-0.170*** (0.028)

Table 1.15: continued.

	(1) lpce	(2) lhlaby	(3) lwnonlaby
elec	-0.010	-0.120***	0.090***
	(0.012)	(0.041)	(0.022)
sewage	0.045***	-0.038	0.033
	(0.016)	(0.057)	(0.030)
healthf	-0.115***	0.041	-0.011
	(0.014)	(0.054)	(0.029)
mobilehf	0.031**	0.062	-0.055***
	(0.012)	(0.046)	(0.021)
mp_tom	-0.026***	0.032**	-0.021***
	(0.003)	(0.013)	(0.007)
mp_on	-0.011***	0.003	-0.025***
	(0.004)	(0.017)	(0.009)
mp_pot	0.008**	-0.057***	0.027***
	(0.004)	(0.014)	(0.007)
mp_orng	-0.011***	0.014**	0.001
	(0.002)	(0.006)	(0.003)
mp_plat	-0.001	-0.023	-0.039***
	(0.005)	(0.019)	(0.008)
mp_tort	-0.012*	-0.049*	0.005
	(0.007)	(0.027)	(0.014)
mp_corn	0.030***	0.056*	0.054***
	(0.008)	(0.030)	(0.016)
mp_ndle	0.026***	-0.047	0.052***
	(0.009)	(0.035)	(0.016)
mp_rice	-0.001	0.029	0.005
	(0.005)	(0.020)	(0.010)
mp_bean	0.003	0.031***	0.007
	(0.003)	(0.011)	(0.006)
mp_chic	0.005***	-0.014**	-0.013***
	(0.002)	(0.007)	(0.005)
mp_egg	-0.002	0.017	0.016**
	(0.003)	(0.013)	(0.007)
mp_cof	-0.004**	-0.010	-0.008**
	(0.002)	(0.007)	(0.004)
Constant	5.845***	-6.646***	0.254
	(0.106)	(0.441)	(0.266)
Observations	7814	7911	7981
R-squared	0.26	0.41	0.87
F-test: Ivs jointly significant	19.40	169.47	4616.72

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 1.16: Testing for the strict exogeneity restriction for the extended family.

	(1) clpcc	(2) clpcc
clpce	0.454***	0.455***
	(0.006)	(0.007)
cnearners	-0.015***	
	(0.004)	
chhsz	0.006	0.006
	(0.011)	(0.011)
lpce	-0.002	-0.006
	(0.006)	(0.006)
nearners	-0.013***	
	(0.004)	
cch4	0.055***	0.053***
	(0.009)	(0.009)
cch510	0.045***	0.044***
	(0.009)	(0.009)
cm1114	0.040***	0.039***
	(0.011)	(0.011)
cf1114	0.055***	0.054***
	(0.011)	(0.011)
cm1519	0.047***	0.046***
	(0.011)	(0.011)
cf1519	0.042***	0.041***
	(0.010)	(0.010)
cm2034	0.019*	0.021**
	(0.010)	(0.010)
cf2034	0.011	0.009
	(0.011)	(0.011)
cm3554	0.045***	0.049***
	(0.016)	(0.016)
cf3554	0.014	0.013
	(0.017)	(0.017)
cm55p	0.015	0.020
	(0.021)	(0.021)
cf55p	0.017	0.015
	(0.020)	(0.020)
Nfenter		0.033***
		(0.010)
Nfdrop		0.034***
		(0.011)
Nmdrop		0.013
		(0.008)
Nmenter		-0.018**
		(0.008)
nmearners		-0.023***
		(0.005)
nfearners		-0.011*
		(0.006)
Constant	0.024	0.047
	(0.034)	(0.035)
Observations	35489	35489
R-squared	0.18	0.18

Robust standard errors in parentheses;
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 1.17: First stage regression for the estimation of the first-difference model.

	(1) cnearners	(2) Nmenter	(3) Nfenter	(4) Nmdrop	(5) Nfdrop
lpcy	0.355***	0.111***	0.020***	-0.155***	-0.008***
	(0.006)	(0.003)	(0.003)	(0.003)	(0.002)
m1519	0.018**	0.096***	-0.002	0.080***	0.017***
	(0.009)	(0.004)	(0.003)	(0.003)	(0.002)
f1519	0.014*	0.020***	0.045***	0.006*	0.045***
	(0.009)	(0.004)	(0.004)	(0.003)	(0.003)
treated		-0.027***	0.058***	0.047***	-0.040***
		(0.005)	(0.004)	(0.004)	(0.003)
m2034		0.041***	-0.020***	0.049***	-0.006**
		(0.004)	(0.003)	(0.003)	(0.002)
f2034		0.015***	0.026***	-0.031***	0.028***
		(0.004)	(0.004)	(0.003)	(0.003)
chhsz	0.034**	-0.007	0.013*	-0.019***	-0.003
	(0.016)	(0.009)	(0.007)	(0.006)	(0.005)
cch4	-0.017	-0.015**	0.024***	0.011**	0.008*
	(0.013)	(0.007)	(0.005)	(0.005)	(0.005)
cch510	-0.036***	-0.019***	-0.002	-0.001	0.004
	(0.013)	(0.007)	(0.005)	(0.005)	(0.004)
cm1114	-0.027	-0.001	-0.011*	0.009	-0.001
	(0.018)	(0.009)	(0.007)	(0.007)	(0.005)
cf1114	-0.047***	-0.011	-0.002	0.005	0.022***
	(0.017)	(0.009)	(0.007)	(0.006)	(0.006)
cm1519	-0.002	-0.027***	0.005	-0.013**	-0.006
	(0.018)	(0.009)	(0.006)	(0.006)	(0.005)
cf1519	-0.031**	-0.011	-0.003	0.009	-0.003
	(0.016)	(0.008)	(0.006)	(0.006)	(0.005)
cm2034	-0.015	-0.005	0.008	0.014**	0.006
	(0.016)	(0.009)	(0.006)	(0.006)	(0.005)
cf2034	-0.007	-0.014	0.007	0.011*	0.001
	(0.018)	(0.009)	(0.007)	(0.006)	(0.005)
cm3554	-0.054**	0.011	-0.033***	0.043***	-0.011
	(0.023)	(0.013)	(0.009)	(0.008)	(0.007)
cf3554	0.004	0.003	0.007	-0.012	0.007
	(0.025)	(0.014)	(0.010)	(0.009)	(0.007)
cm55p	-0.078***	0.023	-0.050***	0.051***	0.002
	(0.029)	(0.015)	(0.012)	(0.010)	(0.009)
cf55p	-0.008	-0.024	0.021*	0.011	0.003
	(0.029)	(0.015)	(0.011)	(0.010)	(0.008)
Constant	-1.687***	-0.409***	-0.017	0.885***	0.103***
	(0.032)	(0.015)	(0.016)	(0.016)	(0.010)
Observ.	34556	34556	34556	34556	34556
R-squared	0.10	0.07	0.02	0.14	0.02
F-test: IVs jointly significant	1209.21	437.23	80.82	605.15	128.04

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 1.18: Estimation of the first-difference model in the case of the extended family.

	(1)	(2)
cnearners	-0.091***	
	(0.011)	
Nfenter		0.198**
		(0.080)
Nfdrop		-0.154*
		(0.092)
Nmenter		-0.102***
		(0.035)
Nmdrop		0.178***
		(0.030)
clpce	0.466***	0.460***
	(0.006)	(0.006)
chhsz	0.003	-0.001
	(0.011)	(0.012)
cch4	0.060***	0.052***
	(0.010)	(0.010)
cch510	0.046***	0.049***
	(0.010)	(0.010)
cm1114	0.035***	0.039***
	(0.012)	(0.012)
cf1114	0.048***	0.055***
	(0.011)	(0.012)
cm1519	0.051***	0.047***
	(0.011)	(0.011)
cf1519	0.041***	0.039***
	(0.010)	(0.011)
cm2034	0.021*	0.017
	(0.011)	(0.011)
cf2034	0.005	0.002
	(0.012)	(0.012)
cm3554	0.046***	0.049***
	(0.016)	(0.017)
cf3554	0.001	0.001
	(0.018)	(0.018)
cm55p	0.014	0.025
	(0.021)	(0.022)
cf55p	-0.004	-0.013
	(0.021)	(0.021)
Constant	0.004	-0.032**
	(0.004)	(0.015)
Observations	32065	32065

Note: Instruments in the first column (equation 2.3 in the text) are LPCY, M1519 and F1519. Instruments in the second column (equation 2.4 in the text) are LPCY, TREATED, M2034, F2034, M1519 and F1519.

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 1.18: continued.

	(1)	(2)
Test Nfdrop=-Nfenter		0.14 (0.71)
Test Nmdrop=-Nmenter		1.85 (0.17)
Test Nfdrop=Nmdrop		9.78 (0.02)
Test Nfenter=Nmenter		11 (0.0009)
Test of OIR for all IVs	1.27 (0.52)	0.47 (0.78)
Test of OIR for lpcy	0.86 (0.35)	
test of OIR for treated		0.22 (0.63)

Table 1.19: Testing the unitary model restriction based on the restricted collective model and the unrestricted collective model.

	Restricted collective model chi2 (1) (p-value)	Unrestricted collective model chi2 (2) (p-value)
Total calories	13.59	6.66
	(0.0002)	(0.03)
Vegetable and fruits calories	8.39	8.70
	(0.0038)	(0.01)
Cereals and grains calories	14.78	4.55
	(0.0001)	(0.10)
Meat and meat products calories	0.17	4.44
	(0.6814)	(0.11)
Other food calories	8.84	12.60
	(0.0029)	(0.0002)

Table 1.20: Testing Pareto-efficiency in the unrestricted collective model for pairs of goods.

chi2 (1) (p-value)	Vegetable and fruits calories	Cereals and grains calories	Meat and meat products calories
Cereals and grains calories	4.20	_	_
	(0.04)		
Meat and meat products calories	1.40	1.39	_
	(0.23)	(0.23)	
Other food calories	0.79	2.62	1.99
	(0.37)	(0.10)	(0.15)

Appendix 2: Tables of results and figures for chapter 2

Table 2.1: Impacts on wealth and nutrition along program targeting criteria

	November 1998	June 1999	November 1999
P.C. Consumption			
T	10.09 (15.26)	38.69** (16.27)	61.87*** (12.76)
T*Pindex	0.01 (0.02)	0.03 (0.02)	0.07*** (0.02)
T*Vindex	91.71*** (10.83)	46.44*** (11.84)	28.29*** (8.54)
T*Pindex*Vindex	0.12*** (0.02)	0.06*** (0.02)	0.04*** (0.01)
$H_0 : \beta_1 = \beta_2 = \beta_3 = 0.$ p-value	0.0001	0.0001	0.0001
P.C. Expenditures			
T	6.75 (12.89)	32.16** (14.52)	39.65*** (10.99)
T*Pindex	0.01 (0.02)	0.03 (0.02)	0.05*** (0.02)
T*Vindex	97.19*** (8.84)	58.60*** (10.93)	49.58*** (7.41)
T*Pindex*Gindex	0.14*** (0.01)	0.09*** (0.02)	0.06*** (0.01)
$H_0 : \beta_1 = \beta_2 = \beta_3 = 0.$ p-value	0.0001	0.0001	0.0001
P.C. Food Consumption			
T	18.86* (11.57)	28.79*** (10.58)	41.31*** (9.86)
T*Pindex	0.02 (0.02)	0.03* (0.02)	0.04*** (0.01)
T*Vindex	52.10*** (7.76)	17.64** (7.82)	8.12 (6.37)
T*Pindex*Vindex	0.07*** (0.01)	0.02 (0.01)	0.01 (0.01)
$H_0 : \beta_1 = \beta_2 = \beta_3 = 0.$ p-value	0.0001	0.0001	0.0001
P.C. Food Expenditures			
T	13.73 (8.89)	22.17*** (7.47)	22.11*** (6.97)
T*Pindex	0.02 (0.01)	0.02* (0.01)	0.02** (0.01)
T*Vindex	60.29*** (5.86)	23.61*** (6.16)	33.17*** (4.97)
T*Pindex*Vindex	0.09*** (0.01)	0.03*** (0.01)	0.04*** (0.01)
$H_0 : \beta_1 = \beta_2 = \beta_3 = 0.$ p-value	0.0001	0.0001	0.0001

*** Significant at 1%, ** Significant at 5%, * Significant at 10%. Robust Standard Errors in parentheses.

Table 2.2: Fraction with a positive program impact on wealth and nutrition using treatment effects on subgroups defined by the targeting criteria

	Systematic impacts standard deviation (S.E.)*	H_o: any program impact p-value	Fraction with a positive program impact
November 1998			
P.C. Consumption	16.16 (0.06)	0.0001	0.81
P.C. Expenditures	17.11 (0.08)	0.0001	0.67
P.C. Food Consumption	11.03 (0.05)	0.0001	0.83
P.C. Food Expenditures	12.43 (0.07)	0.0001	0.76
June 1999			
P.C. Consumption	10.82 (0.07)	0.0001	0.992
P.C. Expenditures	10.21 (0.09)	0.0001	0.97
P.C. Food Consumption	6.90 (0.05)	0.0001	0.9995
P.C. Food Expenditures	8.08 (0.07)	0.0001	0.9975
November 1999			
P.C. Consumption	13.44 (0.07)	0.0001	0.96
P.C. Expenditures	12.26 (0.09)	0.0001	0.93
P.C. Food Consumption	8.50 (0.04)	0.0001	0.987
P.C. Food Expenditures	7.53 (0.07)	0.0001	0.987

Table 2.3: Program Impacts on boys' time allocation outcomes.

	T Coefficient (std. err.)	T*Pindex Coefficient (std. err.)	T*Vindex Coefficient (std. err.)	T*Pindex*Vindex Coefficient (std. err.)	Fraction with a positive program impact	H_o: any impact p-value $H_0 : \beta_1 = \beta_2 = \beta_3 = 0$. p-value
Primary school age boys						
Participation in school	0.064 (.085)	0.00006 (0.0001)	-0.032 (0.063)	-0.00006 (0.00009)	0.97	0.413 0.816
Time spent in school	34.52 (53.39)	0.037 (0.078)	-20.12 (39.84)	-0.043 (0.062)	0.96	0.436 0.796
Participation in labor activities	0.036 (.033)	0.0001* (0.00005)	-0.074** (0.03)	-0.0001*** (0.00004)	0.17	0.001*** 0.004***
Time spent working	148 (178)	0.39 (0.27)	-376*** (143)	-0.74*** (0.22)	0.15	0.0009*** 0.002***
Participation in domestic activities	0.11 (0.07)	0.0001 (0.0001)	-0.16*** (0.06)	-0.0002*** (0.0001)	0.23	0.084* 0.055*
Time spent in domestic activities	21.92 (33.45)	0.033 (0.049)	-46.8* (25.1)	-0.072* (0.039)	0.51	0.3195 0.319
Secondary school age boys						
Participation in school	0.038 (.09)	0.00006 (0.0001)	-0.148** (0.069)	-0.0003*** (0.0001)	0.65	0.007*** 0.005***
Time spent in school	23.8 (76.5)	0.043 (0.11)	-136** (58)	-0.27*** (0.08)	0.63	0.005*** 0.003***
Participation in labor activities	0.011 (0.069)	0.00007 (0.0001)	-0.095* (0.053)	-0.0001** (0.00008)	0.013	0.007*** 0.025**
Time spent working	28 (142)	0.15 (0.20)	-210* (109)	-0.36** (0.16)	0.02	0.007*** 0.186
Participation in domestic activities	0.11 (0.08)	0.0001 (0.0001)	0.004 (0.067)	0.00001 (0.0001)	0.47	0.691 0.524
Time spent in domestic activities	14.6 (36)	0.014 (0.052)	21.5 (27.5)	0.037 (0.042)	0.85	0.771 0.771

*** Significant at 1%, ** Significant at 5%, * Significant at 10%. Robust standard error in parentheses. In the participation equations, coefficients reported measure change in the probability of participating.

Table 2.4: Program Impacts on girls' time allocation outcomes.

	T Coefficient (std. err.)	T*Pindex Coefficient (std. err.)	T*Vindex Coefficient (std. err.)	T*Pindex*Vindex Coefficient (std. err.)	Fraction with a positive program impact	H_o: any impact p-value $H_0 : \beta_1 = \beta_2 = \beta_3 = 0$. p-value
Primary school age girls						
Participation in school	0.013 (.087)	0.00003 (0.0001)	-0.012 (0.063)	-0.00005 (0.0001)	0.58	0.732 0.610
Time spent in school	1.97 (53.41)	0.0007 (0.078)	-16.38 (39.35)	-0.05 (0.06)	0.76	0.428 0.478
Participation in labor activities	0.026 (.026)	0.00003 (0.00004)	-0.04* (0.02)	-0.00005 (0.00003)	0.90	0.238 0.354
Time spent working	207 (209)	0.19 (0.31)	-289* (151)	-0.39 (0.24)	0.89	0.169 0.232
Participation in domestic activities	0.08 (0.08)	0.0001 (0.0001)	-0.22*** (0.06)	-0.0003*** (0.0001)	0.37	0.01*** 0.005***
Time spent in domestic activities	46 (40)	0.06 (0.06)	-108*** (30)	-0.15*** (0.04)	0.64	0.009*** 0.003***
Secondary school age girls						
Participation in school	0.26*** (.08)	0.0003** (0.0001)	-0.26*** (0.069)	-0.0004*** (0.0001)	0.93	0.0001*** 0.0008***
Time spent in school	239*** (82)	0.28** (0.11)	-230*** (63)	-0.36*** (0.09)	0.92	0.0001*** 0.001***
Participation in labor activities	0.003 (0.003)	0.00003 (0.00005)	-0.034 (0.029)	-0.00003** (0.00004)	0.46	0.496 0.340
Time spent working	125 (223)	0.14 (0.32)	-168 (182)	-0.14 (0.28)	0.45	0.593 0.437
Participation in domestic activities	0.013 (0.09)	0.00008 (0.0001)	-0.11 (0.06)	0.00017* (0.0001)	0.001	0.03** 0.412
Time spent in domestic activities	-38 (45)	-0.027 (0.066)	-39 (35)	-0.06 (0.05)	0.008	0.04** 0.561

*** Significant at 1%, ** Significant at 5%, * Significant at 10%. Robust standard error in parentheses. In the participation equations, coefficients reported measure change in the probability of participating.

Table 2.5: Bounds on the standard deviation of total impacts using the Fréchet-Hoeffding Inequalities

Impact Standard Deviation (Standard error)	Lower-bound total impacts standard deviation (S.E.)	Upper-bound total impacts standard deviation (S.E.)	Average untreated outcome level
November 1998			
Per capita household expenditures	4.8*** (.02)	221*** (1.1)	162
Per capita value of consumption	12.8*** (0.28)	258*** (1.3)	201
Per capita food expenditures	4.36*** (.07)	161*** (0.8)	118
Per capita value of food consumption	6.32*** (0.13)	202*** (1.1)	158
June 1999			
Per capita household expenditures	8.49*** (0.23)	203*** (1.1)	144
Per capita value of consumption	7.68*** (0.09)	241*** (1.3)	180
Per capita food expenditures	7.36*** (0.15)	137*** (0.7)	99
Per capita value of food consumption	8.42*** (.08)	181*** (1.07)	134
November 1999			
Per capita household expenditures	6.82*** (0.15)	193*** (0.98)	146
Per capita value of consumption	6.20*** (0.12)	226*** (1.2)	180
Per capita food expenditures	3.33*** (0.04)	128*** (0.6)	98
Per capita value of food consumption	3.81*** (.18)	163*** (0.8)	131

Note: *** 1% significance level, Bootstrap S.E. in parentheses (500 replications). Results obtained using the percentiles of the two empirical c.d.f.s.

Table 2.6: Bounds of the unobserved part of the standard deviation of impacts using the Fréchet-Hoeffding Inequalities

	Lower-bound unobserved impacts standard deviation (S.E.)	Upper-bound unobserved impacts standard deviation (S.E.)
November 1998		
Per capita household expenditures	3.02*** (.03)	211*** (1.1)
Per capita value of consumption	15.07*** (.32)	249*** (1.3)
Per capita food expenditures	1.83*** (.01)	154*** (.77)
Per capita value of food consumption	8.77*** (.23)	197*** (1.1)
June 1999		
Per capita household expenditures	10.66*** (.25)	196*** (1.1)
Per capita value of consumption	10.36*** (.14)	237*** (1.3)
Per capita food expenditures	9.36*** (.19)	133*** (.70)
Per capita value of food consumption	7.84*** (.04)	178*** (1.1)
November 1999		
Per capita household expenditures	6.52*** (.11)	184*** (.98)
Per capita value of consumption	5.65*** (.03)	216*** (1.1)
Per capita food expenditures	5.24*** (.02)	124*** (.59)
Per capita value of food consumption	7.08*** (.03)	159*** (.84)

* Standard Errors for the systematic standard deviation are obtained from the boootstrap with 500 replications.

Table 2.7: Standard Deviation of Total Impacts from the QTE estimation

	H_o: impact is constant across deciles of the untreated distribution p-value	Standard Deviation of program impacts (S.E.)
November 1998		
P.C. Consumption	0.2665	3.34 (.04)
P.C. Expenditures	0.5834	2.14 (.03)
P.C. Food Consumption	0.1355	3.10 (.03)
P.C. Food Expenditures	0.7935	1.65 (.03)
June 1999		
P.C. Consumption	0.0001	2.83 (.03)
P.C. Expenditures	0.0002	1.90 (.03)
P.C. Food Consumption	0.0001	2.91 (.04)
P.C. Food Expenditures	0.0001	1.46 (.02)
November 1999		
P.C. Consumption	0.0049	3.04 (.04)
P.C. Expenditures	0.0216	2.06 (.03)
P.C. Food Consumption	0.0001	3.23 (.03)
P.C. Food Expenditures	0.0051	1.65 (.02)

The dependent variable is the residual from the OLS regression of the outcome on a set of covariates X that includes household size, household composition and characteristics of the head of household.
Standard Errors are obtained from the bootstrap with 200 replications.

Table 2.8: Systematic and unobserved heterogeneity in impacts from the QTE estimation

	Standard Deviation in systematic impacts	Standard Deviation of Unobserved impacts	H_o: no unobserved heterogeneity in impacts - p-value
November 1998			
P.C. Consumption	12.07 (.03)	3.29 (.05)	0.507
P.C. Expenditures	11.85 (.04)	2.16 (.04)	0.148
P.C. Food Consumption	7.75 (.02)	2.89 (.03)	0.310
P.C. Food Expenditures	7.93 (.03)	1.60 (.02)	0.613
June 1999			
P.C. Consumption	12.07 (.03)	3.22 (.05)	0.003
P.C. Expenditures	11.79 (.04)	2.02 (.03)	0.012
P.C. Food Consumption	7.75 (.02)	2.86 (.03)	0.001
P.C. Food Expenditures	7.84 (.03)	1.68 (.02)	0.001
November 1999			
P.C. Consumption	12.10 (.03)	3.36 (.05)	0.053
P.C. Expenditures	11.86 (.04)	2.10 (.04)	0.326
P.C. Food Consumption	7.76 (.02)	3.17 (.04)	0.004
P.C. Food Expenditures	7.94 (.03)	1.84 (.03)	0.001

The dependent variable is the residual from the OLS regression of the outcome on a set of covariates X that includes household size, household composition and characteristics of the head of household.
Standard Errors are obtained from the bootstrap with 200 replications.

Table 2.9: Testing the presence of heteroscedasticity from the Hildreth-Houck Random Coefficient Model for Household-level Outcome Variables under the strong independence assumption.

In the estimation results below, the only random coefficient is β, T is the treatment dummy and X a set of control variables ::

$y = \alpha + \beta_i T + \delta X + \varepsilon$

	Breusch-Pagan LM-stat (p-value)	LR-stat for groupwise heteroscedasticity (p-value)	S.D.(Δ)	% with a positive impact †
Household Expenditures November 98	0.067 (0.79)	0.4 (0.527)	31.63 (74.4)	81
Household Expenditures June 99	8.78*** (0.003)	11.39*** (0.001)	107* (77.1)	77
Household Expenditures November 99	1.18 (0.27)	0.45 (0.5)	60.77 (71.5)	71
Household value of consumption November 98	5.83*** (0.01)	7.08*** (0.008)	118 (99)	63
Household value of consumption June 99	25.58*** (0.0001)	33.78*** (0.0001)	178*** (106)	72
Household value of consumption November 99	11.58*** (0.0001)	4.179** (0.041)	132* (98)	76
Household food expenditures November 98	1.67 (0.19)	2.605 (0.107)	50.45 (51)	66
Household food expenditures June 99	40.87*** (0.0001)	54.71*** (0.0001)	107*** (53)	71
Household food expenditures November 99	27.08*** (0.0001)	15.47*** (0.0001)	89*** (46)	74
Household value of food consumption November 98	3.82** (0.05)	4.126** (0.042)	84 (84)	64
Household value of food consumption June 99	37.46*** (0.0001)	49.57*** (0.0001)	149*** (87)	70
Household value of food consumption November 99	54.56*** (0.0001)	43.39*** (0.0001)	147*** (81)	71

Note: *** indicates 1% significance level, ** indicates 5% significance level.
† Percentage with a positive impact is derived assuming a normal distribution.

Table 2.10: Heterogeneity in impacts from the Hildreth-Houck Random Coefficient Model for Household-level Outcome Variables under the less restrictive independence assumption.

In the model below, the only random coefficient is β, T is the treatment dummy, Z is a subgroup indicator and X a set of control variables:

$y = \alpha + \beta_i T + \gamma Z + \beta_1 T \times Z + \delta X + \varepsilon$

	S.D. in systematic impacts (S.E.)†	S.D. in unobserved impacts (S.E.)	% with a positive unobserved impact †	S.D.(Δ) (S.E.)	% with a positive total impact †
Household Expenditures November 98	59 (.45)	79 (75.3)	92	92 (75)	64
Household Expenditures June 99	60 (.46)	119** (77)	99	131 (77)	74
Household Expenditures November 99	66 (.45)	100.5** (70.9)	99	120 (70)	70
Household value of consumption November 98	71 (.63)	152** (100)	87	164 (100)	56
Household value of consumption June 99	64 (.62)	169** (106)	98	186 (106)	70
Household value of consumption November 99	69 (.59)	158** (98)	99	178 (98)	73
Household food expenditures November 98	45 (.32)	58.7 (51.7)	97	74 (51)	62
Household food expenditures June 99	43 (.30)	108*** (53)	98	117 (53)	70
Household food expenditures November 99	49 (.32)	100*** (46)	98	116 (46)	69
Household value of food consumption November 98	53 (.44)	107 (85)	92	118 (85)	60
Household value of food consumption June 99	55 (.50)	142** (87)	92	150 (87)	70
Household value of food consumption November 99	48 (.45)	147*** (81)	98	154 (81)	70

† Percentage with a positive impact is derived assuming a normal distribution.

† S.E. are computed from the bootstrap.

Figure 1: Types of households based on their indirect utility levels

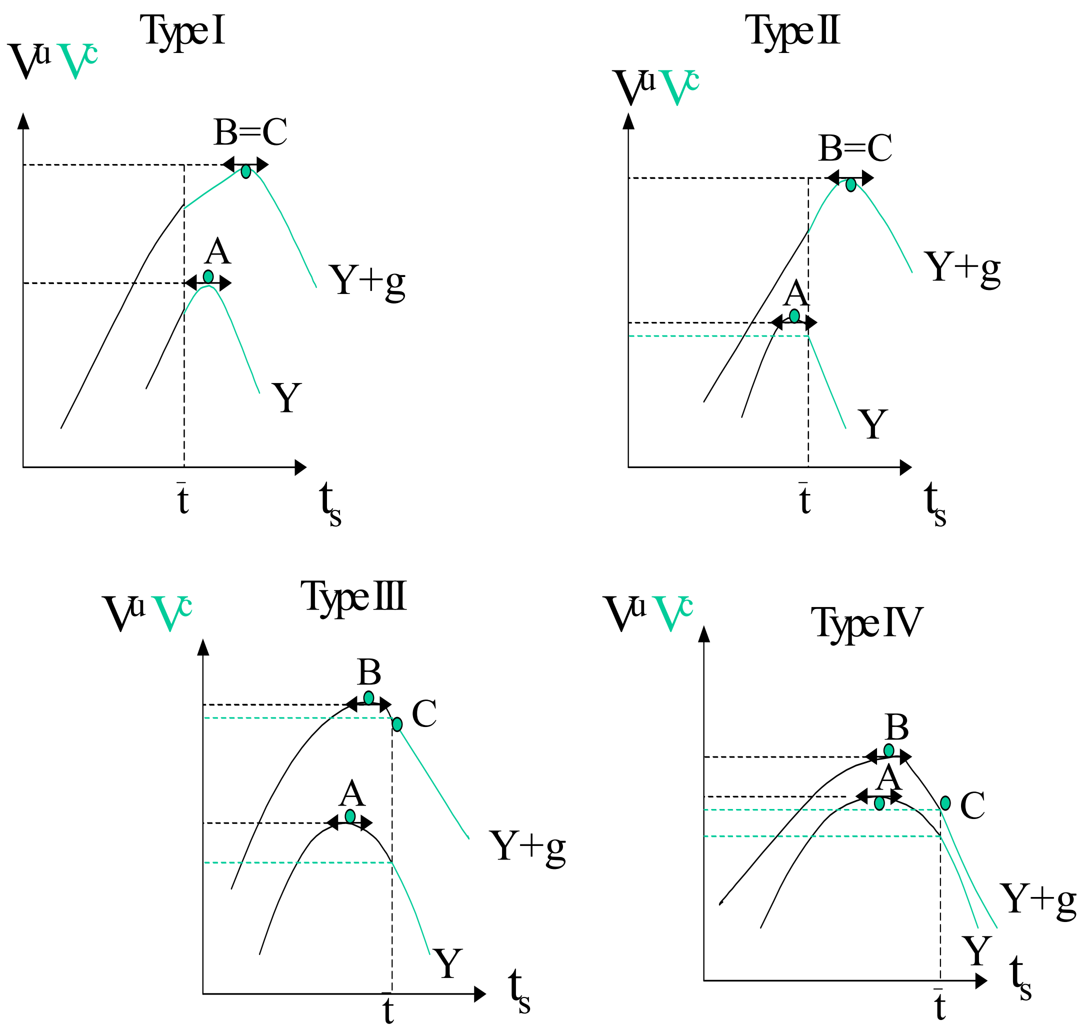


Figure 2: Difference in per capita value of consumption in the treatment and control group in November 1998 (Nov 1998 pesos) controlling for covariates

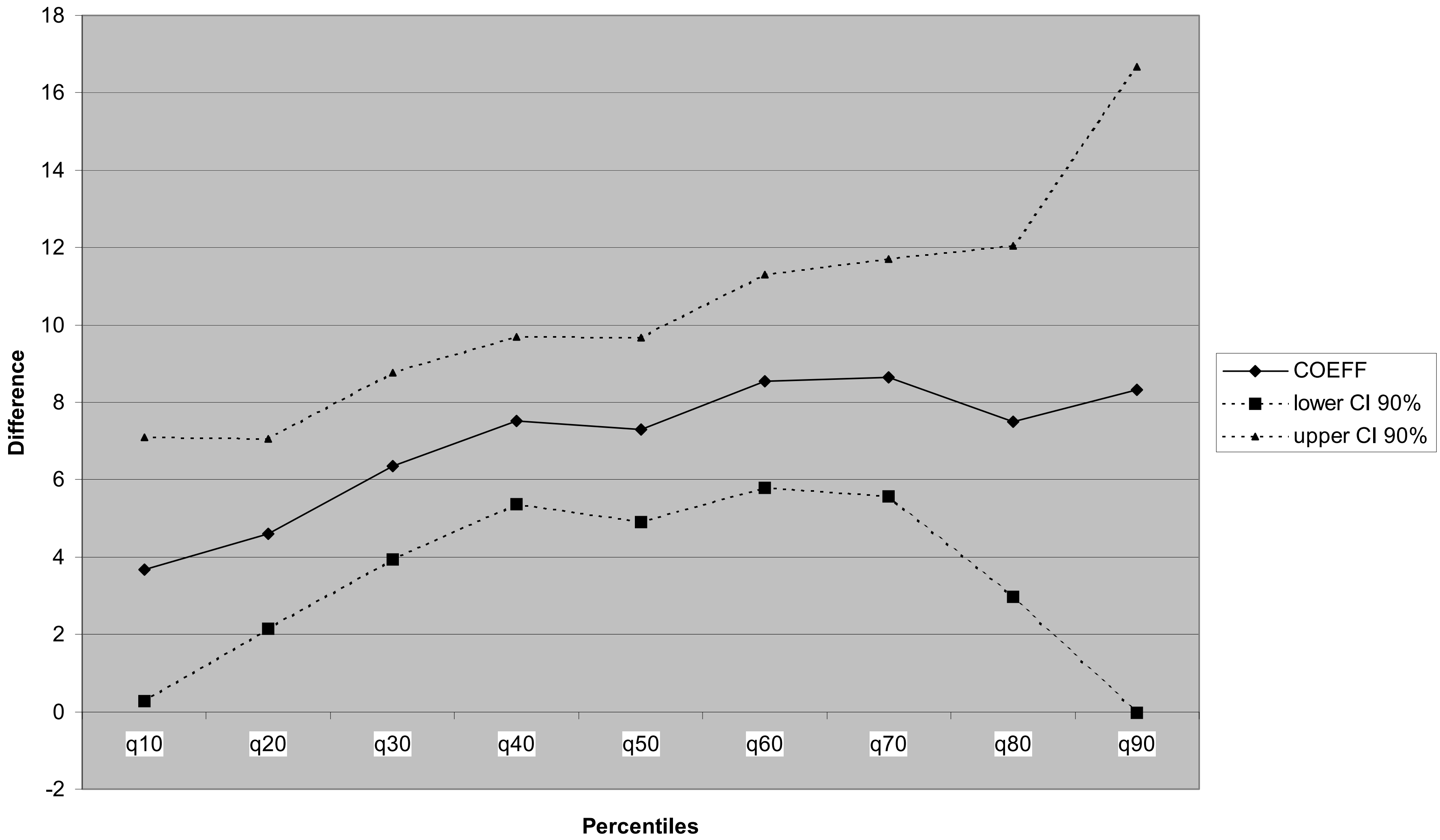


Figure 3: Difference in per capita value of consumption in treatment and control in June 1999 (Nov. 1998 pesos) controlling for covariates

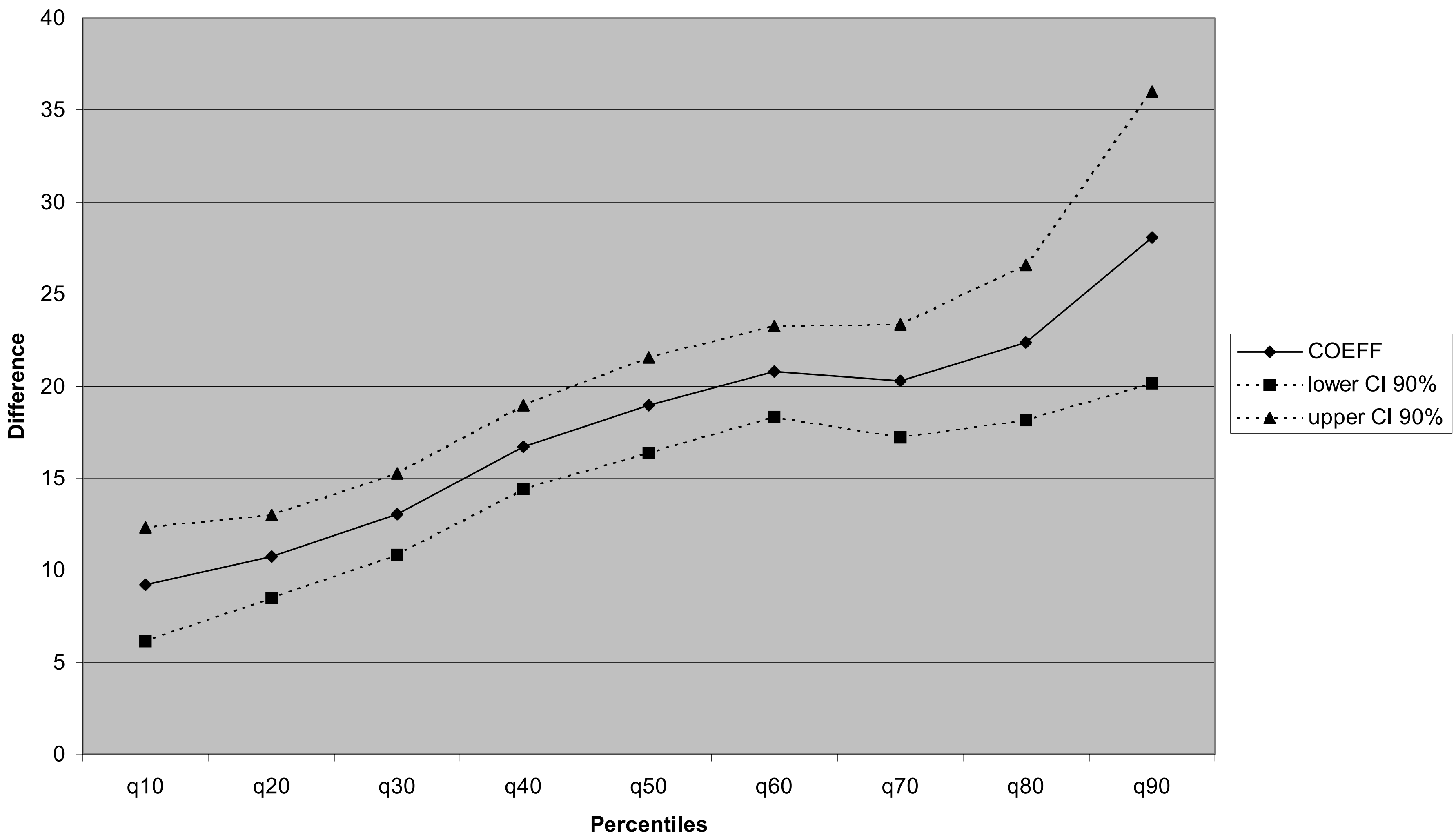


Figure 4: Difference in per capita value of consumption in treatment and control group in November 1999 (Nov. 1998 pesos) controlling for covariates

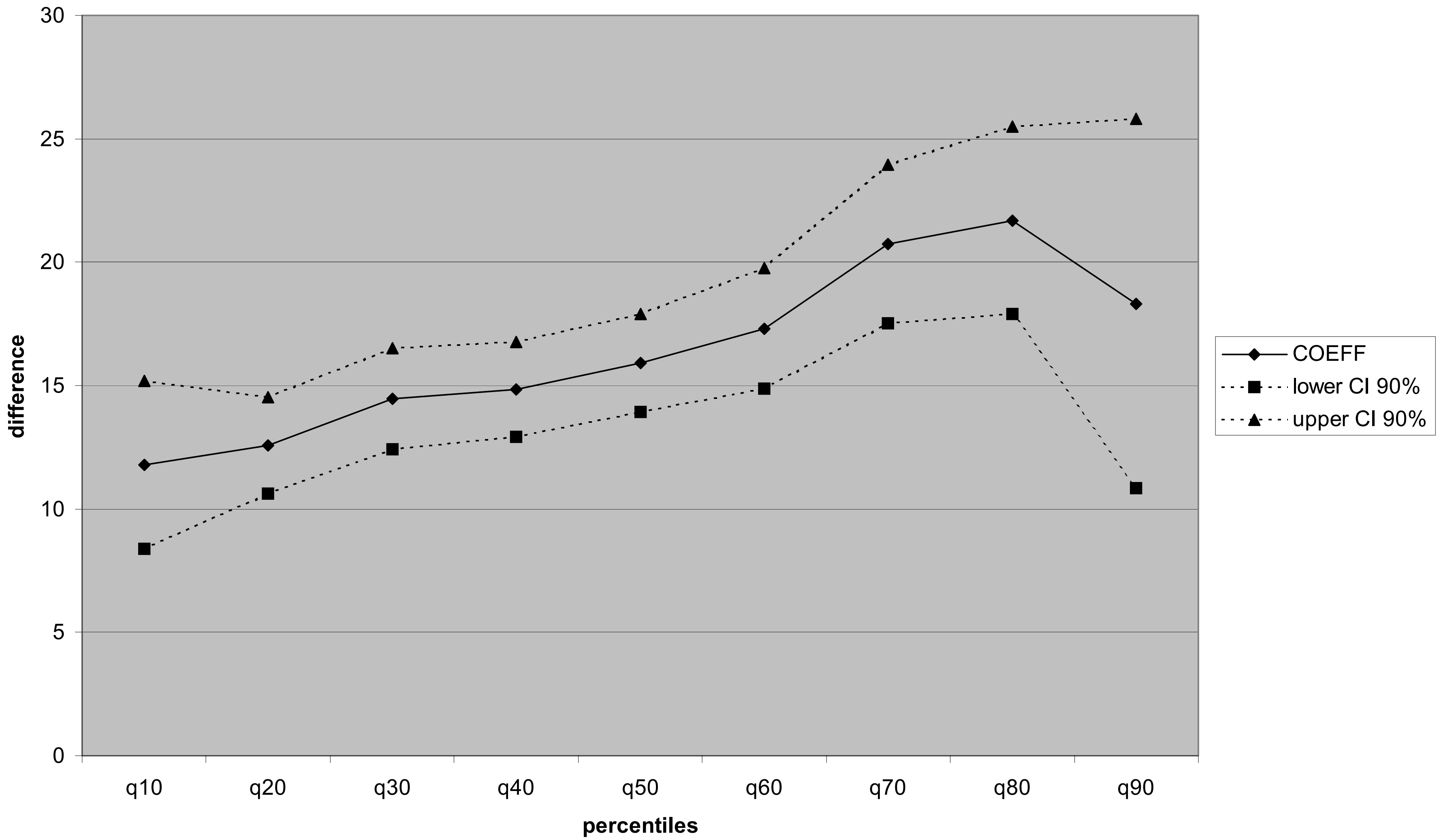


Figure 5: Differences in per capita value of food consumption in treatment and control groups in November 1998 (in Nov. 98 pesos) controlling for covariates

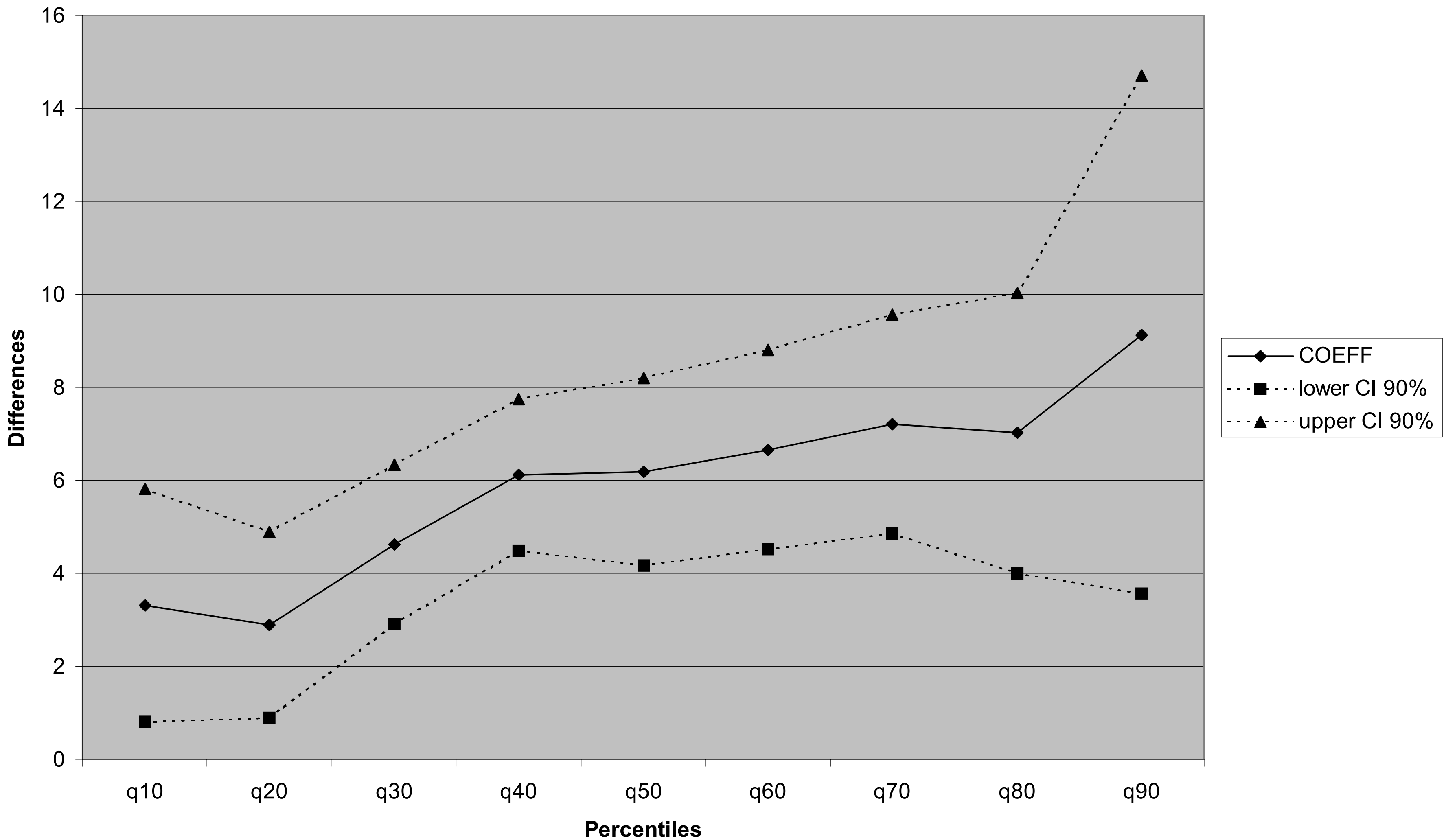


**Figure 6: Difference in per capita value of food consumption in treatment and control groups in June 1999
(in Nov 98 pesos) controlling for covariates**

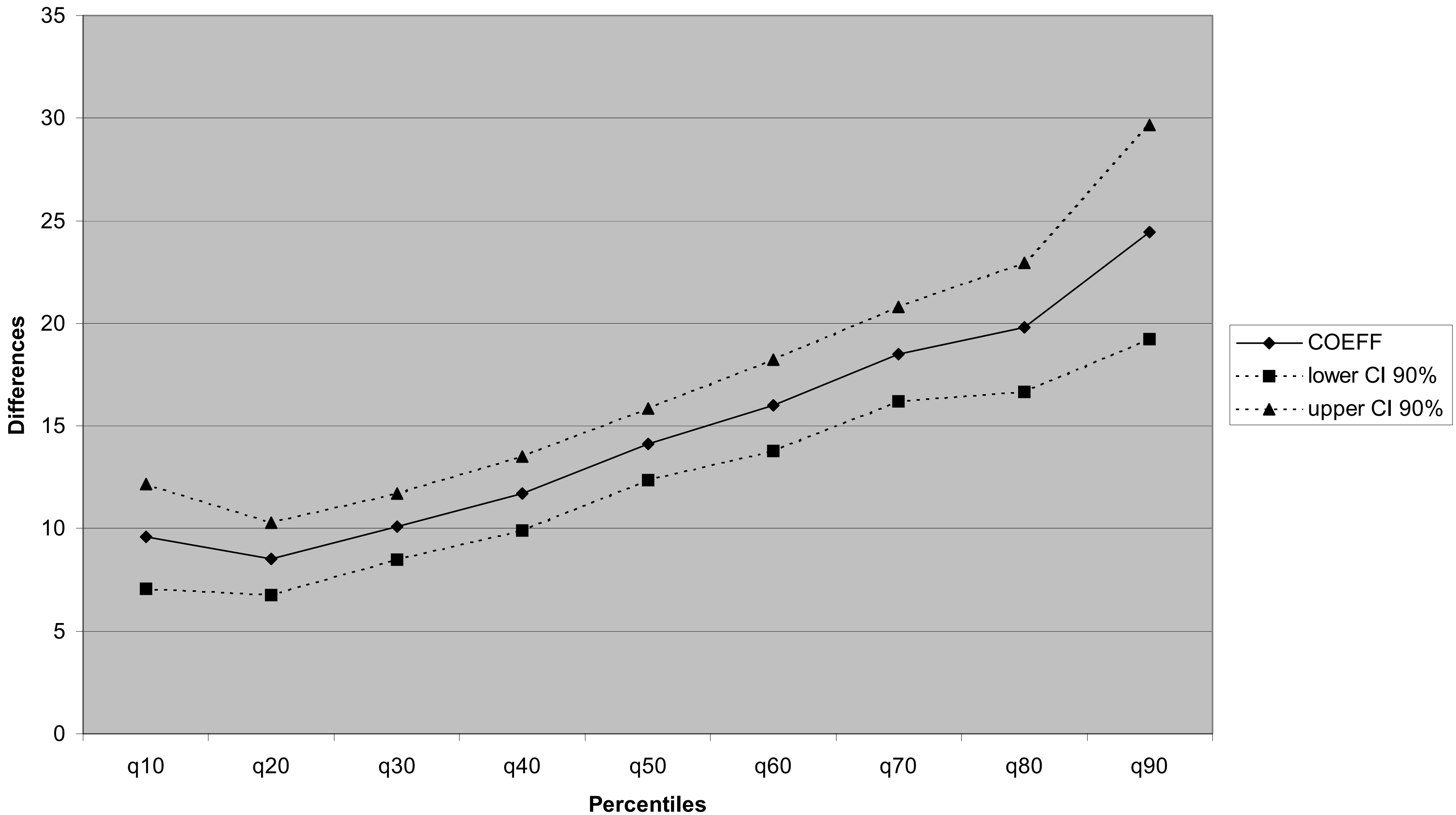


Figure 7: Difference in per capita of food consumption in treatment and control groups in November 1999 (in Nov. 98 pesos) controlling for covariates

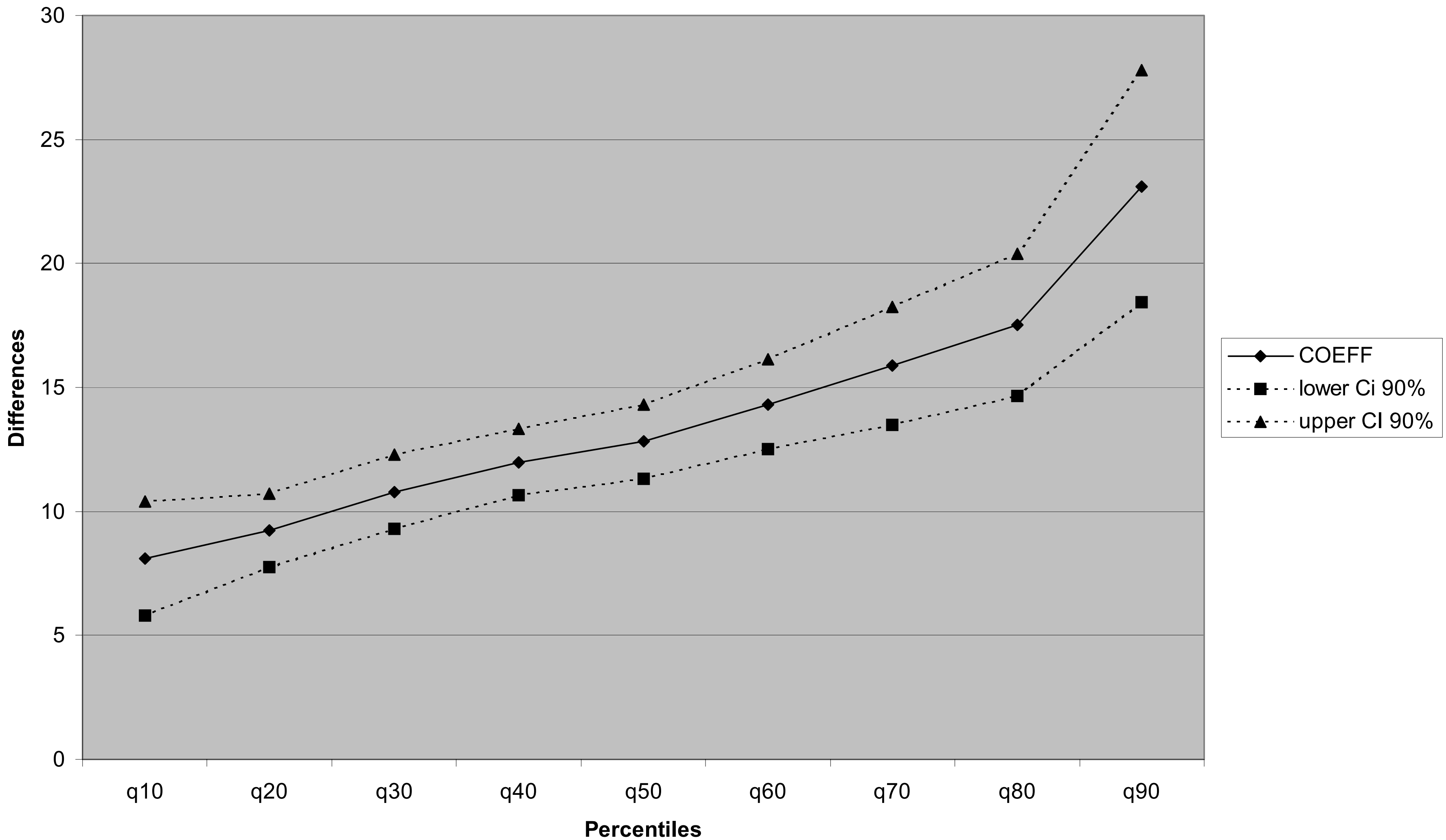


Figure 8: Unobserved difference in per capita value of consumption in the treatment and control group in November 1998 (Nov 1998 pesos) controlling for covariates

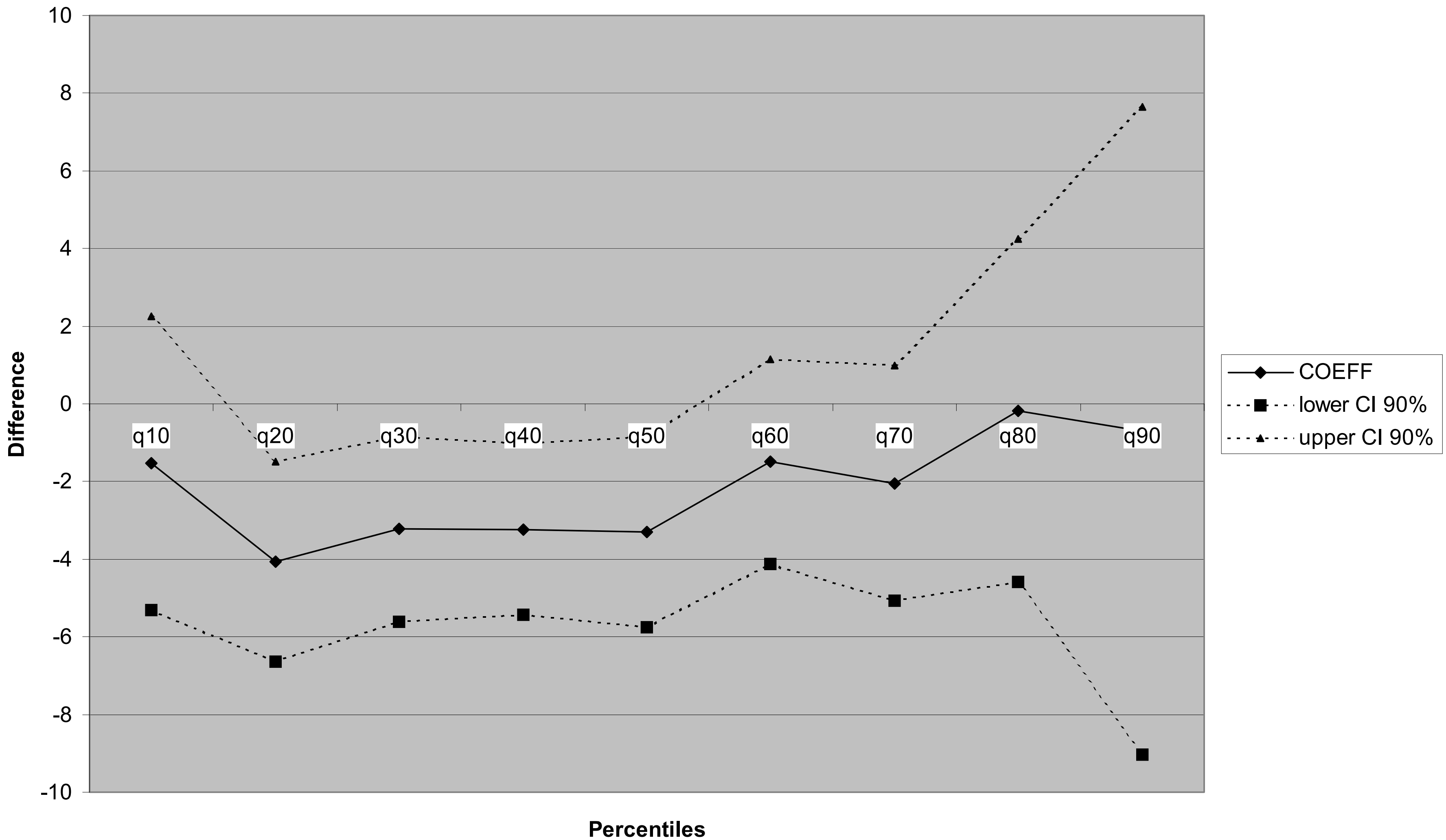


Figure 9: Unobserved difference in per capita value of consumption in treatment and control in June 1999 (Nov. 1998 pesos) controlling for covariates

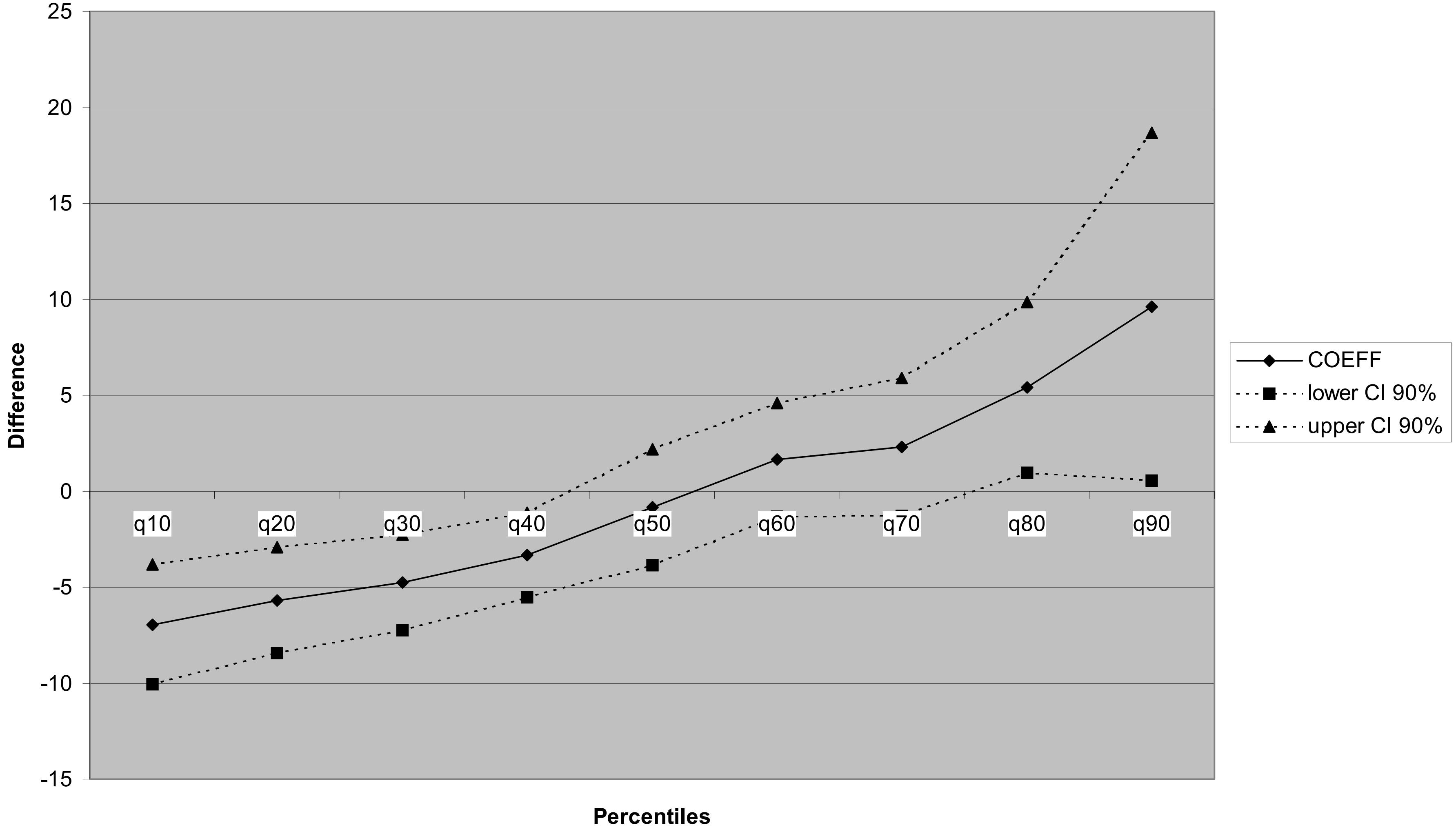


Figure 10: Unobserved difference in per capita value of consumption in treatment and control group in November 1999 (Nov. 1998 pesos) controlling for covariates

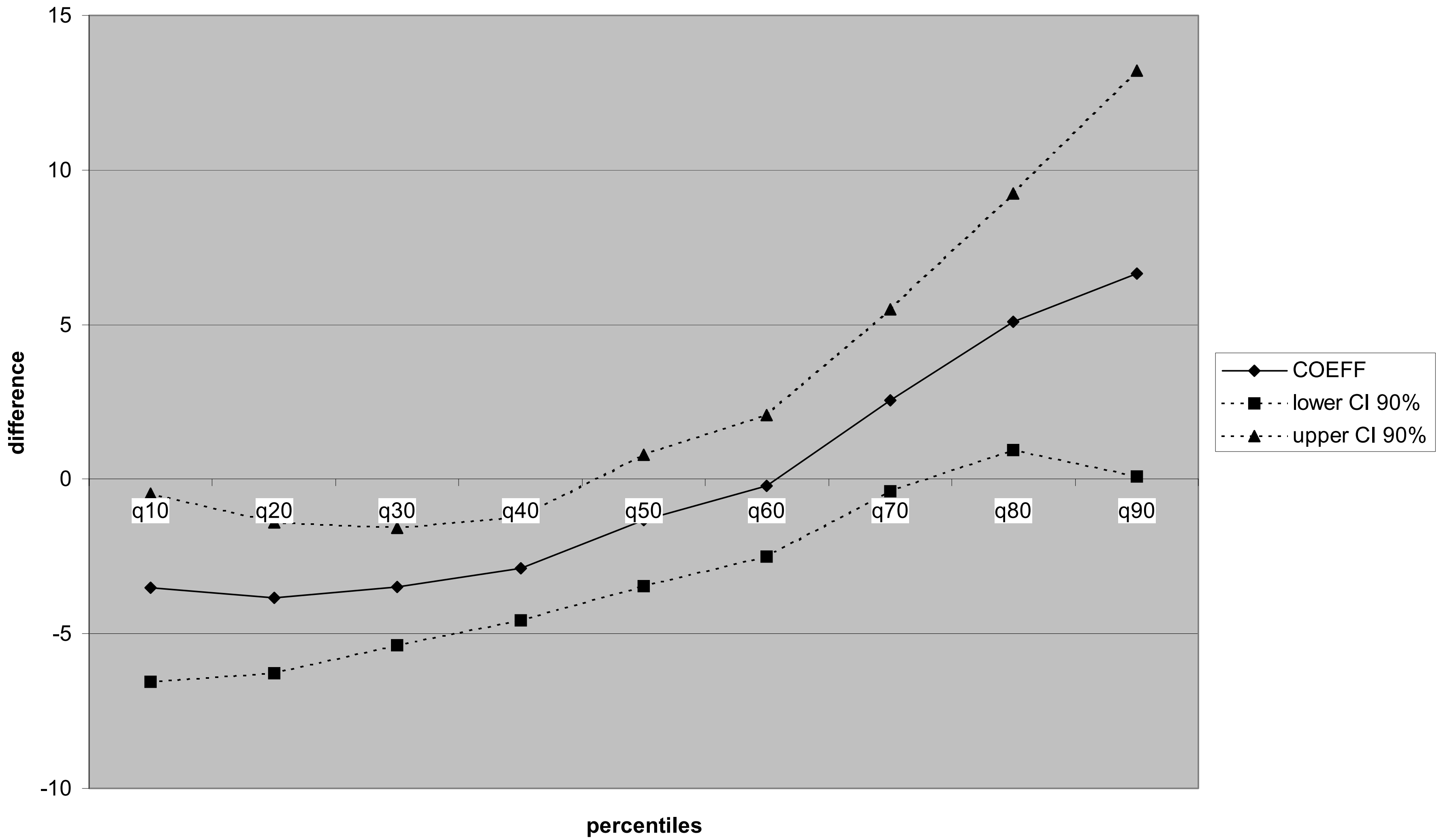


Figure 11: Unobserved differences in per capita value of food consumption in treatment and control groups in November 1998 (in Nov. 98 pesos) controlling for covariates

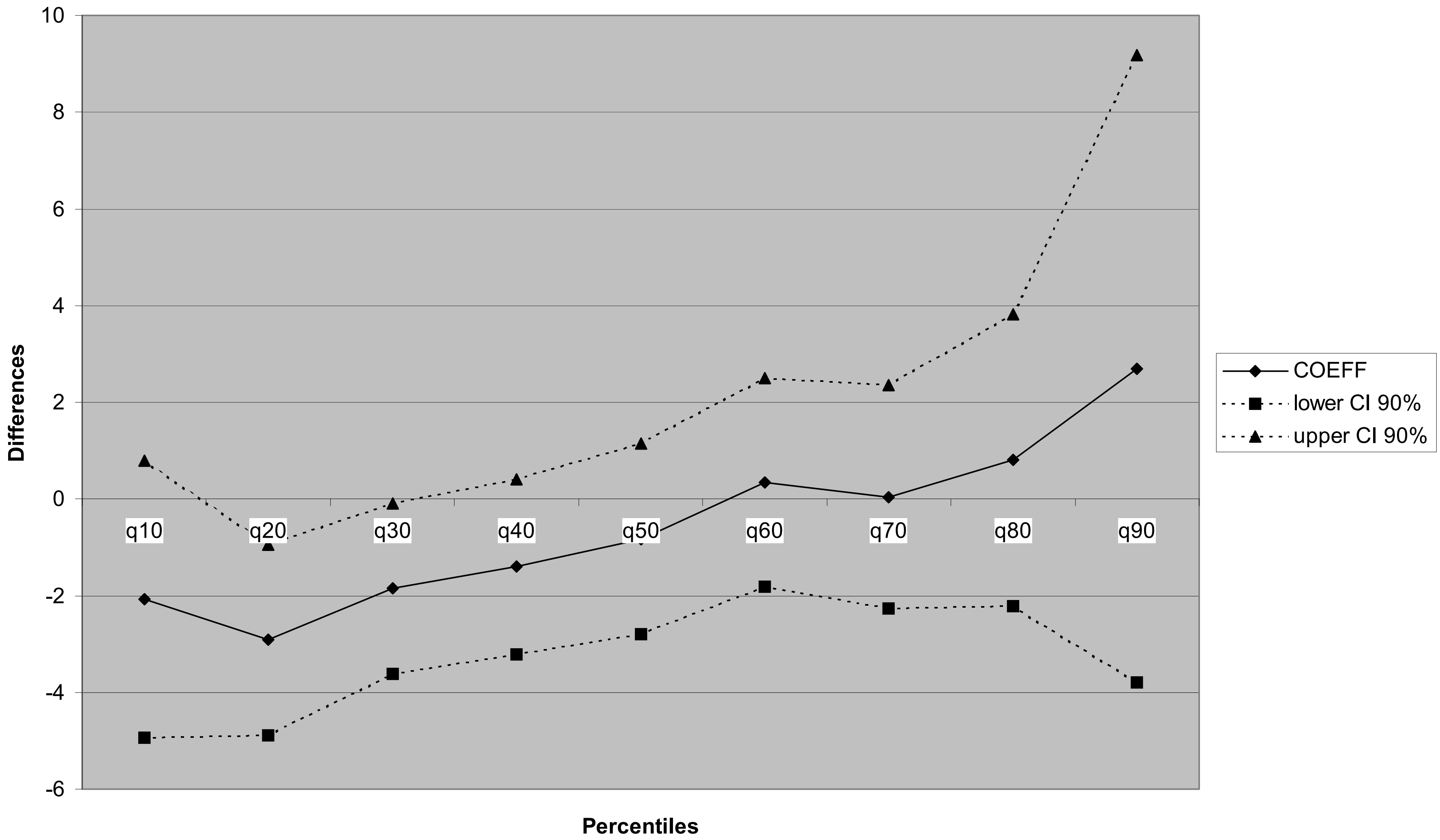


Figure 12: Unobserved difference in per capita value of food consumption in treatment and control groups in June 1999 (in Nov 98 pesos) controlling for covariates

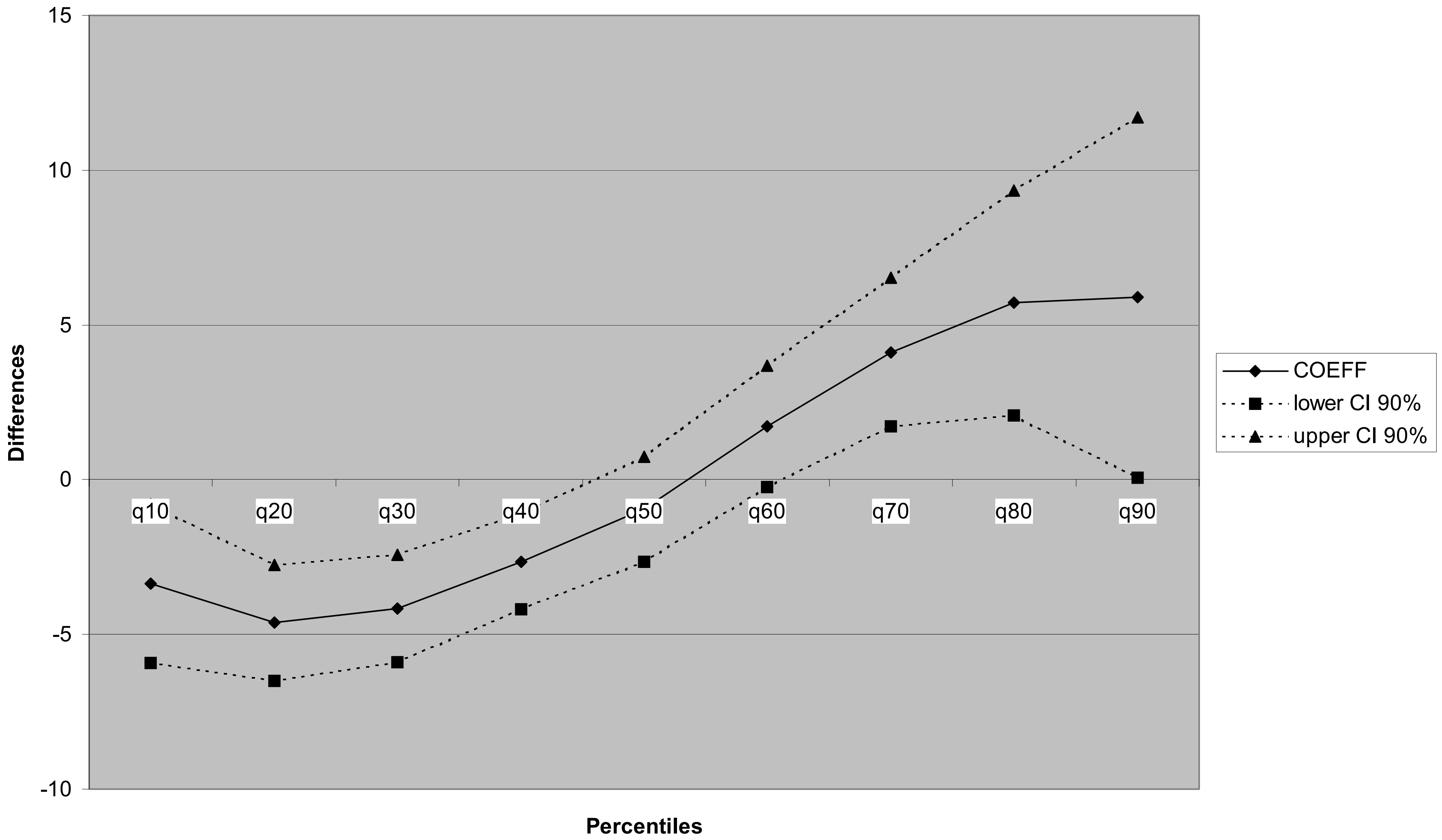


Figure 13: Unobserved difference in per capita of food consumption in treatment and control groups in November 1999 (in Nov. 98 pesos) controlling for covariates

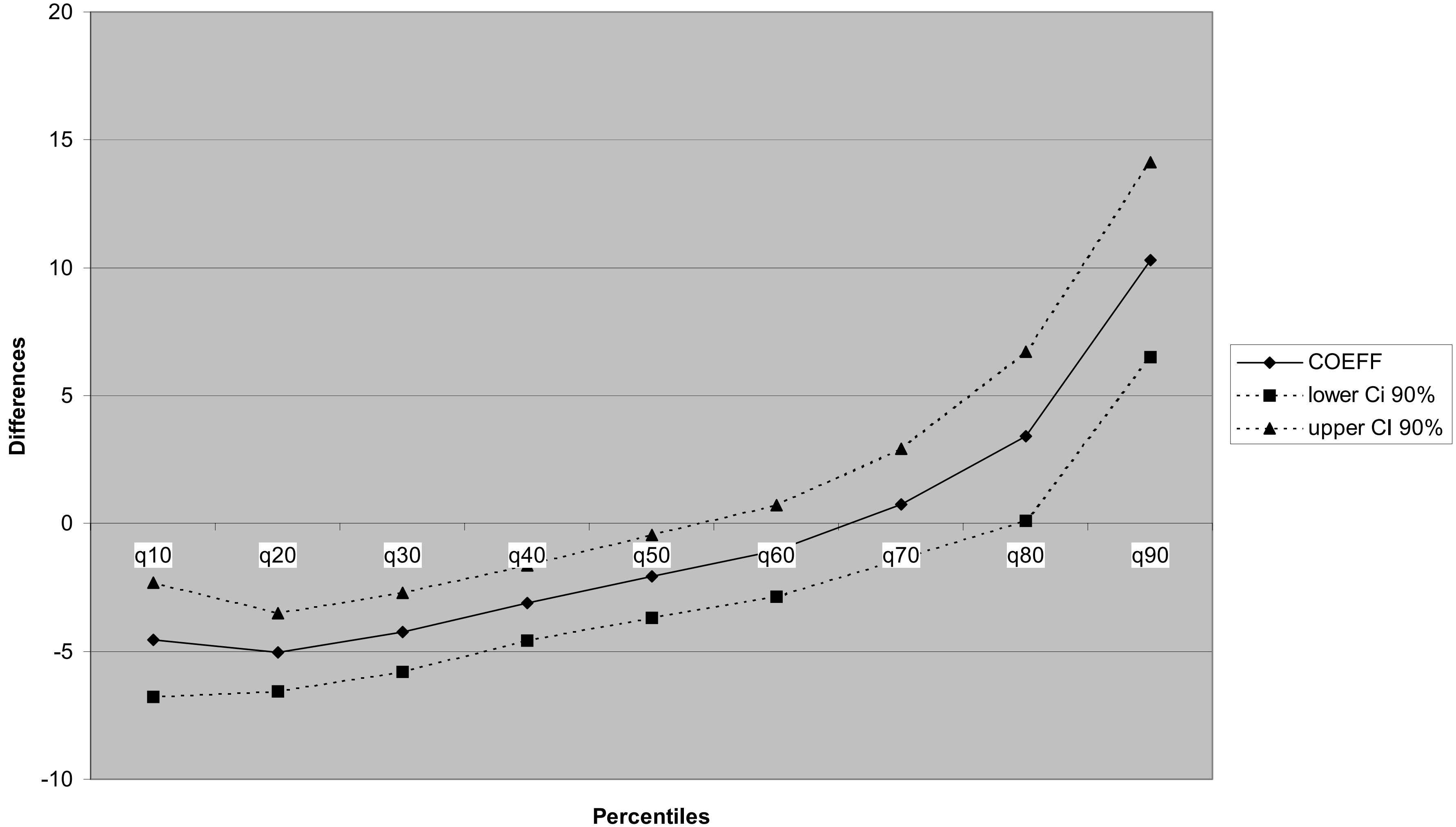


Figure 14: Distribution of impacts on total household consumption under the normality assumption

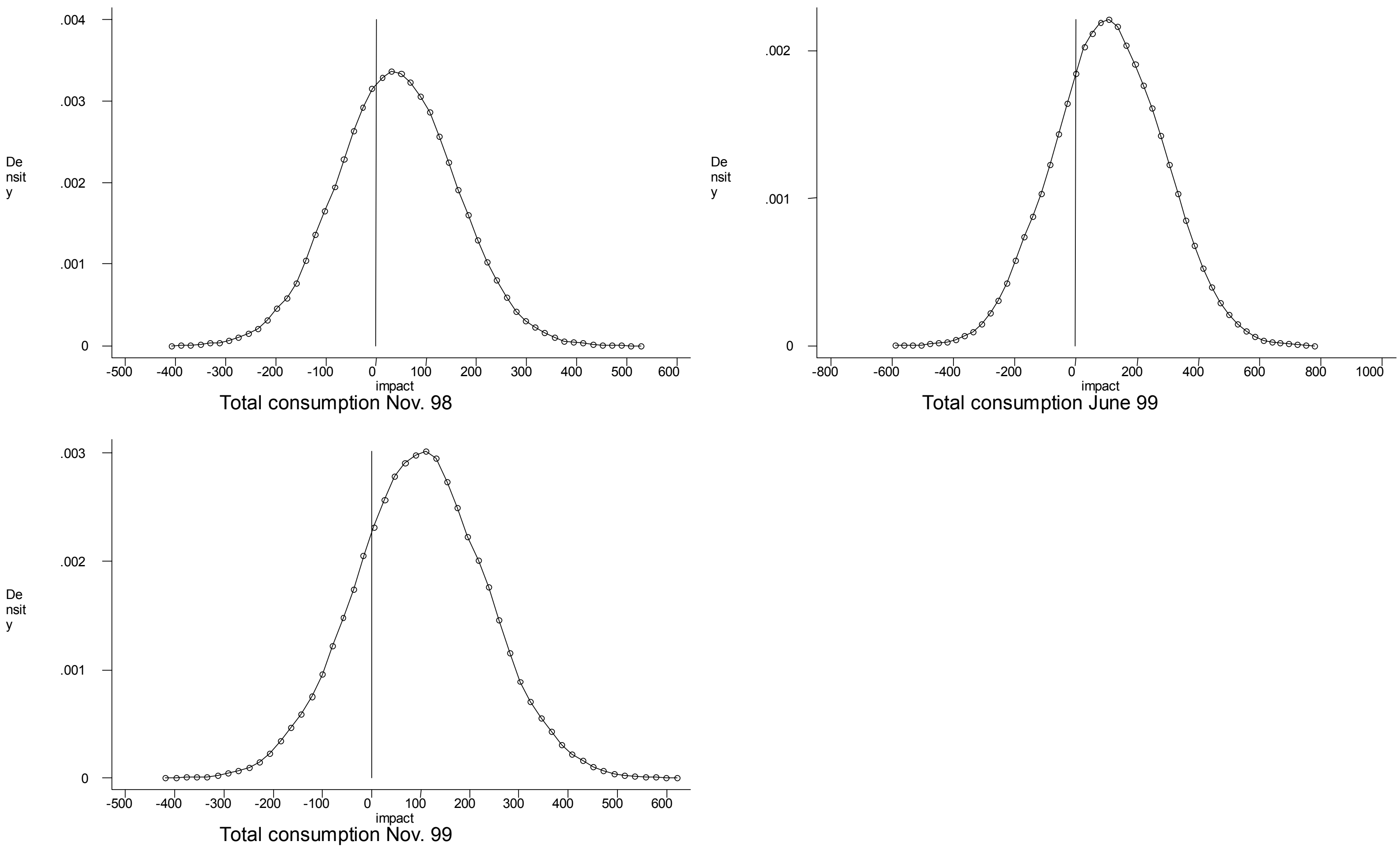


Figure 15: Distribution of impacts on food consumption under the normality assumption

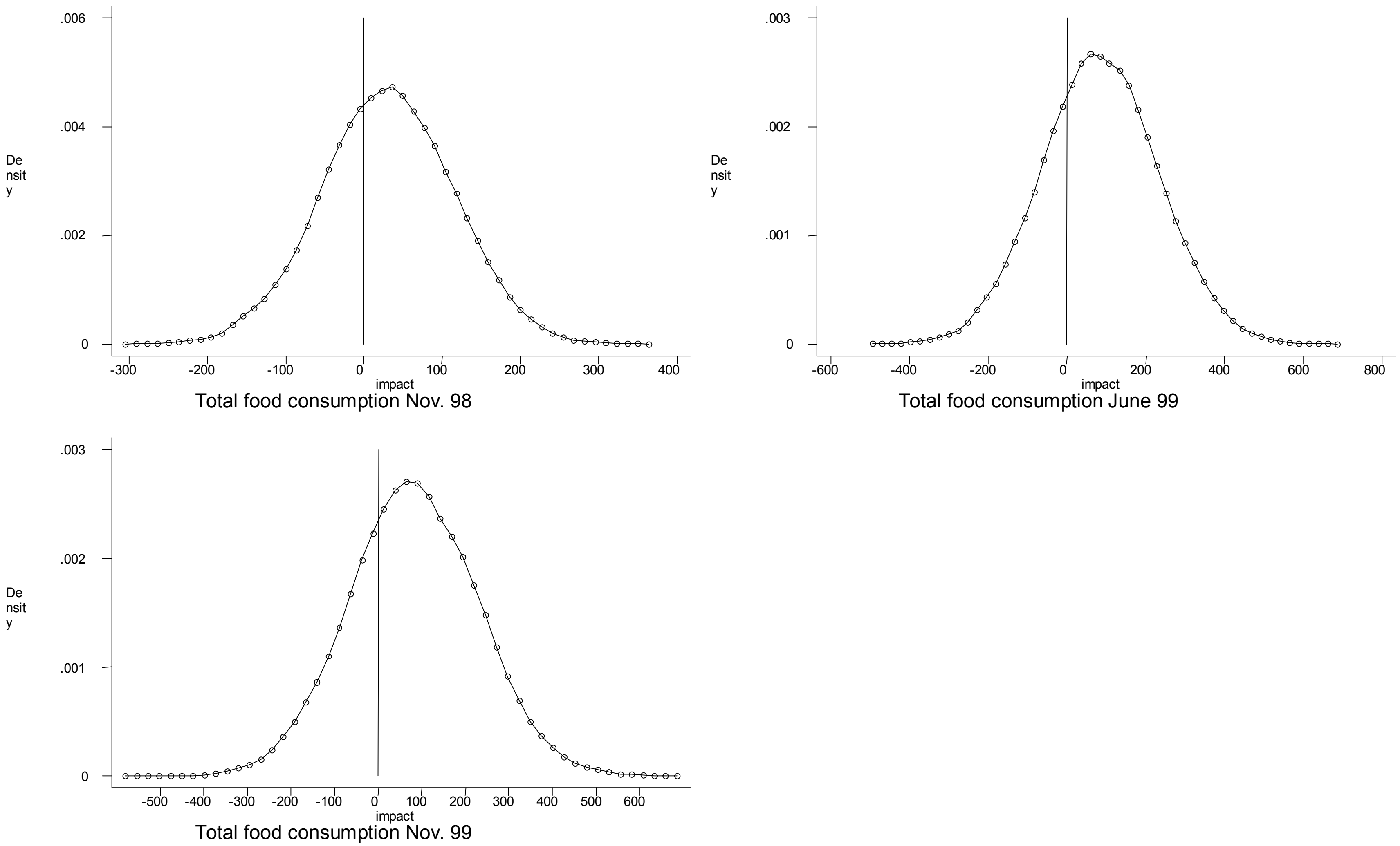
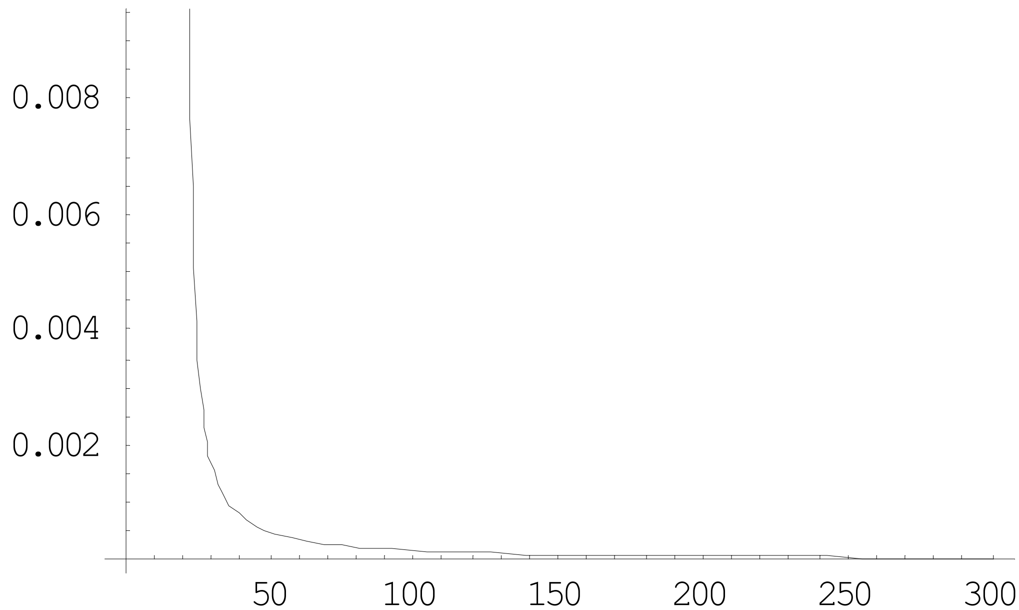
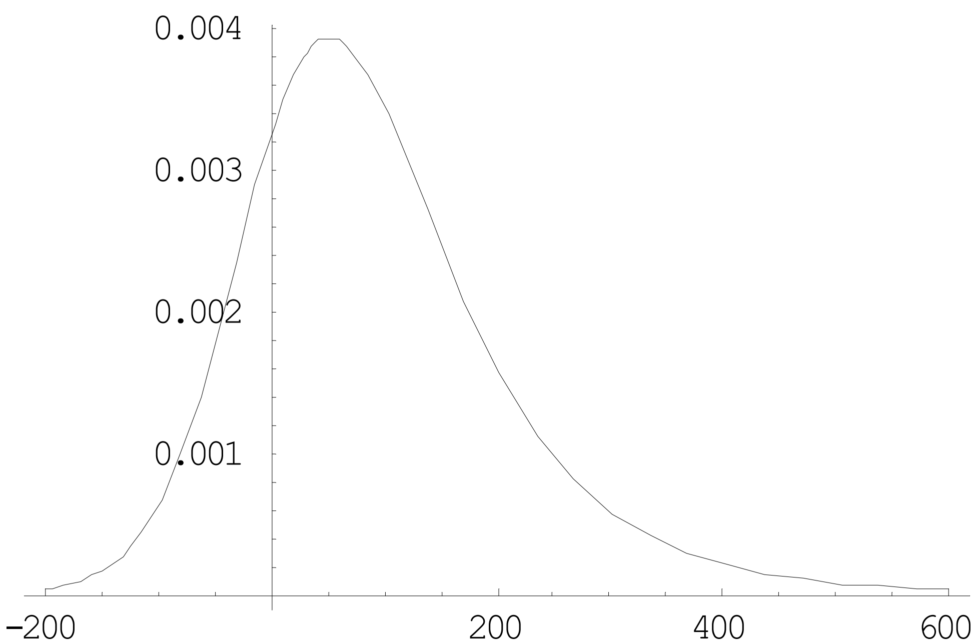


Figure 16: Distribution of impacts on November 1998 household consumption – Gamma distribution.



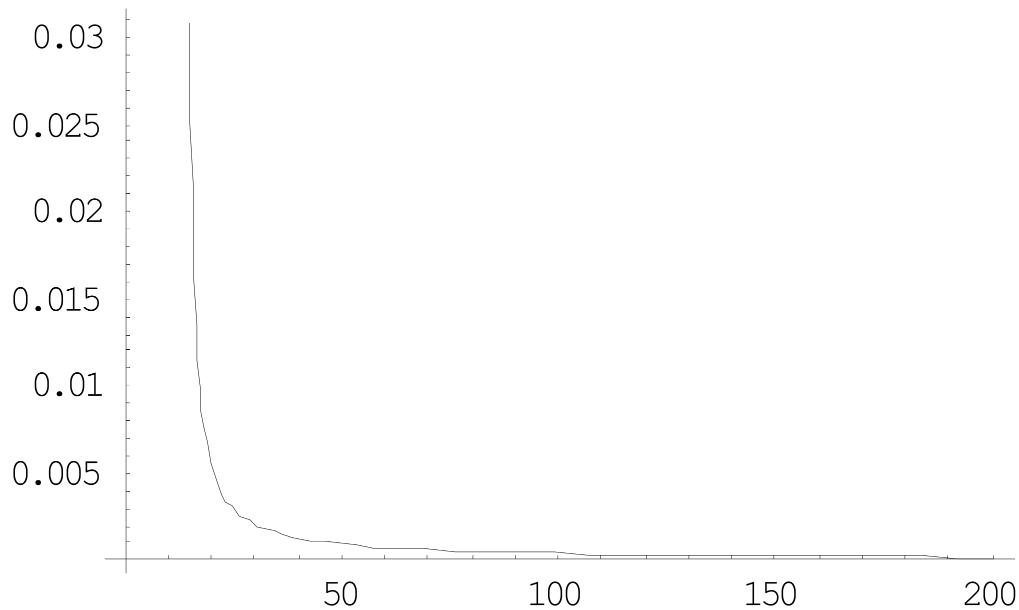
On the x-axis are program impacts in pesos, and on the y-axis the associated frequencies.

Figure 17: Distribution of impacts on November 1999 household consumption – Type IV Pearson distribution.



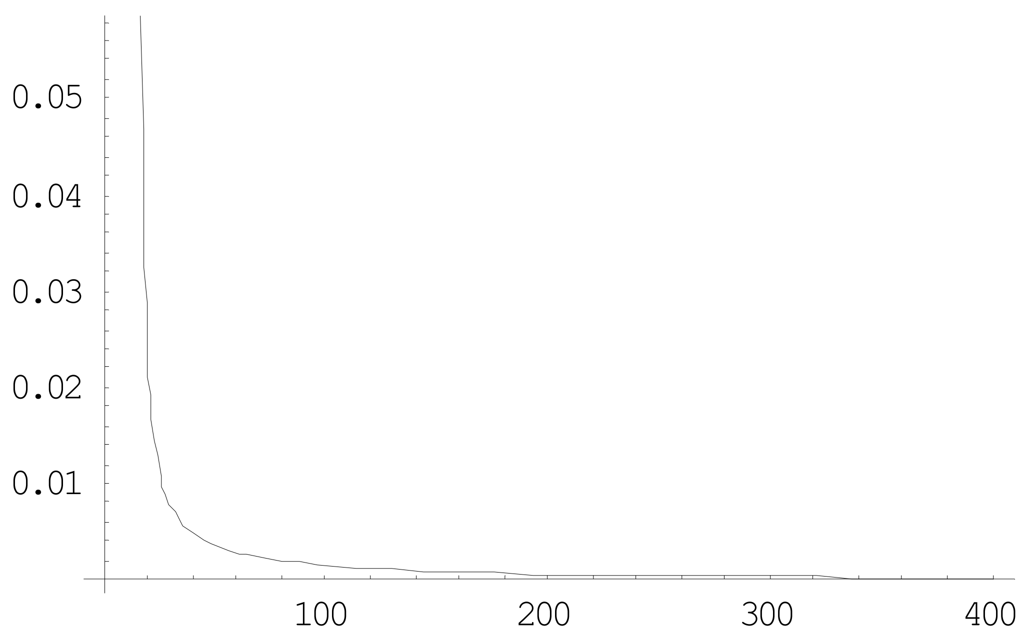
On the x-axis are program impacts in pesos, and on the y-axis the associated frequencies.

Figure 18: Distribution of impacts on June 1999 food expenditures – Gamma distribution.



On the x-axis are program impacts in pesos, and on the y-axis the associated frequencies.

Figure 19: Distribution of impacts on November 1999 food consumption – Gamma distribution.



On the x-axis are program impacts in pesos, and on the y-axis the associated frequencies.

Appendix 3: Data appendix of chapter 2

In most of the study, we use the household-level datasets respectively collected in November 1998, June 1999 and November 1999. We restrict the sample to only include households who are eligible to PROGRESA benefits. The breakdown of the data by round is as follow:

Table 3.1: Number of households by round

	Number of Households
November 1998	18,066
June 1999	17,230
November 1999	16,761
Total	52,057

The program evaluation sample is based on a random allocation of villages into treatment and control groups. The numbers of PROGRESA eligible households in treatment and control groups are given in the following table:

Table 3.2: Number of households by experimental groups

	Number of household
Treatment group	31,910
Control group	20,147

In the section 4, we analyze the heterogeneity of impacts on wealth and nutrition along the program targeting criteria. The two program targeting criteria are found in the October 1997 baseline dataset. Thus, we merge the November 1998, June 1999 and November 1999 data with the October 1997 data. The following table shows that the resulting sample has 50,206 households:

Table 3.3: Number of household in the wealth and nutrition sample of section 4

	Number of households
Resulting sample	50,206
October 1997 only	6,546
Nov. 98-June 99-Nov. 99 only	1,851

In this resulting sample used in section 4, the breakdowns of the data by round and the breakdown of the data by treatment/control group are given in the following tables:

Table 3.4: The wealth and nutrition sample of section 4 by round

	Number of Households
November 1998	17,044
June 1999	16,526
November 1999	16,636
Total	50,206

Table 3.5: The wealth and nutrition sample of section 4 by experimental groups

	Number of household
Treatment group	30,764
Control group	19,442

In addition, in section 4, we analyze the heterogeneity of impacts along the program targeting criteria for children's time allocation. We use a detailed individual-level time allocation module collected in June 1999, that we supplement with the October 1997 data on the targeting indices. In the following table, we show the number of children in this dataset and its distribution in the treatment and control groups:

Table 3.6: The time allocation sample of section 4 by experimental groups

	Number of children
Treatment group	18,628
Control group	11,621
Total	30,249

In the remaining of the data appendix section, we present descriptive statistics for the variables used in the different analyses.

Table 3.7: Descriptive statistics for the wealth and nutrition sample of section 4

Variable	Number of observations	Mean	S.D.
P.C. Value of consumption	47576	193.98	142.07
P.C. Expenditures	47573	155.54	119.18
P.C. Value of food consumed	47580	147.35	109.45
P.C. Food expenditures	47588	109.21	81.67
Treatment indicator	48852	0.61	0.49
Household size	48852	5.94	2.83
# children less than 2 yrs old	48852	0.52	0.79
# of children 3-5	48852	0.56	0.74
# of children 5-10	48852	1.13	1.15
# of boys 11-14	48852	0.35	0.59
# of girls 11-14	48852	0.33	0.58
# of boys 15-19	48852	0.34	0.62
# of girls 15-19	48852	0.33	0.60
# of men 20-34	48852	0.50	0.63
# of women 20-34	48852	0.56	0.61
# of men 35-54	48852	0.45	0.51
# of women 35-54	48852	0.45	0.52
# of men 55 and more	48852	0.27	0.46
# of women 55 and more	48852	0.27	0.47
= 1 if the head of household is an ag-worker	45134	2.11	2.13
=1 if the head of household is a male	48843	0.89	0.31
# years of schooling for the head	48852	2.67	2.68
Head speaks indigenous language	48852	0.38	0.49
Age of head of household	48726	46.71	15.97

Table 3.8: Poverty score and village marginality score for the wealth and nutrition sample of section 4

Variable	Number of observations	Mean	Median	25th percentile	75th percentile
Poverty score	48733	-696	-767	-698	-622
Village marginality score	48852	0.48	-0.14	0.41	1.01

Table 3.9: Descriptive statistics for the time allocation sample if section 4

Variable	Number of observations	Mean	S.D.
=1 if boy	30249	0.51	0.49992
=1 if participates to school	30249	0.43	0.495135
=1 if works in the labor market	30249	0.13	0.336259
=1 if works at home	30249	0.42	0.493271
Hours spent studying	30249	157	193
Hours spent working outside the home	30249	50	147
Hours spent working in the home	30249	65	125
=1 if 8 yrs old	30249	0.10	0.30
=1 if 9 yrs old	30249	0.10	0.30
=1 if 10 yrs old	30249	0.09	0.29
=1 if 11 yrs old	30249	0.10	0.30
=1 if 12 yrs old	30249	0.10	0.30
=1 if 13 yrs old	30249	0.10	0.29
=1 if 14 yrs old	30249	0.09	0.29
=1 if 15 yrs old	30249	0.09	0.28
=1 if 16 yrs old	30249	0.09	0.28
=1 if 17 yrs old	30249	0.07	0.26
=1 if 18 yrs old	30249	0.07	0.25
# of children below age 4	30249	0.99	1.13
# of children 5-10	30249	1.68	1.19
# of boys 11-14	30249	0.69	0.75
# of girls 11-14	30249	0.66	0.74
# of boys 15-19	30249	0.66	0.78
# of girls 15-19	30249	0.62	0.77
# of men 20-34	30249	0.45	0.66
# of women 20-34	30249	0.52	0.63
# of men 35-54	30249	0.66	0.49
# of women 35-54	30249	0.67	0.50
# of men 55 and more	30249	0.21	0.42
# of women 55 and more	30249	0.17	0.39
# of yrs of schooling for the head of household	29879	2.56	2.46
# of yrs of schooling for the spouse	30247	2.51	2.54
=1 if the head is a men	29879	0.93	0.26
Age of the head	30245	45.7	11.5
Age of the spouse	30245	39.1	11.2
=1 if head speaks an indigenous language	30249	0.38	0.49

Table 3.10: Poverty score and village marginality score for the time allocation sample of section 4

Variable	Number of observations	Mean	Median	25th percentile	75th percentile
Poverty score	30174	-662	-739	-667	-587
Village marginality score	30249	0.52	-0.09	0.44	1.03

Table 3.11: Descriptive statistics for the sample used in all sections but section 4

	Number of observations	Mean	S.D.
P.C. Value of consumption	50577	195	143
P.C. Expenditures	50574	156	120
P.C. Value of food consumed	50576	148	110
P.C. Food expenditures	50580	110	83
Value of consumption	51017	982	546
Expenditures	51017	775	435
Value of food consumed	51023	745	418
Food expenditures	51025	541	297
Treatment indicator	52057	0.61	0.49
Household size	51597	5.9	2.7

Appendix 4: Deriving moments of a distribution from deconvolution

Let *X* and *Y* be two independent random variables and let *Z* be the convolution of *X* and *Y*. Suppose the first four moments of *Z* and *Y* are known. The objective is to derive the first four moments of *X* as a function of the first four moments of *Z* and *Y*.

Let μ_X, μ_Y and μ_Z be the first moment of *X*, *Y* and *Z*, such that:

$$\mu_X = E(X),$$
$$\mu_Y = E(Y),$$
$$\mu_Z = E(Z).$$

Let μ_{2X}, μ_{2Y} and μ_{2Z} be the second moments of *X*, *Y* and *Z* about their means, such that:

$$\mu_{2X} = E((X - \mu_X)^2),$$
$$\mu_{2Y} = E((Y - \mu_Y)^2),$$
$$\mu_{2Z} = E((Z - \mu_z)^2).$$

Let μ_{3X}, μ_{3Y} and μ_{3Z} be the third moments of *X*, *Y* and *Z* about their means, such that:

$$\mu_{3X} = E((X - \mu_X)^3),$$
$$\mu_{3Y} = E((Y - \mu_Y)^3),$$
$$\mu_{3Z} = E((Z - \mu_z)^3).$$

Let μ_{4X}, μ_{4Y} and μ_{4Z} be the fourth moments of *X*, *Y* and *Z* about their means, such that:

$$\mu_{4X} = E((X - \mu_X)^4),$$
$$\mu_{4Y} = E((Y - \mu_Y)^4),$$
$$\mu_{4Z} = E((Z - \mu_z)^4).$$

Mean of X.

If $Z = X + Y, \quad X \perp Y$, then :
$\mu_Z = E(Z) = E(X+Y) = E(X) + E(Y).$

Thus, $\mu_X = E(X) = \mu_Z - \mu_Y$.

Variance of X.

If $Z = X + Y, \quad X \perp Y$, then :
$\mu_{2Z} = E((Z-\mu_Z)^2) = E((X+Y-\mu_X-\mu_Y)^2) = E(((X-\mu_X)+(Y-\mu_Y))^2),$
$\mu_{2Z} = \mu_{2X} + \mu_{2Y} + 2\,E(X-\mu_X)\,E(Y-\mu_Y) = \mu_{2X} + \mu_{2Y}.$

Thus, $\mu_{2X} = \mu_{2Z} - \mu_{2Y}$.

Third moment of X about the mean.

If $Z = X + Y, \quad X \perp Y$, then :
$\mu_{3Z} = E((Z-\mu_Z)^3) = E((X+Y-\mu_X-\mu_Y)^3) = E(((X-\mu_X)+(Y-\mu_Y))^3),$
$\mu_{3Z} = \mu_{3X} + \mu_{3Y} + 3\,E((X-\mu_X)^2)\,E(Y-\mu_Y) + 3E((Y-\mu_Y)^2)\,E(X-\mu_X),$
$\mu_{3Z} = \mu_{3X} + \mu_{3Y}.$

Thus, $\mu_{3X} = \mu_{3Z} - \mu_{3Y}$.

Fourth moment of X about the mean.

If $Z = X + Y, \quad X \perp Y$, then :
$\mu_{4Z} = E((Z-\mu_Z)^4) = E((X+Y-\mu_X-\mu_Y)^4) = E(((X-\mu_X)+(Y-\mu_Y))^4),$
$\mu_{4Z} = \mu_{4X} + \mu_{4Y} + 4\,E((X-\mu_X)^3)\,E(Y-\mu_Y) + 4\,E((Y-\mu_Y)^3)\,E(X-\mu_X)$
$+6\,E((X-\mu_X)^2)\,E((Y-\mu_Y)^2),$
$\mu_{4Z} = \mu_{4X} + \mu_{4Y} + 6\mu_{2X}\,\mu_{2Y}.$

Thus, $\mu_{4X} = \mu_{4Z} - \mu_{4Y} - 6\mu_{2X}\,\mu_{2Y}$.

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