Effects of a Reform in the Rural Pension System on Intra-Household Allocations in Brazil*

Vladimir Ponczek†

Princeton University

April 9, 2006

Preliminary and incomplete draft. Please do not quote without permission. Comments welcome.

Abstract

In this paper, we present evidences that the reform in the rural pension system in Brazil occurred in 1991 had significant impacts on schooling and health indicators. Moreover, this research uses the reform to investigate the unitary model of household allocation by testing if there were uneven impacts on those indicators depending on the recipient gender. The main conclusions of the paper are that indeed the reform had significantly positive effects on the outcomes of interest, especially on those co-residing with a male pensioner, indicating that the unitary model is not a well-specified framework to understand family allocation decisions. The highest impacts were on school attendance for boys, literacy for girls and illness for middle-age people. We explore a collective model as defined by [Chiappori, 1992] as one possible alternative representation for the decision-making process of the poor rural Brazilian families. In the cooperative Nash equilibrium, the reform effects can be divided into two pieces: a direct income effect and bargaining power effect. The data support the existence of these two different effects. [JEL=O15, I12, I28]

*I would like to thank Anne Case for encouragement and extraordinary guidance. I also thank Fernando Botelho, Carlos Bozzoli, Chris Paxson, Nisreen Salti, Luke Williard, and participants of the development lunch seminar at Princeton University for comments and critiques. All remaining mistakes are mine

†346 Wallace Hall. Princeton, NJ 08544 vponczek@princeton.edu

1 Introduction

In the past decades, the economic literature has shown evidences that the household cannot be characterized as a unit with individuals sharing the same preferences or pooling their resources. Within the family unit, different individuals may have different concerns about how to spend their total family income and the decision-making process of the resource allocation may be affect by the difference in preferences. The problems lead by this faulty family characterization inspired this paper, which intend to take advantage of variation in social security system rules in Brazil to estimate the impact of a change in family income on socio-economic outcomes.

Using the unitary model of the household as a guideline for policy prescriptions may lead to some failures. The effect of public transfers may differ depending on the identity of the income recipient. Therefore, targeting transfers to the household may not result in the desired consequences, given that transfers directed to the head of the family, to the spouse or to elderly people may have different impacts over the family. For example, if the receiver of the transfer were an elderly person, there could be a larger fraction of the transfer allocated to health care. In the same way, if the beneficiary were the mother instead of the father, the income augment might cause a reduction on her labor supply, once she may now want to allocate more of her time raising the children. Hence, an increase in the family income may have uneven impacts on different members of the household depending on the characteristics of the transfer recipient.

In many developing countries, the pension system is the most important source of public transfers for poor families. Therefore, a major change in the rules of the social security system in a continental country is an excellent opportunity to understand how an unanticipated increase on income is allocated within household members. The Brazilian social security reform occurred in 1991 will allow us to measure different impacts of an unanticipated increase in income on several social-economic variables depending on the characteristics of the pensioner.

To many families, especially in rural Brazil, pensions are the only stable source of income, even for families that the pensioner is not the head. Indeed, a considerable number of household units is formed by several people including one or more pensioners, one or more adults and children. A change in the eligibility rules as well as in the pension values may modify the balance of power over

the allocation decisions towards the pensioner preferences. Therefore, if the pensioner's preferences over education, health, and leisure are different from other family members' preferences, the modification in the social security system may cause a massive change in intra-household allocations. For instance, if female pensioners concern more about children education, a change in the pension rules that allows spouses to also be a recipient may cause an important boost in schooling for children living in an elderly female. In this case, the impact would be even higher than one caused if the beneficiary were a male. Thus, estimating uneven impacts of the reform on different family members creates a possibility to better understand how intra-household allocations are designed. In this research, we plan to concentrate the analysis of the impact of the pension system rules on two different outcomes: education and health. Both issues are important to the design of policies with the goal of enhancing the quality of live of poor families and reducing inequality in developing countries.

The empirical investigation about the unitary model of intra-household allocation is a recurrent topic in the literature. A classical study of [Thomas, 1990] tests, also using Brazilian data, whether mother's unearned income has different impact on family health than the father's income. He found that for child survival that is true, rejecting the *common preference or neoclassical* model, which underlies Becker's seminal works[1] about household formation. He also observed that mothers privilege their daughters, while fathers their sons in terms of nutritional intakes. [Duflo, 2000] uses the South African social pension program to study whether the impact of cash transfer on child nutritional status is affected by the gender of the recipient. The author claims that pensions received by women had a large impact on the anthropometrics status of girls but little effect on boys. However, she could not find similar effect for pensions received by men. [Emerson and Souza, 1997] study the existence of uneven impacts of parent's social-economic characteristics on school attendance and child labor for boys and girls. Their results show that the father's characteristics such as years of schooling, non-labor income, age he first began working have a greater impact on sons than on daughters and the opposite occurred with the mother's characteristics, i.e., they are more effective on daughters than on sons. Moreover, the authors found that both non-labor income of mothers

[1] see [Becker, 1964], [Becker, 1974] and [Becker, 1981]

and fathers affect more the son's schooling attendance than they affect daughter's attendance.

[Delgado, 1997] and [Delgado, 1999] descriptively reports the impacts of the reform on social-economic indicators such as poverty, personal and regional income distribution and found a significant reduction of poverty and income redistribution in favor of those families affected by the reform.[de Carvalho Filho, 2000] and [de Carvalho Filho, 2001] also focus on the reform occurred in 1991. He studies the impact of the income variation caused by the social security changes on schooling decisions, child labor, retirement decisions and labor market responses. In the first paper, the author found a positive impact in the reduction of child labor for both girls and boys and positive impact on schooling enrollment for both genders. In the second paper, he also observed a reduction in the retirement age among those affected by the reform.

Summarizing, the restructuring in the Brazilian social security system presents itself as a valuable opportunity to understand the individual and family responses to unanticipated income shocks. This study intends to explore exogenous variation in social security income at the household level, caused by the pension system reform for rural workers in Brazil, to estimate the impact of increased incomes on the cited social outcomes (education and health). Our primary objective in this paper is to test whether an extra amount of family income coming from the social security benefits creates uneven impacts on its different members depending on the characteristics of the beneficiary.

This paper is organized as follows: Section 2 discusses basic models of intra-household allocations. Section 3 talks about the dataset used in this paper. Section 4 explains the details of the reform. Identification strategies are discussed in section 5. The results are shown in section 6. Section 7 concludes.

2 Basic Model of Intra-Household Allocation

In this section, we briefly analyze the main differences and implications of *non*-cooperative and cooperative equilibria of the resource distribution within a household unit[2]. The analysis focuses on the behavior of adult members of a family with children. Each adult i has preferences represented by the utility function $U_i = U_i(x_i, Y)$, where x_i is the consumption of a private good and Y is the

[2]More details can be found in [Ermisch, 2003]

amount of a public good, which represents benefits for the children, but could also denote the consumption of a particular good for any person in the family that benefits all other members, such as health. Each adult can make contributions to the public good y_i, so $x_i = I_i - y_i$ and $Y = \sum_i y_i/p$, where I_i is adult i income and p is the relative price of the public with respect to the private good. Non-negative consumption constraints hold. In a Nash equilibrium, each adult decides her (his) contribution to the public good, given the other contributions. Therefore, adult i:

$$\max_{y_i} U_i(I_i - y_i, \sum_i y_i/p)$$
$$s.\ to\ y_i \geq 0$$

If the members of the household cannot cooperate with each other, the solution implies that for each i:

$$\frac{\partial U_i(x_i^*, Y^*)/\partial Y}{\partial U_i(x_i^*, Y^*)/\partial x} \leq p$$

The inequality above holds with equality if the adult contribution is positive for all adults, generating a system of n equations in $y_i's$, which represents their strategies and its solution constitutes a Nash equilibrium. In this case, the amount of public good provided depends only on the total household income. The distribution of income within the household does not interfere the choice of the children benefits. This result is general, but can be easily shown, assuming a particular utility function: $U_i = \alpha_i \ln(x_i) + (1 - \alpha_i) \ln(Y)$. With interior solutions, each adult will provide:

$$y_i = (1 - \alpha_i)I_i - \alpha_i \sum_{-i} y_{-i}$$

The total supply of the public good in a Nash equilibrium (Y^N) is:

$$Y^N = \frac{\left(\prod_{i=1}^{n}(1 - \alpha_i)\right)\left(\sum_{i=1}^{n} I_i\right)}{detA}$$

where *det A* is the determinant of the following matrix A:

$$A = \begin{vmatrix} 1 & \alpha_1 & \dots & \alpha_1 \\ \alpha_2 & 1 & \dots & \alpha_2 \\ \vdots & \vdots & \ddots & \vdots \\ \alpha_n & \dots & \dots & 1 \end{vmatrix}$$

Therefore, a *non*-cooperative Nash equilibrium with an **interior solution** has the same characteristics of a dictatorial or a unitary household allocation model where redistribution of income among members has no effect on the provision of the public good.

On the other hand, in a cooperative Nash equilibrium, adults maximize their utility function constrained by given levels of utility achieved by the other adult members. Therefore, the equilibrium should be *Pareto-Efficient*, in the sense that in the equilibrium allocation of public and private goods, no adult can be better off with out make at least one of the others worse off. This is called the "collective" model by [Chiappori, 1992]. Therefore, each adult:

$$\begin{aligned} \max_{Y,x_i} U_i(x_i, Y) \\ s.\ to\ y_i \geq 0 \\ \sum_i I_i = \sum_i x_i + pY \\ U_j(x_j, Y) \geq U_j^* \quad \forall j \neq i \end{aligned}$$

This problem can also be writen as:

$$\begin{aligned} \max_{y_i,x_i} U_i(x_i, Y) + \sum_{j \neq i} \lambda_j U^j(x_j, Y) \\ s.\ to\ y_i \geq 0 \\ \sum_i I_i = \sum_i x_i + pY \end{aligned}$$

The solution implies:

$$\frac{\partial U_i(x_i^*, Y^*)}{\partial x} = \lambda_j \frac{\partial U_j(x_j^*, Y^*)}{\partial x} \quad \forall j \neq i$$
$$\sum_i^n \frac{\partial U_i(x_i^*, Y^*)/\partial Y}{\partial U_i(x_i^*, Y^*)/\partial x} = p$$

Assuming the same particular utility function again, we have that:

$$pY^* = \frac{\sum_j I_j}{\sum_{j=1} \lambda^j} \left[\sum_{j=i} (1 - \alpha_j) \lambda^j \right] \text{ [3]}$$

In general, each λ_j is a function of income levels (I_j's) and the public good price (p) and can be interpreted as a reflection of bargaining in the family. An increase in the income of a particular member j or a decrease of any member $i's$ income would increase λ_j. In this situation, it is easy to show that a redistribution of resources among the family members will potentially affect the amount of public good, and it will increase or decreases depending on the preferences of the receiving adult over it. Let $\gamma_k = \lambda_k / \sum_{j=1} \lambda^j$ be the share of family income allocated to adult k.

$$pY^* = \sum_{j=1} I_j \left[\sum_{j=1} (1 - \alpha_j) \gamma_j \right]$$

Therefore, the impact of increasing the income of adult k is:

$$\frac{\partial (pY^*)}{\partial I_k} = \sum_{j=1} (1 - \alpha_j) \gamma_j + \left(\sum_{j=1} I_j \right) \left[\frac{\partial \gamma_k}{\partial I_k} (1 - \alpha_k) + \sum_{j \neq k} \frac{\partial \gamma_j}{\partial I_k} (1 - \alpha_j) \right] \quad (1)$$

Assuming that the impact of the increase of income has an uniform effect on the share of all

[3] Notice that for one member $\lambda_i = 1$.

other adult members (i.e. $\partial\gamma_j/\partial I_k = \partial\gamma_i/\partial I_k \ \forall i, j \neq k$), the r.h.s. of equation (1) becomes:

$$\underbrace{\sum_{j=1}(1-\alpha_j)\gamma_j}_{A} \quad + \quad \underbrace{\left(\sum_{j=1} I_j\right)\left[\frac{\partial\gamma_k}{\partial I_k}\left(\sum_{j\neq k}\alpha_j/(N-1) - \alpha_k\right)\right]}_{B}$$

Therefore, an increase on member k's income will have two distinct effects on the public good: (i) - A direct income effect (A), which will be non-negative, as long it is a not an inferior good (which is the case in this particular utility function); (ii) there is an increase in the bargaining power of member k (B), which could reinforce or offset the income effect depending on the preferences of that particular adult over the public good compared to the other adults (i.e. if $\sum_{j\neq k}\alpha_j/(N-1) \geq$ *or* $\leq \alpha_k$).

The empirical strategies consist in estimating whether the variation in income driven by the social security reform generated a different impact on the demand for the public goods (schooling and health) depending on the gender of the eligible member, and also whether the pension income is spent like any other non-pension income or not. In a *non*-cooperative it is expected that the gender of the pensioner has no importance on the provision of the public good and also that the pension income is spent like any income from any other source.

3 Data

The data used in this research come from the *Pesquisa Nacional por Amostra de Domicílios* (*PNAD)* database. The PNAD is an annual household survey, with sample size equal to 1/500 of the Brazilian population (about 100,000 households) and is designed to produce a picture of the social-economic conditions of the Brazilian population. It covers all urban and almost all rural areas, except the Amazon region. It has been conducted in regular basis since 1981 by *IBGE* (Brazilian Census Bureau) except in years in which census data was collected (1991 and 2000) and years with budget constraint (1994). *PNAD* contains also extensive data on personal and household information. For each person, information about age, schooling attendance, literacy, migration, labor participation,

retirement, income sources (including values), etc. is available. Moreover, periodically questions about some other special topics (the "Supplements") are included in the survey. For instance, in the 70's, the topic migration was included; in the 80's the special topics were heath education, labor market and social security; in the 90's, migration, fertility and child labor. To study the schooling outcomes (literacy and attendance), we will use the 1988, 1989 and 1990 PNAD's for years before, and the 1992, 1993 and 1995 for years after the reform took place (1991). We will analyze the impact on children between 6 and 18 years old who live in rural areas. Tables (5) and (6) show the evolution of literacy and attendance rate throughout the period analyzed. In the case of health indicators, there are three PNAD's supplements that have the health topic included: 1986, 1998 and 2003. We will use the first one for before and the 1998 for after the reform. Two indicators will be evaluated: reported illness and the search for any health care service in the past two weeks prior to the interview. We study the effects of the reform on those two health indicators to everyone, only to children (between 0 and 14 years old) and middle-age people (men between 40 and 55; and women between 40 and 50 years old).

4 The Reform

In October of 1988, the new Brazilian Constitution was promulgated. The Constitution established many changes in the principles of the entire social security system. However, it also determined that the Congress should approve an Ordinary Law, which should implement all changes. The main guiding principles stipulated by the Constitution were: extension of old-age benefits to anyone who was not a household head; no benefit should be smaller than one minimum wage; reduction in the minimum age of old-age eligibility; length-of-service eligibility to rural workers.[4]

The Congress passed the Ordinary Law [5] on July 24 of 1991. Only after that all the changes could be implemented and the reform went into effect. The new main rules sanctioned the law were:

[4][Beltrao *et al.*, 2003] and [Delgado, 1997] have more details about the constitutional new rules concerning the rural pension system

[5]Law # 8212 and 8213
http://www010.dataprev.gov.br/sislex/paginas/42/1991/8212.htm
http://www010.dataprev.gov.br/sislex/paginas/42/1991/8213.htm

- New minimum age eligibility equal to 60 for men and 55 for women against 65 years for both men and women before the reform
- Anyone in the household could be eligible if attained the minimum age requirement.
- Minimum benefit is increased to 100% of the minimum wage
- The value of the benefit is calculated based on previous earnings against a flat benefit of 50/
- Length-of-service pension available after 30 years of services for men and 25 years for women. The value of the benefit is also calculated based on previous earnings.

Beyond minimum age, eligibility to a rural pension requires from the worker proof of residence in a rural area and engagement in one of the rural activities defined by the law (farmers, fishers, miners, loggers, etc.) for at least 60 months prior to the application. Proving previous engagement in a rural activity was extremely easy, since many documents were accepted as sufficient proofs, such as individual labor contracts, tenancy contracts, sharecropping agreement, among others.

Although the law stipulated an earning-based benefit, the great majority of the rural pensioners received the minimum wage at least until 1997 because almost all rural workers did not keep a document record of previous earnings. For instance, the average rural benefit paid in 1997 was around R$121, while the minimum wage was R$ 120. By the same reason, the proportion of length-of-service retirees in the rural system is insignificant, less than 0.1% of the total number of pensioners. Therefore, we do not worry about any kind of endogeneity caused by differences in the value of the benefit or in the age of retirement.

The increase in the minimum benefit for the current pensioners was instantaneous. The income of those who were pensioners in the old system doubled right after July of 1991. However, for those who are now eligible or decided to apply after the change in the rules, the entire process of registration took several months due to administrative delays. Therefore, for the entire group of people who became eligible after the reform, the impact was not automatic. Beside, since the worker should apply to be registered in the system, concerns about selectivity bias, especially about the timing of the reform impacts, arise. Probably many newly eligible workers in a first moment ignored the new rules of the system and the decision about applying could be correlated with

outcome of interest. That is one of the reasons why we use datasets until 1995 in order to let the take-up process of the newly eligible complete. As we can see in table (2), the proportion of pensioners among newly eligible before the reform was around 15%[6] and increase until stabilizing around 50% after 1995.

5 Identification Strategies

Identification

Consistently estimating the effect of income variations on different family members is not a simple task. Using disparity of income across families from a single cross-section may introduce lots of identification problems in the regressions. Family's unobserved characteristics might be correlated with family's income and also with investment in schooling and health. This situation could lead to several misinterpretations of the data. Based on the results of such simple regressions, one could argue, for example, that an extra amount income would imply an increase on schooling attendance, when, in reality, it is the intellectual level of the parents, which is correlated with the family income that drives the decision about the child schooling. An exogenous source of income variation is a *sine qua non* condition for having consistent and reliable results. Hence, the modification in the social security system in Brazil in 1991 is an excellent source of income variation to be explored, since it is not correlated with any family unobserved characteristics.

However, using total income that comes from the benefits or even a direct indicator if the family has a person that is a pensioner is also problematic. The value received from the pension (when it is not the minimum value) is derived from the past labor earnings and could possibly also be correlated with unobservable characteristics. Directly including a dummy variable indicating if the person is a pensioner might also generate inconsistent results, since the decision to apply (and when to apply) to the benefit could potentially be endogenous. For instance, rich people might not be willing to go to a post office and stay in the line in order to apply for the pension, since their extra income utility could not compensate the cost of applying to the pension. In this case,

[6] This number is not zero probably because of migration of previous urban workers to rural areas or public employees who have their own pension system

a selectivity bias problem could arise. Therefore, we will pursue a *intent-to-treat* approach, which actual treatment is replaced by eligibility in order to avoid the selectivity bias problem[7].

Moreover, the use of variation across households in social security income to identify the impact of earnings on social outcomes requires adequate control of the effects of living with an elderly person unrelated to their social security revenue. Families who co-reside with an elderly person may differ from other families for several reasons. Elderly people may have different preferences over the importance of children education compared to the other family members, or concern more about their own health, or, in general, the presence of an elderly person may be correlated with other unobserved characteristics that are also correlated with the outcomes of interest. Therefore, from one single cross-section, it is impossible to disentangle the direct income effect of coming from the old-age pension from the impacts of living with an elderly person. An exogenous reform in social security, however, permits the separation of theses effects.

Nevertheless, the comparison between the cross-sectional patterns of outcomes before and after the reform would identify the effect of the changes in social security income only in the absence of any ongoing trend. In the presence of such trend a *before and after* estimator would be upward or downward biased depending whether the trend is positive or negative sloped. In order to control for the time trend effect, we will use a *difference-in-difference* approach. The *difference-in-difference* estimator will be consistent as long as the time variation on the outcome of interest would be the same for both *treated* and *control group* in the absence of the *treatment*, i.e., only if both groups have the same time trend. If the control group has a different time pattern from the *treated*, the *difference in difference* estimator will be biased. Suppose $Y_{it,0}$ and $Y_{it,1}$ are, respectively, the outcomes for the non-treated and treated individual i at time t and they are modeled by the following equations:

$$Y_{it,0} = \beta_{i,0} + \delta_{t,0} + \epsilon_{it}$$

$$Y_{it,1} = \beta_{i,1} + \delta_{t,1} + \alpha + \epsilon_{it}$$

where $\beta i, .$ are the fixed effects, $\delta_{t,.}$ are the time effects and α indicates the true effect of the treatment. Let $t = b, a$ (before and after the treatment). For simplicity, we will assume that

[7]Nonetheless, keep in mind that we will refer to families with an *eligible* people as the *treated* group

$\delta_{b,1} = \delta_{b,0} = 0$. The *difference in difference* estimator will only be unbiased if $\delta_{a,1} = \delta_{a,0}$, i.e, if both *treated* and *control groups* have the same time pattern.

$$\begin{aligned} E[\alpha_{DD}] &= E[Y_{ia,1} - Y_{ib,1}] - E[Y_{ia,0} - Y_{ib,0}] \\ &= \alpha + \delta_{a,1} - \delta_{a,0} = \alpha if\ \delta_{a,1} = \delta_{a,0} \end{aligned}$$

Assuming now

$$\delta_{a,\cdot}^{e=1} = \delta_{a,\cdot}^{e=0} + \Delta^e \tag{2}$$

where $e = 1$ indicates if the individual has an elderly member in her family. More specifically, we are assuming that the potential difference in the time trend of the *treatment* and control group (Δ^e) does not come from the *treatment per se*, but instead from the presence of an elderly person in the family. An unobservable shock could have affected only families with an elderly member in the same moment of the pension reform. Therefore, in order to obtain an unbiased estimator of the true *treatment* effect, we need a *control group* that has an elderly member ($e = 1$), but was not affected by the reform.

We plan to use families with an *almost*-eligible person – man between 55 years and 60 years old and/or women between 50 years and 55 years old– as the *control* group[8] in order to disentangle the true *treatment* effect from the specific elderly trend effect.

Furthermore, the reform is also an extraordinary opportunity to check the validity of the *non*-cooperative intra-household allocation model illustrated in section 3. Testing the validity of the unitary model of intra-household resource allocation without an exogenous income variation may also lead to erroneous conclusions. Most studies in the literature use only the variation across families of unearned income in the hands of different member (for example mothers and fathers) to identify the allocation process with households. They usually regress the children outcomes (health, schooling, anthropometrics, nutrient intakes, etc) on parent's unearned income. By comparing the coefficient of each parent income, those studies gauge the consistency of the unitary model. However, since the difference between unearned incomes of distinct members in the family is not

[8]From now on, evey time we refer to the *control* group, we will be mentioning about families with an *almost*-eligible member

likely to be orthogonal to unobserved characteristic in the family that also affect the outcome of interest, the conclusion based on those estimated coefficients may be invalid. For instance, suppose families that depend more on unearned income care less (both fathers and mothers) about education. In addition, it is possible that the fact that they need more this extra money induces the member of the family who works to also search for these alternative resources. In this case, the difference in unearned income between the working and non-working member of the family would be higher for such families and could lead to a spurious correlation with the outcome of interest. Since the income variation caused by the reform was out of the family's control and therefore orthogonal to any unobserved characteristic that could be correlated to the provision of the public good, we will test the validity of the unitary model by examining if the reform had uneven impacts on the outcome of interest conditioning on the gender of the eligible person.

The benchmark regression is the following:

$$E\left[Y \mid T^{m,f}, C^{m,f} Post, W\right] = \beta_0 + \beta_1^m T^m + \beta_1^f T^f + \beta_2^m.C^m + \beta_2^f.C^f + \beta_3.Post$$
$$+\beta_4^m T^m \times Post + \beta_4^f T^f \times Post + \beta_5^m.C^m \times Post + \beta_5^f.C^f \times Post + W.\gamma \quad (3)$$

where Y is the outcome variable (explicitly, schooling and health indicators); T^j (for both $j = m, f$) points out if the individual's family has the presence of at least one eligible person in the new system rules – (T^m) man 60 years or older or (T^f) woman 55 years or older – i.e, the *"treated"* group; $C^{m,f}$ indicates whether the individual co-resides with at least one male (C^m) or female (C^f) *almost*-eliligible person, i.e, if she is part of the *control* group; W is a vector of household and personal characteristics such as age, age squared, education attainment of the head of household, head's gender, age and race, number of family members, number of children in family, etc; *Post* is a dummy denoting post-reform years (after 1991). $T^{m,f}$ and $C^{m,f}$ enter in the equation as main effect and interacted with *Post*.

With this specification and assuming a linear probability model, β_4^j (the coefficients of the interaction terms between T^j and *Post*) will be the *Difference-in-Difference* estimators of the *treated* against the *reference* group, which is, in this case, everyone who resides in the rural areas

and does not co-reside with an eligible or an *almost*-eligible person. β_4^j is consistent as long as $\Delta^e = 0$ in equation (2), i.e, both *treated* and *reference* groups have the same time trend.

Comparing β_4^j with β_5^j allows us to check if β_4^j are indeed capturing the reform effect driven by the increase in benefits[9] or if it is just reveling an elderly presence trend effect. $\beta_4^j - \beta_5^j$ is the *difference and difference* estimator when the comparison is the *control* group. In this case, both *treated* and *control* groups have an elderly member in their family. Therefore, even if there is a specific time trend related to the presence of an elderly person, $\beta_4^j - \beta_5^j$ will consistently estimate the true treatment effect.

Finally, the unitary model will be a good specification of the decision-making process only if $\beta_4^m = \beta_4^f$. Assuming that the direct income impact of the reform was uniform across man and women, difference in the effects on the provision of the public good must be caused by changes in the bargaining power within the family. In other words, if part A of equation (2) is the same for both male and female pensioners and if $\beta_4^m \neq \beta_4^f$ then part B of the same equation must be different from zero, which violates the unitary model.

New × old eligibility

Since the decision about applying to the benefit is potentially endogenous, the results based on specification (3) have to be carefully interpreted. As explained in section (4), the reduction in the age eligibility does not exogenously guaranteed an extra amount of income for families with a newly eligible person right after the reform in the system. Depending on how important the extra income is for the eligible member and her family, the decision of whether and when to apply to the benefit could be different from family to family. And if the source of this "application" heterogeneity was correlated to the outcome of interest, the result estimated would have to be understood differently than if it was not the case. With effect, each β_4^j in equation 3 captures the average impact of the reform on the entire group of potentially *"treated"* families.

Moreover, even disregarding the possible heterogeneity problem exposed above, work with just one *treatment* effect could raise another problem, since the impact of the reform on families with

[9] Actually, in this specification, β_4^j is also capturing the effect of the reduction in age eligibility. Below, we will disentangle the outcome of those two change in the rural pension system

a newly eligible was different on those with an old eligible member. More specifically, for those families that have a newly eligible member who now receives the minimum benefit, the amount of benefit received was zero before and increased to one minimum wage after the reform. On the other hand, for those who were already beneficiaries, the reform impact was half as much as on the first group. Therefore, again, β_4^j would capture an "average" effect that could underestimate or overestimate the true impact on each one of those two *treatment* groups.

In order to disentangle the effect of the increase in the minimum benefits and the change in eligibility age we estimate the following:

$$E\left[Y \mid T_i^j, Post, W\right] = \beta_0 + \sum_{j=m,f}\left[\sum_{i=1}^{2}\left(\beta_{1,i}^j.T_i^j\right) + \beta_2^j.C^j\right] + \beta_3.Post + \sum_{j=m,f}\left[\sum_{i=1}^{2}\left(\beta_{4,i}^j.T_i^j.Post\right) + \beta_5^j.C^j.Post\right] + W.\gamma \quad (4)$$

where $T_i^j\prime s$ are: $(i=1)$ the presence of an individual eligible in the old system rules – 65 years or older person– this term captures the impact of the increase in the minimum benefits values from 50% of the minimum wage to 100% of it.; $(i=2)$ presence of a newly eligible individual in the new system rules – man between 60 years and 65 years old or women between 55 years and 65 years old – this term captures the effect of reduction in the age requirement.

In this case, $\beta_{4,i=1}^j$ and $\beta_{4,i=2}^j$ will be, respectively, the *Difference-in-Difference* estimators of the *treated* group 1 and 2 against the *reference* group. Again, testing $\beta_{4,i=1}^j = \beta_5^j$ and $\beta_{4,i=2}^j = \beta_5^j$ allows us to check if the effect is coming from the reform or a specific characteristic of families with elderly people.

The model specified by equation (4) also let us analyze whether the different changes in the rural pension system provoked uneven outcomes by testing if $\beta_{4,i=1}^j = \beta_{4,i=2}^j$.

Again, testing if $\beta_{4,i=1}^m = \beta_{4,i=1}^f$ and $\beta_{4,i=2}^m = \beta_{4,i=2}^f$ indicates the unitary model validity.

6 Results

Impacts on Income

Before attempting to measure any effect of the changes of the pension system on social-economic outcomes, it is important to be sure that the social security reform has impacted on families' total income in rural areas.

Graph 1 illustrates the variation over time of the total family income[10] of the *"treated"* ,*control* and *reference* groups in rural areas. As expected, there was a big jump in the family income with the presence of eligible people after 1990. In 1988, the average family income of that group was R$ 453, lower than the *control* group's (R$ 582) and all other families' income (R$ 507). There was a uniform jump up for all groups in 1989 due to the country economic growth and decrease in 1990 after a big economic recession[11]. In 1992 (after the reform), the *"treated"* group experiences a considerable growth in family income. On the other hand, both the control group and all rural families had their income diminished. From 1993 to 1995, Brazil has grown in average 5% per year and that growth is exhibited in the graph: all groups presented significant increase (around 15%) in total family income during the period. From 1988 to 1995, the *"treated"* group has experienced a total family income growth of 30.4% in real terms, while the for the *control* group has grown only 9.0% and just 1.3% for all other rural families.

In graph 2, the *"treated"* group is broken into two different groups, depending on the gender of the eligible person. Both T^m and T^f groups' incomes have very similar path throughout the period. It is an indicative that uneven impacts of the reform for families with a man or a woman pensioner cannot be credited only to differences in the family income variation. It is more likely that such discrepancies have arisen due to divergences in preferences of male and female recipient.

As mentioned above, the reform consisted in many different changes in the law. With the dataset available, we were able to disentangle the impacts of two sets of *"treatments"*: (1) the effect of the increase in the minimum benefit (from half to one minimum wage) and (2) the reduction in the age requirement in order to become eligible (from 65 to 60 years for men and from 65 to 55 for women).

[10] in September 2002 Reais (R$)

[11] The real GDP growth rates in 1989 and 1990 were respectively 3.2% and -5.05%

Graph 3 shows the variation of the total family income of both *"treated"* groups. Clearly, the impact of the minimum benefit increase starts right after the reform in 1992. The rise in the total family income of the "old eligible" group was 30.2% in the first year after the reform, while the impact on family income of the new age eligibility rule was only sufficient to offset the significant recession happened in the country in that period – the total family income of that group was practically stable from 1990 to 1992. From 1992 to 1995, both *"treated"* groups presented an increase in the family income. However, in this period the growth in "newly eligible" group was much more significant than the "old eligible" one. The former has increased 30.6% and the latter only 6% in the period. The *control* and *reference* groups experienced an increase of 17.2% and 16.2% respectively.

The timing of family income boost for the "old eligible" group illustrates the impact of the increase in the minimum benefit value. Since the change in benefit was automatic for everyone already registered in the social security system, there was effect was immediate. On the other hand, "newly eligible" individuals had first to register in system before receiving the benefit. Disinformation, administrative delays, transportation costs to the near post office could explain the postponement of the increase in the family income.

From 1988 to 1995, the total increases in the family income were 33.2% for the "old eligible" group; 18.3% for the "newly eligible"; 4.9% for the *control* group and a decrease of 1.2% for the entire rural sector.

Graph 5 shows that the participation of the eligible person's income in the total family income has also increased in the period, and graph 6 illustrates that this increase has happened for both male and female recipient, but was more acute for women, since now the system allows spouses to also be pensioners.

Table 9 shows the results of an OLS regression of model (4) where the dependent variable is the total family income in logs. All control variable coefficients are statistically significant and have the expected sign. The total family income is higher if the head is older (with concavity), more educated, male and white; also, families with more children tend to have lower income. It is also worthy noticing that all *"treated"* are poorer compared to the *reference* group, indicating

that the reform mostly impacted families in the bottom of the income distribution.. Looking now at the *difference-in-difference* coefficients, the results corroborate the evidence found in the graphs above. The *"treated"* families have experienced a significant growth in their income after the reform. All $\beta^j_{4,i}$ are strongly significantly different from zero and from their respective counterpart in the control group (β^j_5). Moreover, the coefficients of the "old eligible" groups (for both male and female pensioners) ($\beta^j_{4,i=2}$) are statistically significantly higher than the "newly eligible" ones ($\beta^j_{4,i=1}$). Once more, this could be an illustration of the new registration delays occurred after the reduction in the eligibility rule. As expected both $\beta^f_{4,i=1}$ and $\beta^f_{4,i=2}$ are significantly bigger than $\beta^m_{4,i=1}$ and $\beta^m_{4,i=2}$, indicating a bigger effect on income of the presence of an eligible females compared to the presence of an eligible male due to the new rules concerning spouses.

Impacts on Schooling and Health

Table 10 has the results of specification (4) regression with schooling attendance or literacy as the dependent variables. Columns (1a) and (1b) show the results for regressions with the entire sample of children between 6 and 18 years old who live in rural areas. As expected, children living in *treated* families are in general less likely to be literate and attending school, since they are in average poorer families. Again, the coefficients of the control variables are all significant with expect sign, reflecting the results found on the income regressions. Children living with an older, male, more educated and white head show better schooling outcomes. By the same token, the number of children in the family has a negative impact on education. The *Post* coefficient captures the big positive trend in both attendance and literacy in the period. Since the beginning of the last decade, Brazil has been considerably improving its educational performance, especially in the elementary level of schooling. The significantly positive trend illustrates the importance of having a control group that correctly mimics the behavior of the treatment group in the absence of the reform in order to consistently estimate its effects.

The *difference-in-difference* coefficients show that the presence of an eligible male in the family had a positive and significant effect in the schooling achievements. Children living with a newly eligible male ($T^m_{i=1}$) are 2.6% and 2.7% more likely to be literate and attending school, respectively,

while children with an old eligible male ($T^m_{i=2}$) is also significantly more likely to attend school (2.7%). On the other hand, despite causing a bigger increase in the family income, the presence of an eligible female does not seem to have improved the schooling outcome of the children. Either the presence of a newly ($T^f_{i=1}$) or an old ($T^f_{i=2}$) eligible females has positively affected literacy or attendance. Actually, the presence of an old eligible female seems to decrease the likelihood of being literate.

Breaking the sample between boys and gilrs, we find positive effects in attendance for both $T^m_{i=1}$ and $T^m_{i=2}$ boys. On the other hand, $T^m_{i=1}$ girls seem to benefit in terms of literacy. Again, either boys or girls benefited form living with an eligible female.

Table 11 and 12 shows the results after splitting the sample into two different subgroups: younger children - between 6 and 14 - and older children - between 14 and 18. It is clear that the first group benefited much more from the reform than the second, especially the $T^m_{i=1}$ children, however $T^m_{i=2}$ older boys also suffered a significantly positive effect on attendance. Again, children with an eligible female seem to not benefited from the reform, at least compared to those in the reference group.

The last rows of each panel in table (13) show the joint F-test if the presence of an eligible male is significant different from the presence of an eligible female in the family. Panel (A) shows the results for the regressions with the entire sample; Panel (B) and (C) show the results for children between 6 and 14; and 14 and 18, respectively. We can see that in all sub-samples the presence of an eligible male had a significantly bigger effect than the presence of a female, especially for boys' attendance. Those results indicate that the families are not pooling their income and deciding the provision of those public goods based on the total family income; if that were the case there would be no reason why the presence of an eligible male would have a different impact on schooling than the presence of an eligible female. Thus, the findings contradict the core of the unitary model.

Table 18 displays the results of specification (**??**) having both health indicators (illness and search for health care) as dependent variables. As expected, there is a negative relation between the probability of being ill and head's schooling and gender; and the opposite occurs in the case of health care search. There is negative correlation between age and illness driven by the children. In

the case of middle-agers, the correlation turns positive. This U-shaped relation between age and illness is acknowledged by the literature. In the case of search for health care services, there is a negative correlation with age for both children and middle-agers. Again, the impact of the reform can be measured by the *dif-in-dif* coefficients ($\beta^j_{4,i}$). Looking at the results, we can see that only significant impact of the reform was in the likelihood of being sick middle-agers who live with an old eligible male. Compared to the *reference* group, all other groups seem to have suffered no effect from any of the other *treatment* groups.

The last row of table (19) displays the test whether there were different effects from the presence of eligible males and females. Once more, middle-age people who live with an eligible male significantly[12] benefited more than those living with an eligible female.

Robustness Checks

As mentioned in section 5, the *dif-in-dif* estimator is consistent only if *reference* group has the trend process as the *treatment* group in the absence of the treatment. For that reason, we included in the regressions a *dif-in-dif* estimator for the *control* group - familes with an *almost*-eligible member - in order to capture a possible trend associated with the presence of an elderly person in the family. Table (13) displays the F-tests comparing the *dif-in-dif* coefficients of the *treatment* and *control* groups in the schooling regressions.

Looking across the three different panels (one for each sub-sample), we observe that results are, in general, similar to those found comparing the *reference* group. The presence of eligible males seems more valuable than eligible females; and children between 6 and 14 years old benefited more from the reform than the older ones.

One possible caveat of the *intent-to-treat* approach rises if, for instance, the take-up ratios for males and females are very different from each other. In this case, all uneven impacts of males and females shown in the main results could be driven such differences in the take-up ratios. One way to test if that is the case is to see the effects of the true *treatment* and compare to the *intent-to-treat* ones. Defining a *treated* group as the families that have a member who is an eligible *pensioner* (i.e.

[12] at 10% of significance

there is a person in the family who receives a pension and matches the age eligibility criteria), we ran specification 3 regressions using both definitions of *treatment*. Tables (14) and (20) show both regressions for schooling and health, respectively. Panel (A) of the tables displays the results of the *intent-to-treat*, while Panel (B) the actual *treatement* effects.In both case, it does seem to have a big difference between those two strategies. The results are qualitatively very similar, showing again a much bigger effect of the male presence compared to the female.

Income × Bargaining

[Case and Deaton, 1998] show that one way to test if the pension income has the same impact as any other income source is by doing the following decomposition of total family income effect on the outcome of interest:

$$\ln[I_n + \phi I_p] = \ln[I + (\phi - 1)I_p] \approx \ln(I) + (\phi - 1)I_p/I \tag{5}$$

With the above specification, we need to test if the coefficient of the pension income share over the total family income (I_p/I) is different from zero. If that is the case, $\phi \neq 1$ and the pension money has a different effect on the public good provision than the rest of the family income. Moreover, this specification is also a direct test of the unitary model of intra-household allocation. Non-zero coefficients of the income shares confirm the influence of the bargaining power in the decision-making process over the public good allocation within the family as illustrated in the equation 2

Table 15 shows the results of a regression with schooling attendance as the dependent variable and total family income, male pension share, female pension share and all other control variables. We can see that indeed the fractions of income provided by the pensioner have a positive effect on attendance, indicating the double impact of the reform on attendance by, first, increasing total family income and, second, by increasing the bargaining power of the pensioner, defined by her share on total family income. The results shed light on the impact differences of the reform depending on the gender of the recipient. It seems that, although both man and woman pensioners care more about education than the average adult member of the family especially for boys, eligible males

manage to transform bargaining power in actual provision more efficient than females. This result is even more evident when we reduce our sample to only treated families as shown in table 16. This could be driven not only by differences in preferences, but it also possible that social norms or cultural models make the male income share more relevant for concrete bargaining power inside the family. Narrowing down our sample to only families with both female and male pensioners (table 17) does not change the main conclusion that male pension share is more important for schooling attendance, especially for boys.

In the case of illness, the same conclusions arise. As expected total family income has a negative effect on the likelihood of being ill, and the male pension income share has a significant and bigger impact on the reduction of illness for middle-agers (table 21). Reducing the sample to only treated families (table 22) diminishes the precision of the estimators without changing their signs[13]. The same occurs in the sample including only families with both male and female pensioners (table 23).

7 Conclusion

The social security reform occurred in 1991 in Brazilian rural pension system has led to an increase in schooling indicators for young children living with an eligible male. Boys are more likely to attend school and girls to be literate. The reform has also negatively impacted the probability of being ill of middle-age people living with a male pensioner. These uneven results for the presences of male and female recipients are evidences that the unitary model of household resource allocation does not represent the Brazilian poor rural families' decision-making process over their budget. The income is not pooled and provision of education and health is not made based upon the total family resources. Moreover, we find an indication that the impact of the pension income on the schooling and health was higher than the rest of the family resources. The increase in the bargaining power of the elderly, especially males, may explain these findings, suggesting that they have stronger preferences toward those outcomes. The results show the importance of targeting specific family members in cash transfer programs in order to maximize the effect on desirable outcomes. Although families with an elderly person have in average low schooling and health

[13]Except the coefficient of the female pension share on the sample with only children

indicator levels, cash transfers to those members of the family have significantly bigger effect than unconditional transfers. In addition, since more than 10% of all children in rural areas live with an eligible person, it is very plausible to attribute part of the success Brazil has achieved in the past 15 years in education and health, specially for poor families, to the social security reform of 1991.

The findings have also great consequences for the design of conditional cash transfer programs, such as *Bolsa Escola* and *Bolsa Familia*. Those programs directly create incentives for schooling by requiring attendance; however, they do not set a particular member of the family as the receiver. Therefore, targeting specific members in the family may boost the direct income effect on schooling outcomes, enhancing the effectiveness of the programs.

A straightforward extension for this paper is to measure the impact of the reform in many other social outcomes, like child labor, fertility, anthropometrics, and other finer health indicators.

References

[Becker, 1964] Becker, G. S. (1964) *Human capital*. Columbia Press University.

[Becker, 1974] Becker, G. S. (1974) "A Theory of Social Interactions." *Journal of Political Economy* **82**(6): pp. 1063–93. Available at http://ideas.repec.org/a/ucp/jpolec/v82y1974i6p1063-93.html.

[Becker, 1981] Becker, G. S. (1981) *A Treatise on the Family*. Harvard University Press.

[Beltrao *et al.*, 2003] Beltrao, K. I., *et al.* (2003) "The 1988 Constitution and Acess to Social Security in Rural Brazil: Towards Universalization."

[Case and Deaton, 1998] Case, A. and Deaton, A. (1998) "Large Cash Transfers to the Elderly in South Africa." *The Economic Journal* **108**: pp. 1330–1361.

[Chiappori, 1992] Chiappori, P.-A. (1992) "Collective Labor Supply and Welfare." *Journal of Political Economy* **100**(3): pp. 437–67. Available at http://ideas.repec.org/a/ucp/jpolec/v100y1992i3p437-67.html.

[de Carvalho Filho, 2000] de Carvalho Filho, I. E. (2000) "Old-age benefits and the labor supply of rural elderly in Brazil." Mimeo.

[de Carvalho Filho, 2001] de Carvalho Filho, I. E. (2001) "Household Income as a Determinant of child Labor and School Enrollment in Brazil: Evidennces from a Social Security Reform." Boston University Working Paper Series.

[Delgado, 1997] Delgado, G. C. (1997) "Previdencia Rural - Relatorio de Avaliacao Socioeconomica." Texto para Discussao - IPEA 477.

[Delgado, 1999] Delgado, G. C. E. (1999) "Experiencias Exitosas de Combate a Pobreza Rural: Licoes para Reorientacoes de Politicas." Projeto FAO Pobreza - LOA 98290/RLC.

[Duflo, 2000] Duflo, E. (2000) "Grandmothers and Granddaughters: Old Age Pension and Intra-household Allocation in South Africa." Available at http://ideas.repec.org/p/nbr/nberwo/8061.html.

[Emerson and Souza, 1997] Emerson, P. M. and Souza, A. P. (1997) "Bargaining over Sons and Daughters: Child Labor, School Attendance and Intra-Household Gender Bias in Brazil." Available at http://ideas.repec.org/p/van/wpaper/0213.html.

[Ermisch, 2003] Ermisch, J. (2003) *An Economic Analysis of the Family.* Princeton Univerity Press.

[Thomas, 1990] Thomas, D. (1990) "Intra-Household Resource Allocation: An Inferential Approach." *Journal of Human Resources* **25**: pp. 635–664.

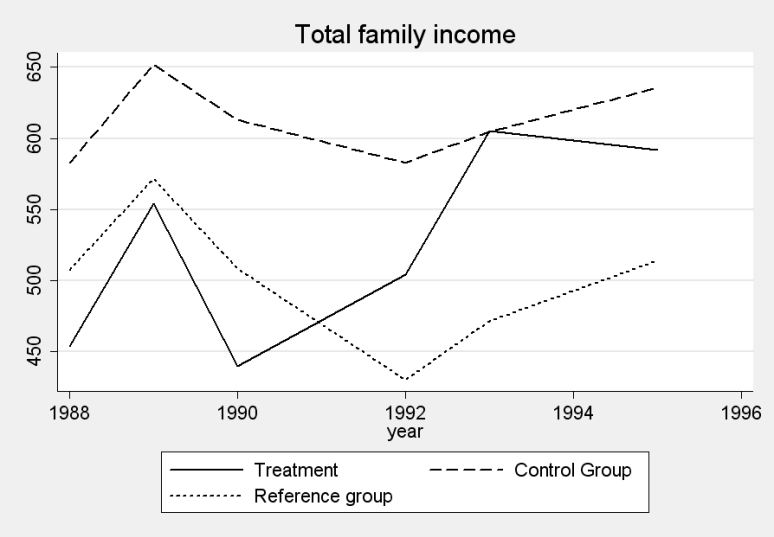

Figure 1: Total family income - in 2002 R$

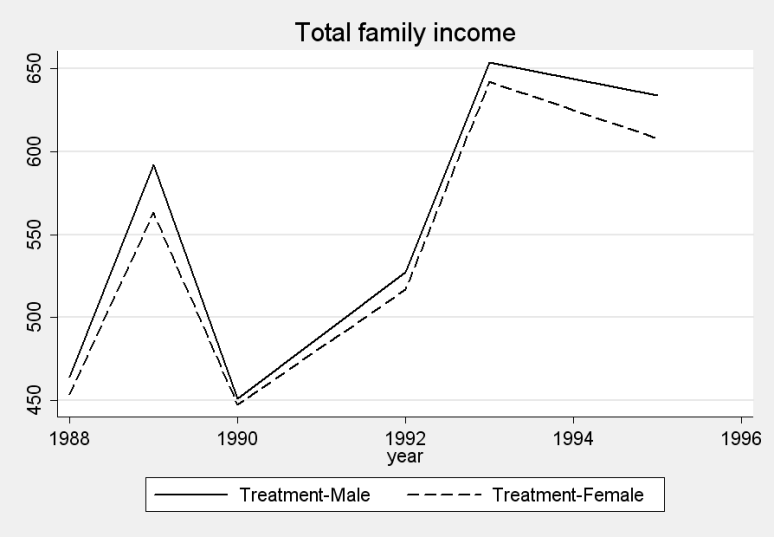

Figure 2: Total Family Income (by gender) - in 2002 R$

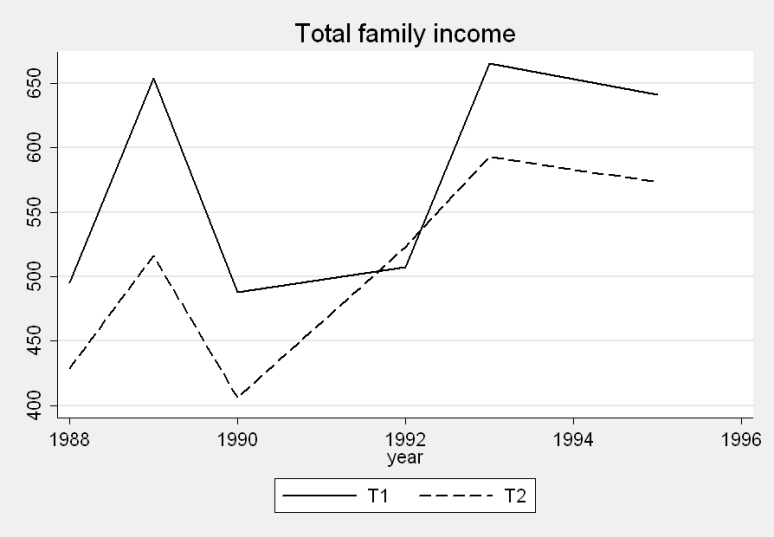

Figure 3: Total Family Income (by treatment groups) - in 2002 R$

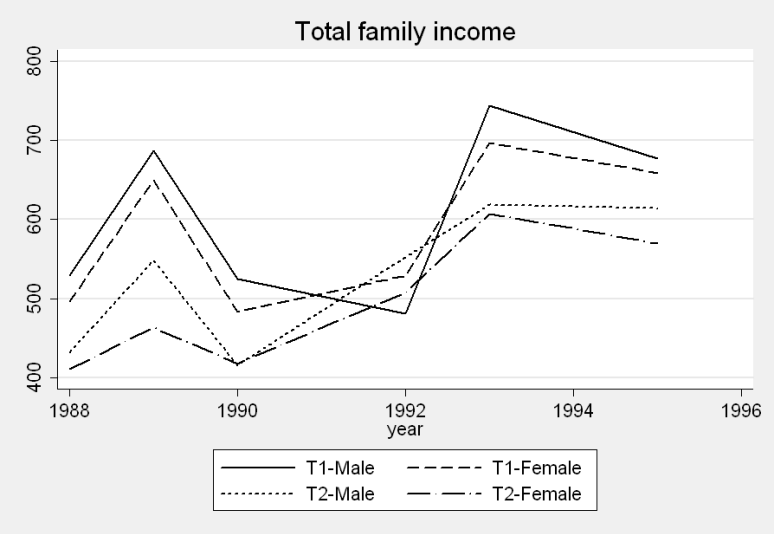

Figure 4: Total Family Income (by treatment groups and gender) - in 2002 R$

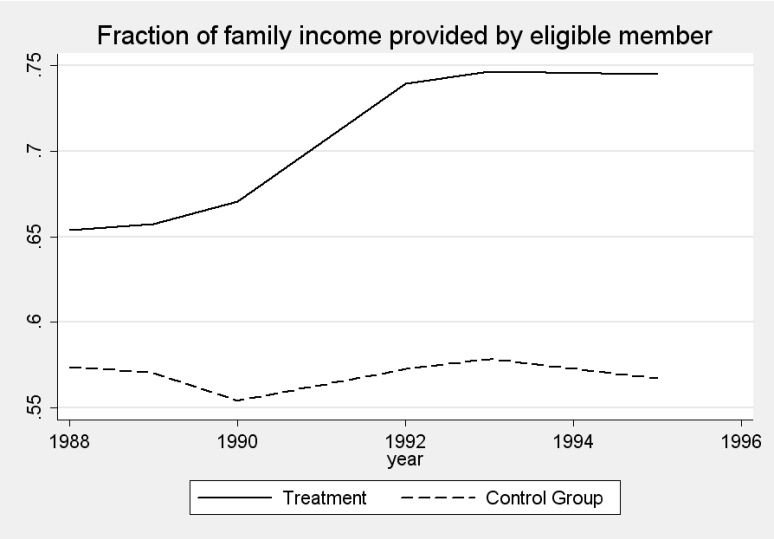

Figure 5: Fraction of family income provided by eligible/pre-eligible person

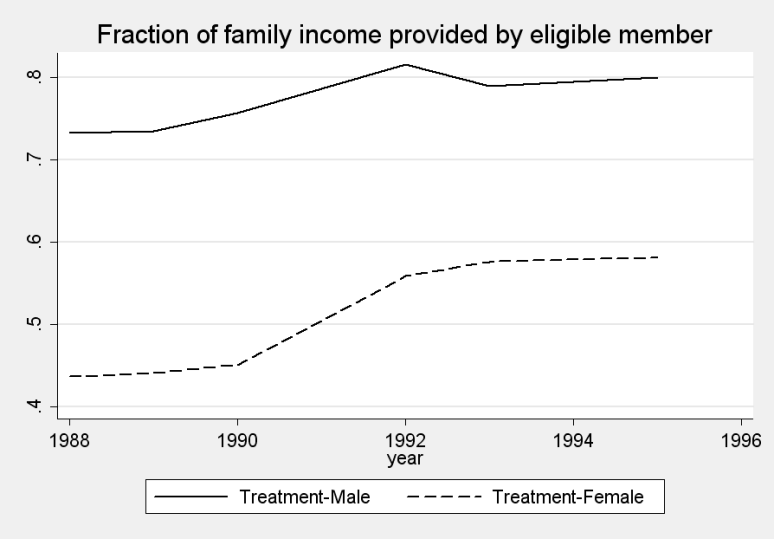

Figure 6: Fraction of total family income provided by eligible/pre-eligible person (by gender)

Table 1: Number of people in the sample - Rural area

	Before reform			After reform		
Year	1988	1989	1990	1992	1993	1995
Eligible people (male $\geq$ 60, female $\geq$ 55)	5,537	5,550	5,987	6,054	5,849	6,220
Eligible males	2,412	2,454	2,675	2,598	2,614	2,655
Eligible females	3,125	3,096	3,312	3,456	3,235	3,565
Newly eligible people (65 $\geq$ male $\geq$ 60, 65 $\geq$ female $\geq$ 55)	2,457	2,407	2,540	2,595	2,560	2,638
Newly eligible males	812	784	843	849	856	847
Newly eligible females	1,645	1,623	1,697	1,746	1,704	1,791
Old eligible people (male and female $\geq$ 65)	3,080	3,143	3,447	3,459	3,289	3,582
Old eligible males	1,600	1,670	1,832	1,749	1,758	1,808
Old eligible females	1,480	1,473	1,615	1,710	1,531	1,774
Control group (60 $\geq$ male $\geq$ 55, 55 $\geq$ female $\geq$ 50)	2,098	2,124	2,229	1,991	2,044	2,077
Control group - males	1,004	997	1,025	928	943	982
Control group - females	1,094	1,127	1,204	1,063	1,101	1,095

Table 2: Take-up rates among eligibles

	Before reform			After reform					
Year	1988	1989	1990	1992	1993	1995	1996	1997	1998
Eligible people	73.08%	73.81%	74.76%	52%	66.05%	68.54%	69.05%	69.01%	70.17%
Elig. men	81.88%	84.49%	85.48%	67.9%	77.01%	78.19%	77.02%	77.18%	78.6%
Elig. women	63.58%	61.71%	62.6%	40.05%	57.19%	61.35%	62.96%	62.83%	63.9%
Old eligibles	73.08%	73.81%	74.76%	74.41%	82%	81.88%	82.57%	83.74%	83.42%
Old elig. men	81.88%	84.49%	85.48%	86.79%	89.7%	89.88%	89.6%	89.26%	89.95%
Old elig. women	63.58%	61.71%	62.6%	61.75%	73.15%	73.73%	75.03%	77.92%	76.63%
Newly eligibles	16.12%	16.24%	15.67%	22.12%	45.55%	50.42%	50.66%	48.66%	52.19%
Newly elig. men	17.61%	19.52%	17.2%	28.98%	50.93%	53.25%	49.17%	50.12%	53.58%
Newly elig. women	15.38%	14.66%	14.91%	18.79%	42.84%	49.08%	51.36%	47.99%	51.56%

Table 3: % of people living with *"treated"* and *control group* families

	Before reform			After reform		
People living w/ an eligilble person	17.45%	20.66%	21.58%	23.95%	23%	23.48%
People living w/ a *control group* person	8.52%	10.5%	10.44%	10.19%	10.57%	10.35%
People living w/ a newly eligilble person	7.34%	8.73%	8.82%	10.01%	9.75%	9.64%
People living w/ an old eligilble person	9.99%	11.77%	12.64%	13.79%	13.14%	13.7%
Families w/ an eligible member	25.84%	25.83%	26.43%	28.77%	27.71%	28.28%
Families w/ a *control group* member	9.88%	10.12%	9.88%	9.63%	10.11%	9.72%
Families w/ a newly eligible member	10.32%	10.14%	10.03%	11.08%	10.84%	10.73%
Families w/ an old eligible member	15.34%	15.51%	16.26%	17.51%	16.69%	17.37%

Table 4: % of children (≤ 15) living with *"treated"* and *control group* families

	Before reform			After reform		
	1988	1989	1990	1992	1993	1995
Children w/ an eligible person	11.08%	10.31%	11.29%	12.25%	11.81%	11.38%
Children w/ a *control group* person	7.06%	7.11%	7.08%	6.96%	7.16%	7%
Boys w/ an eligible person	11.11%	10.31%	11.36%	12.1%	12.33%	11.57%
Boys w/ a *control group* person	7.5%	7.23%	7.3%	7.08%	7.35%	7.16%
Girls w/ an eligible person	11.04%	10.31%	11.22%	12.41%	11.27%	11.18%
Girls w/ a *control group* person	6.6%	6.99%	6.86%	6.84%	6.97%	6.82%
Children w/ an eligible male	4.34%	4.17%	4.53%	4.51%	4.74%	4.02%
Children w/ a *control group* male	2.89%	2.9%	2.51%	2.2%	2.5%	2.39%
Boys w/ an eligible male	4.46%	4.25%	4.51%	4.58%	5.04%	4.1%
Boys w/ a *control group* male	3.06%	2.91%	2.59%	2.2%	2.45%	2.55%
Girls w/ an eligible male	4.2%	4.09%	4.56%	4.44%	4.43%	3.95%
Girls w/ a *control group* male	2.72%	2.89%	2.42%	2.2%	2.55%	2.22%
Children w/ an eligible female	4.56%	4.26%	4.6%	5.21%	4.6%	4.87%
Children w/ a *control group* female	2.34%	2.44%	2.37%	2.71%	2.92%	2.6%
Boys w/ an eligible female	4.42%	4.25%	4.68%	5.04%	4.61%	4.82%
Boys w/ a *control group* female	2.5%	2.47%	2.43%	2.76%	3.04%	2.69%
Girls w/ an eligible female	4.7%	4.26%	4.52%	5.39%	4.58%	4.92%
Girls w/ a *control group* female	2.17%	2.4%	2.31%	2.65%	2.79%	2.5%
Children w/ an newly eligible person	4.43%	4.43%	4.66%	5.4%	5.28%	4.93%
Boys w/ a newly eligible person	4.53%	4.45%	4.57%	5.43%	5.51%	4.98%
Girls w/ a newly eligible person	4.32%	4.4%	4.75%	5.36%	5.04%	4.87%
Children w/ an old eligible person	6.61%	5.83%	6.59%	6.82%	6.55%	6.45%
Boys w/ an old eligible person	6.54%	5.82%	6.73%	6.62%	6.84%	6.58%
Girls w/ an old eligible person	6.69%	5.84%	6.44%	7.03%	6.23%	6.3%

Table 5: Literacy

	Before reform			After reform		
	1988	1989	1990	1992	1993	1995
Children w/ an eligible person	51.58%	54.76%	56.39%	57.4%	58.82%	65.07%
Children w/ a *control group* person	58.42%	59.82%	62.23%	61.92%	62.64%	68.2%
Other children	51.66%	52.98%	53.8%	57.37%	60.07%	63.36%
Boys w/ an eligible person	46.99%	52.14%	51.72%	51.13%	54.33%	59.1%
Girls w/ an eligible person	56.39%	57.65%	61.44%	63.87%	63.99%	71.71%
Children w/ an eligible male	49.41%	51.2%	53.44%	55.41%	57.41%	62.08%
Boys w/ an eligible male	44.37%	49.63%	48.15%	48.24%	53.53%	56.38%
Girls w/ an eligible male	54.87%	53.07%	58.98%	63.34%	61.96%	68.29%
Children w/ an eligible female	54.26%	58.39%	59.54%	57.28%	60.77%	66.28%
Boys w/ an eligible female	49.73%	55.15%	55.74%	51.35%	56.85%	59.05%
Girls w/ an eligible female	58.8%	61.74%	63.81%	63.12%	65%	73.85%
Children w/ a newly eligible person	54.38%	56.29%	58.66%	59.68%	62.5%	65.63%
Children w/ an old eligible person	49.2%	53.5%	54.47%	55.36%	55.57%	64.52%

Note: % of *literate* children (≥ 5 and ≤ 15)

Table 6: Attendance

	Before reform			After reform		
	1988	1989	1990	1992	1993	1995
Children w/ an eligible person	58.16%	58.42%	59.41%	64.51%	66.07%	71.51%
Children w/ a *control group* person	58.72%	58.19%	60.06%	60.85%	63.04%	70.64%
Other children	64.92%	65.1%	64.51%	68.02%	72.63%	76.14%
Boys w/ an eligible person	55.91%	57.54%	55.55%	60.06%	62.05%	68.12%
Girls w/ an eligible person	60.52%	59.39%	63.59%	69.12%	70.69%	75.28%
Children w/ an eligible male	58.15%	55.21%	60.15%	64.26%	65.83%	70.6%
Boys w/ an eligible male	57.71%	54.06%	56.06%	61.37%	63.38%	69.35%
Girls w/ an eligible male	58.61%	56.58%	64.44%	67.46%	68.7%	71.95%
Children w/ an eligible female	59.69%	61.68%	60.86%	65.77%	66.84%	72.51%
Boys w/ an eligible female	56.78%	62.02%	56.91%	61.44%	62.5%	67.43%
Girls w/ an eligible female	62.61%	61.34%	65.3%	70.04%	71.52%	77.84%
Children w/ a newly eligible person	53.98%	56.38%	58.59%	61.75%	62.92%	69.01%
Children w/ an old eligible person	61.55%	60.27%	60.12%	67.06%	68.75%	73.69%

Note: % of children (≥ 5 and ≤ 15) attending school

Table 7: Health Care

	Before reform 1986	After reform 1998
Everyone w/ an eligible person	8.77%	11.11%
Everyone w/ *control group*	7.46%	9.47%
Other people	7.76%	9.1%
Boys w/ an eligible person	6.73%	8.3%
Girls w/ an eligible person	10.82%	14.15%
Middle-agers w/ an eligible person	8.04%	11.29%
Everyone w/ an eligible male	7.42%	9.37%
Boys w/ an eligible male	6.46%	8.31%
Girls w/ an eligible male	8.78%	11.19%
Everyone w/ an eligible female	8.83%	11.21%
Boys w/ an eligible female	5.4%	6.98%
Girls w/ an eligible female	11.37%	14.34%
Everyone w/ an old eligible person	8.52%	10.97%
Everyone w/ a newly eligible person	9.02%	11.24%
Middle-agers w/ a newly eligible person	6.03%	12.88%
Middle-agers w/ an old eligible person	8.83%	10.82%

Note: % of people that looked for heath care in the past 2 weeks.

Table 8: Illness

	Before reform 1986	After reform 1998
Everyone w/ an eligible person	9.47%	8.34%
Everyone w/ *control group*	7.19%	6.94%
Other people	6.43%	5.07%
Boys w/ an eligible person	8.61%	7.72%
Girls w/ an eligible person	10.34%	9.01%
Middle-agers w/ an eligible person	10.34%	9.00%
Everyone w/ an eligible male	9.08%	7.57%
Boys w/ an eligible male	9.5%	8.58%
Girls w/ an eligible male	8.47%	5.83%
Everyone w/ an eligible female	9.35%	8.15%
Boys w/ an eligible female	7.17%	5.53%
Girls w/ an eligible female	10.96%	10.08%
Everyone w/ an old eligible person	8.48%	6.89%
Everyone w/ a newly eligible person	10.26%	9.23%
Middle-agers w/ a newly eligible person	13.03%	7.21%
Middle-agers w/ an old eligible person	10.68%	7.10%

Note: % of people that claimed had had any health problem in the past 2 weeks.

Table 9: Dependent Variable = Total Family Income (in logs)

Variable	**Coefficient** (Robust Std. Err.)
Male $Treatment_1$	-0.133***
	(0.0205)
Male $Treatment_2$	-0.11***
	(0.0158)
Male C. Group	-0.077***
	(0.0181)
Fem. $Treatment_1$	-0.074***
	(0.0149)
Fem. $Treatment_2$	-0.079***
	(0.014)
Fem. C.Group	-0.052***
	(0.0164)
Post	-0.125***
	(0.0068)
Male $Treat._1 \times$ *Post* ($\beta^m_{4,i=1}$)	0.151***
	(0.0262)
Male $Treat._2 \times$ *Post* ($\beta^m_{4,i=2}$)	0.302***
	(0.0166)
Male C. Group $\times$ *Post* (β^m_5)	-0.033
	(0.0253)
Female $Treat._1 \times$ *Post* ($\beta^f_{4,i=1}$)	0.232***
	(0.019)
Female $Treat._2 \times$ *Post* ($\beta^f_{4,i=2}$)	0.443***
	(0.016)
Female C. Group $\times$ *Post* (β^f_5)	0.041*
	(0.023)
Head's age	0.041***
	(0.0011)
Head's age squared	-0.0003***
	(0.00001)
Head's schooling	0.138***
	(0.0013)
Head's gender	0.242***
	(0.0105)
Family size	0.227***
	(0.0026)
# Childr. ≤ 14 *in the fam.*	-0.251***
	(0.0033)
Head is white	0.215***
	(0.0058)
N	87,386
R^2	0.293

Table 10: Schooling - Children ∈ [6,18]

	All sample		Boys		Girls	
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
	Literacy	Attendance	Literacy	Attendance	Literacy	Attendance
Male Treatment$_1$	-0.067***	-0.08***	-0.068***	-0.062***	-0.064***	-0.095***
	(0.0089)	(0.0096)	(0.0124)	(0.0129)	(0.0126)	(0.0145)
Male Treatment$_2$	-0.097***	-0.079***	-0.095***	-0.056***	-0.097***	-0.1***
	(0.0081)	(0.0087)	(0.0114)	(0.0116)	(0.0114)	(0.0131)
Male C. Group	-0.016**	-0.047***	-0.016*	-0.035***	-0.012	-0.055***
	(0.0065)	(0.0073)	(0.0093)	(0.0098)	(0.009)	(0.0111)
Fem. Treatment$_1$	-0.041***	-0.017**	-0.046***	-0.021**	-0.036***	-0.012
	(0.0074)	(0.008)	(0.0104)	(0.0106)	(0.0104)	(0.0122)
Fem. Treatment$_2$	-0.031***	-0.008	-0.03**	0.019	-0.032**	-0.034**
	(0.0081)	(0.0091)	(0.0118)	(0.0125)	(0.0111)	(0.0133)
Fem. C.Group	-0.021***	-0.005	-0.02**	0.004	-0.02**	-0.016
	(0.0061)	(0.0068)	(0.0086)	(0.0089)	(0.0086)	(0.0104)
Post	0.04***	0.063***	0.036***	0.06***	0.045***	0.066***
	(0.0024)	(0.0027)	(0.0035)	(0.0038)	(0.0034)	(0.0038)
Male Treat.$_1\times$ *Post* ($\beta^m_{4,i=1}$)	0.026**	0.027**	0.019	0.032*	0.036**	0.022
	(0.0119)	(0.0131)	(0.0168)	(0.0175)	(0.0167)	(0.0197)
Male Treat.$_2\times$ *Post* ($\beta^m_{4,i=2}$)	0.014	0.026**	0.015	0.029*	0.014	0.024
	(0.0108)	(0.0116)	(0.0151)	(0.0155)	(0.0152)	(0.0173)
Male C. Group $\times$ *Post* (β^m_5)	-0.015	0.002	-0.01	-0.002	-0.022*	0.005
	(0.0092)	(0.0105)	(0.0131)	(0.0139)	(0.0127)	(0.0159)
Female Treat.$_1\times$ *Post* ($\beta^f_{4,i=1}$)	0.007	0.002	0.006	-0.009	0.009	0.016
	(0.0101)	(0.0111)	(0.0144)	(0.0148)	(0.014)	(0.0166)
Female Treat.$_2\times$ *Post* ($\beta^f_{4,i=2}$)	-0.008	-0.004	-0.017	-0.027	0.004	0.022
	(0.0117)	(0.0126)	(0.017)	(0.0174)	(0.0158)	(0.0182)
Female C. Group $\times$ *Post* (β^f_5)	-0.018**	-0.009	-0.022*	-0.017	-0.013	0.003
	(0.0086)	(0.0095)	(0.0121)	(0.0125)	(0.0119)	(0.0146)
Age	0.256***	0.211***	0.232***	0.216***	0.28***	0.203***
	(0.0018)	(0.0022)	(0.0025)	(0.003)	(0.0024)	(0.0032)
Age^2	-0.008***	-0.01***	-0.008***	-0.01***	-0.009***	-0.01***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Gender (male=1)	-0.082***	-0.036***	–	–	–	–
	(0.0022)	(0.0024)	–	–	–	–
Head's age	0.002***	0.003***	0.001***	0.001***	0.002***	0.004***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Head's Schooling	0.044***	0.032***	0.05***	0.035***	0.038***	0.029***
	(0.0005)	(0.0005)	(0.0007)	(0.0007)	(0.0007)	(0.0007)
Head's gender	0.045***	0.012***	0.053***	0.03***	0.037***	-0.008
	(0.0041)	(0.0044)	(0.006)	(0.0062)	(0.0055)	(0.0062)
Family size	0.008***	0.007***	0.006***	0.003**	0.009***	0.011***
	(0.0009)	(0.001)	(0.0013)	(0.0014)	(0.0012)	(0.0014)
# Childr. ≤ 14 *in the fam.*	-0.038***	-0.012***	-0.04***	-0.012***	-0.035***	-0.011***
	(0.0011)	(0.0013)	(0.0016)	(0.0017)	(0.0016)	(0.0019)
White	0.13***	0.008***	0.145***	0.012***	0.113***	0.001
	(0.0023)	(0.0025)	(0.0033)	(0.0035)	(0.0031)	(0.0036)
N	134,633	134,633	69,907	69,907	64,726	64,726

Table 11: Schooling - Children ∈ [6,14]

	All sample		Boys		Girls	
	(1a) Literacy	(1b) Attendance	(2a) Literacy	(2b) Attendance	(3a) Literacy	(3b) Attendance
Male Treat.$_1$× *Post* ($\beta^m_{4,i=1}$)	0.035**	0.039**	0.024	0.052**	0.049**	0.026
	(0.0161)	(0.0172)	(0.0227)	(0.0243)	(0.0226)	(0.0243)
Male Treat.$_2$× *Post* ($\beta^m_{4,i=2}$)	0.007	0.027*	0.008	0.021	0.007	0.034
	(0.0136)	(0.0143)	(0.0192)	(0.0203)	(0.0191)	(0.0202)
Male C. Group × *Post* (β^m_5)	-0.026**	-0.011	-0.02	-0.004	-0.031*	-0.018
	(0.0125)	(0.0133)	(0.0179)	(0.0185)	(0.0173)	(0.0191)
Female Treat.$_1$× *Post* ($\beta^f_{4,i=1}$)	0.013	-0.009	0.018	-0.012	0.009	-0.004
	(0.0144)	(0.0153)	(0.0204)	(0.0218)	(0.02)	(0.0213)
Female Treat.$_2$× *Post* ($\beta^f_{4,i=2}$)	-0.01	0.015	-0.02	-0.016	-0.001	0.046**
	(0.0145)	(0.015)	(0.0214)	(0.0219)	(0.0197)	(0.0205)
Female C. Group × *Post* (β^f_5)	-0.02*	-0.021*	-0.025	-0.029	-0.017	-0.012
	(0.0119)	(0.0124)	(0.0169)	(0.0173)	(0.0165)	(0.0179)
N	93,539	93,539	47,973	47,973	45,566	45,566

Table 12: Schooling - Children ∈ [14,18]

	All sample		Boys		Girls	
	(1a) Literacy	(1b) Attendance	(2a) Literacy	(2b) Attendance	(3a) Literacy	(3b) Attendance
Male Treat.$_1$× *Post* ($\beta^m_{4,i=1}$)	0.02	0.02	0.022	0.022	0.017	0.018
	(0.0178)	(0.0198)	(0.0251)	(0.0246)	(0.0237)	(0.0328)
Male Treat.$_2$× *Post* ($\beta^m_{4,i=2}$)	0.029	0.029	0.032	0.052**	0.026	-0.001
	(0.0177)	(0.019)	(0.0247)	(0.0234)	(0.0244)	(0.0314)
Male C. Group × *Post* (β^m_5)	0.006	0.022	0.007	0.004	0.001	0.048*
	(0.0135)	(0.0163)	(0.0193)	(0.0203)	(0.0173)	(0.0269)
Fem. Treat.$_1$× *Post* ($\beta^f_{4,i=1}$)	0.01	0.018	0.003	0.002	0.022	0.042
	(0.0145)	(0.016)	(0.0207)	(0.0199)	(0.0192)	(0.0263)
Fem. Treat.$_2$× *Post* ($\beta^f_{4,i=2}$)	0.001	-0.045**	-0.01	-0.049*	0.024	-0.036
	(0.0193)	(0.0225)	(0.0278)	(0.0284)	(0.0251)	(0.036)
Fem. C. Group × *Post* (β^f_5)	-0.006	0.012	-0.009	0.002	-0.001	0.028
	(0.0124)	(0.0144)	(0.0175)	(0.0179)	(0.0166)	(0.024)
N	41,094	41,094	21,934	21,934	19,160	19,160

Table 13: F-Tests - Schooling

	Entire sample		Boys		Girls	
	(1a) Literacy	(1b) Attendance	(2a) Literacy	(2b) Attendance	(3a) Literacy	(3b) Attendance
Panel A: Children $\in$ [6,18]						
$(H_0 : \beta^m_{4,i=1} = \beta^m_5)$	8.31***	2.59	2.08	3.59**	8.68***	0.49
$(H_0 : \beta^m_{4,i=2} = \beta^m_5)$	4.59**	2.64	1.80	3.41**	3.67**	0.75
$(H_0 : \beta^m_{4,i=1} = \beta^m_{4,i=2}$	0.61	0.00	0.02	0.02	1.04	0.01
$(H_0 : \beta^f_{4,i=1} = \beta^f_5)$	4.10**	0.61	2.49	0.23	1.82	0.39
$(H_0 : \beta^f_{4,i=2} = \beta^f_5)$	0.50	0.08	0.05	0.23	0.75	0.68
$(H_0 : \beta^f_{4,i=1} = \beta^f_{4,i=2}$	0.93	0.14	1.11	0.71	0.08	0.07
$(H_0 : \beta^m_{4,i=1} = \beta^f_{4,i=1} \cap \beta^m_{4,i=2} = \beta^f_{4,i=2})$	2.34	3.69**	1.45	6.12***	1.00	0.03
Panel B: Children $\in$ [6,14]						
$(H_0 : \beta^m_{4,i=1} = \beta^m_5)$	9.96***	5.85**	2.64	3.86**	8.80***	2.21
$(H_0 : \beta^m_{4,i=2} = \beta^m_5)$	3.58*	4.02**	1.32	0.95	2.36	3.81**
$(H_0 : \beta^m_{4,i=1} = \beta^m_{4,i=2}$	1.91	0.34	0.32	1.08	2.24	0.08
$(H_0 : \beta^f_{4,i=1} = \beta^f_5)$	3.72*	0.48	3.06*	0.47	1.09	0.09
$(H_0 : \beta^f_{4,i=2} = \beta^f_5)$	0.31	3.58*	0.04	0.23	0.37	4.67**
$(H_0 : \beta^f_{4,i=1} = \beta^f_{4,i=2}$	1.36	1.27	1.72	0.02	0.13	2.98
$(H_0 : \beta^m_{4,i=1} = \beta^f_{4,i=1} \cap \beta^m_{4,i=2} = \beta^f_{4,i=2})$	1.32	2.69*	0.49	3.82**	1.00	0.13
Panel C: Children $\in$ [14,18]						
$(H_0 : \beta^m_{4,i=1} = \beta^m_5)$	0.45	0.00	0.26	0.39	0.34	0.59
$(H_0 : \beta^m_{4,i=2} = \beta^m_5)$	1.18	0.09	0.73	2.76*	0.78	1.59
$(H_0 : \beta^m_{4,i=1} = \beta^m_{4,i=2}$	0.15	0.12	0.10	0.89	0.09	0.21
$(H_0 : \beta^f_{4,i=1} = \beta^f_5)$	0.95	0.11	0.27	0.00	1.02	0.19
$(H_0 : \beta^f_{4,i=2} = \beta^f_5)$	0.11	4.72**	0.00	2.47	0.66	2.30
$(H_0 : \beta^f_{4,i=1} = \beta^f_{4,i=2}$	0.15	5.59**	0.16	2.36	0.00	3.30*
$(H_0 : \beta^m_{4,i=1} = \beta^f_{4,i=1} \cap \beta^m_{4,i=2} = \beta^f_{4,i=2})$	0.76	2.53	1.02	4.21**	0.00	0.02

*** Significant at 1%; ** Significant at 5%; * Significant at 10%

Table 14: Schooling - Intent-to-treat × Treatment (Schooling)

	All sample		Boys		Girls	
	(1a) Literacy	(1b) Attendance	(2a) Literacy	(2b) Attendance	(3a) Literacy	(3b) Attendance
Panel A: Intent-to-Treat						
Male Treat.× Post (β_4^m)	0.021**	0.025***	0.018	0.028**	0.026**	0.022
	(0.0084)	(0.0091)	(0.0119)	(0.0122)	(0.0118)	(0.0137)
Male C. Group × Post (β_5^m)	-0.014	0.002	-0.009	-0.002	-0.022*	0.005
	(0.0091)	(0.0104)	(0.013)	(0.0138)	(0.0126)	(0.0158)
Female Treat.× Post (β_4^f)	0.003	0.001	-0.002	-0.013	0.009	0.019
	(0.0081)	(0.0088)	(0.0116)	(0.0119)	(0.011)	(0.0129)
Female C. Group × Post (β_5^f)	-0.018**	-0.008	-0.022*	-0.017	-0.013	0.004
	(0.0086)	(0.0095)	(0.0121)	(0.0125)	(0.0118)	(0.0145)
Panel B: Treatment						
Male Treat.× Post (β_4^m)	0.018*	0.025**	0.012	0.029**	0.026*	0.033*
	(0.0105)	(0.0113)	(0.0148)	(0.0152)	(0.0146)	(0.0168)
Male C. Group × Post (β_5^m)	0.013	0.012	0.018	0.018	0.006	0.005
	(0.0082)	(0.0091)	(0.0115)	(0.0121)	(0.0114)	(0.0138)
Female Treat.× Post (β_4^f)	0.015	0.018	0.008	0.001	0.025*	0.038**
	(0.0103)	(0.0113)	(0.0149)	(0.0154)	(0.0141)	(0.0165)
Female C. Group × Post (β_5^f)	-0.018**	-0.01	-0.02*	-0.022*	-0.014	0.007
	(0.0077)	(0.0084)	(0.0108)	(0.0112)	(0.0106)	(0.0128)
N	134,633	134,633	69,907	69,907	64,726	64,726

Table 15: Bargaining effect on Attendance

	All sample	Boys	Girls
	(1)	(2)	(3)
Family Income (in logs)	0.022***	0.025***	0.019***
	(0.0017)	(0.0024)	(0.0024)
Male pension / f. income	0.039***	0.048**	0.03
	(0.015)	(0.0213)	(0.0213)
Female pension / f. income	0.034**	0.031	0.034
	(0.0169)	(0.0249)	(0.0231)
(H_0: Male - Female Share)	0.005	0.017	0.004
	(0.0345)	(0.0542)	(0.0478)
N	89,714	46,021	43,693

Table 16: Bargaining effect on Attendance - only treated families

	All sample	Boys	Girls
	(1)	(2)	(3)
Family Income (in logs)	0.04***	0.037***	0.042***
	(0.0056)	(0.0079)	(0.008)
Male pension / f. income	0.128***	0.134***	0.122***
	(0.0177)	(0.0251)	(0.0249)
Female pension / f. income	0.064***	0.067**	0.058**
	(0.0196)	(0.0286)	(0.0268)
(H_0: Male - Female Share)	0.064*	0.067**	0.064
	(0.0375)	(0.0312)	(0.0457)
N	12,000	6,120	5,880

Table 17: Bargaining effect on Attendance - families with eligible male and female

	All sample	Boys	Girls
	(1)	(2)	(3)
Family Income (in logs)	0.042***	0.045***	0.039**
	(0.0124)	(0.0174)	(0.0177)
Male pension / f. income	0.107***	0.163***	0.05
	(0.0365)	(0.0534)	(0.05)
Female pension / f. income	0.067	0.06	0.067
	(0.0499)	(0.0732)	(0.0678)
(H_0: Male - Female Share)	0.040	0.103**	-0.017
	(0.0564)	(0.0497)	(0.0785)
N	2,496	1,281	1,215

Table 18: Health

	Entire sample		Middle-agers		Children		Boys		Girls	
	(1a) Illness	(1b) H.C.	(2a) Illness	(2b) H.C.	(3a) Illness	(3b) H.C.	(4a) Illness	(4b) H.C.	(5a) Illness	(5b) H.C.
Male Treatment$_1$	0.000	0.002	0.026	0.022	-0.007	-0.007	-0.008	-0.014	-0.007	0
	(0.0053)	(0.0053)	(0.0216)	(0.0229)	(0.0084)	(0.0077)	(0.0118)	(0.0098)	(0.0121)	(0.0119)
Male Treatment$_2$	0.002	-0.005	0.05**	0.009	-0.007	-0.001	0	0	-0.015	-0.002
	(0.0051)	(0.0049)	(0.0198)	(0.0196)	(0.0079)	(0.0075)	(0.0117)	(0.0102)	(0.0107)	(0.0109)
Male C. Group	0.002	-0.009**	0.013	0.024	0.004	-0.007	0	-0.002	0.008	-0.013
	(0.0043)	(0.0043)	(0.0147)	(0.017)	(0.0072)	(0.0062)	(0.0099)	(0.0093)	(0.0104)	(0.0082)
Fem. Treatment$_1$	-0.004	0.004	-0.002	-0.062**	-0.015**	0.01	-0.011	0.01	-0.018*	0.01
	(0.0044)	(0.0044)	(0.0266)	(0.0198)	(0.0075)	(0.0083)	(0.0112)	(0.0113)	(0.01)	(0.012)
Fem. Treatment$_2$	-0.008	-0.007	-0.021	-0.032**	-0.003	0.008	0.009	0.003	-0.017*	0.015
	(0.0048)	(0.0048)	(0.0154)	(0.0159)	(0.0079)	(0.0079)	(0.0118)	(0.0101)	(0.0101)	(0.0123)
Fem. C.Group	-0.001	0.007	0.004	-0.016	-0.007	0.007	-0.015	0.006	0	0.008
	(0.0041)	(0.0042)	(0.0137)	(0.0144)	(0.0068)	(0.0069)	(0.0091)	(0.0095)	(0.0102)	(0.0099)
Post	-0.015***	-0.002	-0.003	0.002	-0.024***	0.004	-0.025***	0.005	-0.023***	0.003
	(0.0017)	(0.002)	(0.0064)	(0.0072)	(0.0023)	(0.0028)	(0.0032)	(0.0039)	(0.0034)	(0.0041)
Male Treat.$_1\times$ *Post* ($\beta^m_{4,i=1}$)	-0.006	0.001	-0.042	0.026	0.004	0.013	0.006	0.012	0.003	0.014
	(0.0071)	(0.0079)	(0.0272)	(0.0335)	(0.0116)	(0.0129)	(0.0161)	(0.0172)	(0.0165)	(0.0191)
Male Treat.$_2\times$ *Post* ($\beta^m_{4,i=2}$)	-0.009	0.004	-0.056**	-0.003	0.007	0.003	0.004	-0.003	0.011	0.01
	(0.0063)	(0.0063)	(0.0221)	(0.023)	(0.0098)	(0.0108)	(0.014)	(0.0144)	(0.0138)	(0.0164)
Male C. Group $\times$ *Post* (β^m_5)	0.001	0.012*	0.011	0.035	-0.013	0.016	-0.005	0.011	-0.02	0.021
	(0.0059)	(0.0063)	(0.02)	(0.0237)	(0.009)	(0.0103)	(0.0123)	(0.0147)	(0.013)	(0.0144)
Fem. Treat.$_1\times$ *Post* ($\beta^f_{4,i=1}$)	0.001	0.01	-0.008	-0.005	0.007	-0.001	0	0.005	0.014	-0.006
	(0.0058)	(0.0063)	(0.0343)	(0.0288)	(0.0099)	(0.0124)	(0.0142)	(0.0175)	(0.0139)	(0.0176)
Fem. Treat.$_2\times$ *Post* ($\beta^f_{4,i=2}$)	0.002	0.012*	0.006	0.015	-0.002	-0.014	-0.011	-0.01	0.01	-0.019
	(0.0065)	(0.0066)	(0.0191)	(0.021)	(0.0103)	(0.0112)	(0.015)	(0.0146)	(0.0138)	(0.0171)
Fem. C. Group $\times$ *Post* (β^f_5)	0.009	-0.004	0.018	0.005	-0.001	-0.021**	0.006	-0.02	-0.008	-0.02
	(0.0056)	(0.0062)	(0.0144)	(0.016)	(0.0086)	(0.01)	(0.0116)	(0.0135)	(0.0129)	(0.0147)

Table 18: Health - cont.

	Entire sample		Middle-agers		Children		Boys		Girls	
	(1a) Illness	(1b) H.C.	(2a) Illness	(2b) H.C.	(3a) Illness	(3b) H.C.	(4a) Illness	(4b) H.C.	(5a) Illness	(5b) H.C.
Age	-0.001***	0.000***	0.014	-0.048**	-0.007***	-0.021***	-0.007***	-0.021***	-0.006***	-0.020***
	(0.0001)	(0.0001)	(0.0182)	(0.021)	(0.001)	(0.0012)	(0.0013)	(0.0016)	(0.0014)	(0.0017)
Age^2	0.000***	0.000***	0.000	0.001**	0.000***	0.001***	0.000***	0.001***	0.000*	0.001***
	(0)	(0.0000)	(0.0002)	(0.0002)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Head's age	0.000	-0.001***	0.000	0.000	0.000*	0.000**	0.000	0.000	0.000*	0.000
	(0.0001)	(0.0001)	(0.0005)	(0.0005)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Head's Schooling	-0.002***	0.004***	-0.004***	0.004***	-0.001***	0.005***	-0.002**	0.004***	-0.001**	0.006***
	(0.0003)	(0.0003)	(0.0009)	(0.0011)	(0.0004)	(0.0005)	(0.0005)	(0.0007)	(0.0006)	(0.0008)
Head's gender	-0.011***	0.004	-0.027**	-0.010	-0.008**	0.008*	-0.012**	0.011*	-0.005	0.004
	(0.0027)	(0.0029)	(0.0097)	(0.0105)	(0.0039)	(0.0043)	(0.0055)	(0.0058)	(0.0055)	(0.0063)
Family size	-0.005***	-0.005***	-0.003**	0.002	-0.003**	-0.003***	-0.002*	-0.003**	-0.003**	-0.003**
	(0.0005)	(0.0006)	(0.0017)	(0.002)	(0.0008)	(0.0009)	(0.0011)	(0.0012)	(0.0012)	(0.0013)
# *Childr.*≤ 14	0.006***	-0.001	0.005**	-0.005**	0.003**	-0.003**	0.002	-0.003*	0.004**	-0.004**
	(0.0007)	(0.0007)	(0.0024)	(0.0026)	(0.001)	(0.0011)	(0.0014)	(0.0015)	(0.0015)	(0.0015)
White	0.004*	0.009***	-0.004	0.010	0.008**	0.006	0.01**	0.01*	0.006	0.002
	(0.002)	(0.0025)	(0.0066)	(0.008)	(0.0028)	(0.0038)	(0.0039)	(0.0053)	(0.004)	(0.0054)
N	130,975	130,975	14,167	14,167	51,101	51,101	26,040	26,040	25,061	25,061

Table 19: F-Tests - Health

	Entire sample		Middle-agers		Children		Boys		Girls	
	(1a) Illness	(1b) H.C.	(2a) Illness	(2b) H.C.	(3a) Illness	(3b) H.C.	(4a) Illness	(4b) H.C.	(5a) Illness	(5b) H.C.
$(H_0 : \beta^m_{4,i=1} = \beta^m_5)$	0.58	1.18	2.82*	0.06	1.54	0.05	0.35	0	1.34	0.11
$(H_0 : \beta^m_{4,i=2} = \beta^m_5)$	1.53	0.74	5.66**	1.47	2.44	0.84	0.21	0.5	2.79*	0.27
$(H_0 : \beta^m_{4,i=1} = \beta^m_{4,i=2})$	0.16	0.09	0.17	0.54	0.05	0.36	0.02	0.46	0.15	0.02
$(H_0 : \beta^f_{4,i=1} = \beta^f_5)$	1.22	2.87*	0.49	0.08	0.58	1.79	0.14	1.49	1.58	0.43
$(H_0 : \beta^f_{4,i=2} = \beta^f_5)$	0.59	3.07*	0.28	0.14	0	0.18	0.93	0.26	0.94	0
$(H_0 : \beta^f_{4,i=1} = \beta^f_{4,i=2})$	0.04	0.05	0.12	0.29	0.43	0.68	0.31	0.45	0.04	0.31
$(H_0 : \beta^m_{4,i=1} = \beta^f_{4,i=1} \cap \beta^m_{4,i=2} = \beta^f_{4,i=2})$	1.21	0.83	2.76*	0.05	0.06	1.29	0.36	0.16	0.09	1.44

*** Significant at 1%; ** Significant at 5%; * Significant at 10%

Table 20: Intent-to-treat × Treatment (Health)

	Entire sample		Middle-agers		Children		Boys		Girls	
	(1a) Illness	(1b) H.C.	(2a) Illness	(2b) H.C.	(3a) Illness	(3b) H.C.	(4a) Illness	(4b) H.C.	(5a) Illness	(5b) H.C.
Panel A: Intent-to-Treat										
M. Treat.× *Post* (β_4^m)	-0.007	0.001	-0.05***	-0.003	0.01	0.002	0.007	0	0.013	0.005
	(0.0052)	(0.0054)	(0.0184)	(0.0203)	(0.0081)	(0.0091)	(0.0114)	(0.0121)	(0.0116)	(0.0136)
M. C. Group × *Post* (β_5^m)	0.003	0.003	0.006	0.042*	-0.015*	0.012	-0.006	0.016	-0.025**	0.007
	(0.0052)	(0.0055)	(0.0186)	(0.022)	(0.0078)	(0.0089)	(0.0108)	(0.0127)	(0.0112)	(0.0124)
F. Treat.× *Post* (β_4^f)	-0.004	0.015***	-0.005	0.014	0.001	-0.001	-0.008	0.006	0.009	-0.009
	(0.0049)	(0.0052)	(0.0174)	(0.0184)	(0.0079)	(0.009)	(0.0113)	(0.012)	(0.0111)	(0.0134)
F. C. Group × *Post* (β_5^f)	0.005	-0.008	0.023	0.002	0.004	-0.014	-0.001	-0.023*	0.009	-0.005
	(0.005)	(0.0054)	(0.0145)	(0.0161)	(0.0081)	(0.0092)	(0.0109)	(0.0125)	(0.0122)	(0.0137)
Panel B: Treatment										
M. Treat.× *Post* (β_4^m)	-0.013**	0.006	-0.055**	-0.007	0.006	0.007	0	-0.001	0.013	0.016
	(0.0062)	(0.0063)	(0.0219)	(0.0229)	(0.0098)	(0.011)	(0.0138)	(0.0145)	(0.0138)	(0.0167)
M. C. Group × *Post* (β_5^m)	-0.001	0.004	-0.012	0.04*	-0.011	0.013	-0.004	0.017	-0.019*	0.009
	(0.0051)	(0.0055)	(0.0183)	(0.0214)	(0.0075)	(0.0085)	(0.0104)	(0.0123)	(0.0107)	(0.0119)
F. Treat.× *Post* (β_4^f)	0.002	0.017***	0.01	0.013	0	-0.002	0	0.014	-0.001	-0.017
	(0.0059)	(0.0062)	(0.0195)	(0.0216)	(0.0097)	(0.0107)	(0.014)	(0.0142)	(0.0132)	(0.016)
F. C. Group × *Post* (β_5^f)	0.005	-0.001	0.024	0.004	0.005	-0.016*	-0.003	-0.021*	0.014	-0.011
	(0.0048)	(0.0052)	(0.0146)	(0.0158)	(0.0077)	(0.009)	(0.0104)	(0.0123)	(0.0113)	(0.0131)
N	130,975	130,975	14,167	14,167	51,101	51,101	26,040	26,040	25,061	25,061

Table 21: Bargaining effect on Illness

	All sample	Middle-agers	Children
	(1)	(2)	(3)
Family Income (in logs)	-0.014***	-0.021***	-0.009***
	(0.0003)	(0.0009)	(0.0003)
Male pension / f. income	-0.002	-0.033**	0.006
	(0.0049)	(0.0163)	(0.0079)
Female pension / f. income	0.013**	-0.012	-0.001
	(0.0054)	(0.0203)	(0.0076)
(H_0: Male - Female Share)	-0.015*	-0.021**	0.007
	(0.0094)	(0.0112)	(0.0023)
N	126,208	13,704	48,483

Table 22: Bargaining effect on Illness - only treated families

	All sample	Middle-agers	Children
	(1)	(2)	(3)
Family Income (in logs)	-0.022***	-0.02***	-0.008***
	(0.0007)	(0.0025)	(0.0011)
Male pension / f. income	-0.009	-0.024	0.008
	(0.0055)	(0.0189)	(0.0088)
Female pension / f. income	0.005	-0.005	0.006
	(0.0062)	(0.0238)	(0.0089)
(H_0: Male - Female Share)	-0.014	-0.019*	0.002
	(0.0105)	(0.0119	(0.0231)
N	28,714	2,092	5,302

Table 23: Bargaining effect on Illness - families with eligible male and female

	All sample	Middle-agers	Children
	(1)	(2)	(3)
Family Income (in logs)	-0.024***	-0.025***	-0.009***
	(0.0012)	(0.0083)	(0.0025)
Male pension / f. income	-0.019**	-0.063	0.019
	(0.0084)	(0.0563)	(0.0197)
Female pension / f. income	0.039***	0.002	0.005
	(0.0123)	(0.0837)	(0.0238)
(H_0: Male - Female Share)	-0.058**	-0.065*	0.014
	(0.0249)	(0.0380)	(0.0523)
N	10,426	271	1,045