Household income as a determinant of child labor and school enrollment in Brazil: Evidence from a social security reform

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HOUSEHOLD INCOME AS A DETERMINANT OF CHILD LABOR AND SCHOOL ENROLLMENT IN BRAZIL: EVIDENCE FROM A SOCIAL SECURITY REFORM[1]

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Abstract: This paper studies the effects of household income on labor participation and school enrollment of children aged 10 to 14 in Brazil using a social security reform as a source of exogenous variation in household income. We find that increased benefits are associated with increases in school enrollment for girls, as well as a smaller reduction in their labor participation, but find no effects for boys. We also uncover evidence that the gender of the benefit receiver matters for girls' labor variables: only benefits received by females reduce girls' work.

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How do income increases affect school enrollment and child labor participation in developing countries? That is an important question for the design of policies to enhance the human capital of poor children and reduce inequality in developing countries. If school enrollment increases and entry to the labor market is delayed as incomes rise, targeted transfers to the poor may have long-lasting benefits besides the direct improvement in their consumption levels.[2] That is more likely to occur if the "luxury axiom", which posits that children only work when their family is unable to meet its basic needs, is valid (Basu and Van 1999). On the other hand, if children outcomes are insensitive to rising incomes (for instance, if child labor is related to factors other than low income levels), then policies such as enrollment subsidies, improvements in school quality and teacher pay, construction of new schools or conditional cash transfers might be more adequate at reducing child labor and increasing school enrollment than interventions that directly aiming to increase household income (without changing relative prices), such as pure cash transfers.

There is a large (and still expanding) literature on the determinants of child labor and school enrollment decisions in developing countries.[3] Much of this literature has inquired about the effects of increased incomes on child labor, typically finding that poverty or negative economic shocks are important factors driving children to work. Among other empirical works, Edmonds (2005) finds that seemingly permanent increases in income in Vietnam explain most of the observed decline in child labor; Yang (2007) finds that increased receipt of overseas remittances due to favorable exchange rate movements leads to enhanced human capital investment in the Philippines; Duryea, Lam and Levison (2007) find that child labor helps urban Brazilian families smoothing income during temporary unemployment spells of their adult male household head; and the literature has found positive effects on school enrollment of conditional cash transfer programs such as Bolsa Escola in Brazil (Bourguignon, Ferreira and Leite 2003) and Progresa in Mexico (Schultz 2004), while the latter is also related to a significant reduction in market work.[4]

This paper belongs to the same strand as the papers by Duflo (1999), Bertrand, Miller and Mullainathan (2003) and Ardington, Case and Hosegood (2009) which explored the effects of social pensions in South Africa on respectively children health outcomes and labor force participation of prime-aged males coresiding with pensioners. It is also directly related to the study by Edmonds (2006) on the effects of anticipated pension income in South Africa. That paper documents large changes in children outcomes when black South African families become eligible for pension benefits. Since receipt of those pensions is fully anticipatable, Edmonds argues that changes in school enrollment upon eligibility to those benefits are suggestive of the importance of credit constraints for the education decisions of those households.

This paper explores exogenous variation in social security income for rural workers in Brazil to estimate the impact of an exogenous increase in income on children's labor participation and school enrollment. The source of the exogenous increase was a reform to social security for rural workers which brought about a reduction in the minimum eligibility age for old-age benefits for rural workers from 65 to 60 for males and 55 for females; the end of a rule that determined that no more than one person per household would be eligible to receive old-age benefits for rural workers; and an increase in the size of benefits from half a minimum wage to one minimum wage.[5]

The paper identifies the effect of benefits on the schooling and labor outcomes of children of ages 10 to 14 by comparing the changes in the outcomes for children in households that benefited from the reform (broadly speaking, those with elderly rural workers older than the minimum age eligibility) with those who did not. Because the eligibility to the old-age program for rural workers is based on observable characteristics such as age, gender, and past or present occupation in rural activities, the reform lends itself well for a triple differences design. For the sake of explanation, consider now only the reduction in eligibility age for male rural workers from 65 to 60. A feasible difference-in-differences estimate would compare the time trends of benefits for those households whose oldest male is 60-64 years old (just became eligible) with those with a slightly younger oldest male (say, 55-59 years old). Since the reform affected only benefits for rural workers, one might isolate age-specific trends not related to the reform by comparing the difference-in-differences of rural and urban households, hence the triple differences design (also known as difference-in-differences-in-differences).[6]

This design can be then extended to accommodate the changes in old-age benefits for rural female workers and other changes in benefit receipts, such as the increase in benefits for the already eligible rural workers from ½ to 1 minimum wages in a regression framework. Our empirical model is thus an extension of the triple differences design. It includes dummies for after the reform, rural location (our proxy for rural occupation, past and present) and the age of the oldest male and female in the household; the interactions between after the reform and rural location, after the reform and the age of the oldest male and female, and rural location and the age of the oldest male and female (i.e. all first-level effects and their second-level interactions), while excluding all third-level interactions except the after X rural X age combinations that pin down a group gaining from the reform.

Since we identify exogenous variation in potential benefits for each household due to the reform, our estimates of the impact of the reform on children's outcomes have a causal interpretation in the sense of Angrist, Imbens and Rubin (1996). We interpret our estimates as reflecting the effect of benefits on the subset of households that stood to gain and actually gained from the reform (compliers in the jargon of the LATE theorem, Imbens and Angrist 1994). That is a relevant group for the question at hand because low school enrollment and high incidence of child labor are prevalent among children in rural areas.

This empirical strategy also overcomes some pervasive problems in the literature. Since the estimates are based on comparing changes over time between groups affected or not by the reform, they are not tainted by unobserved factors such as tastes for human capital investments that might be correlated with occupational choice, but stable over time. Cross-sectional studies relating child labor and household income are not able to identify the pure effect of income from the effects of unobserved characteristics that may be correlated with income. For instance, child labor and school enrollment may be correlated with household income because dynasties with more patience, greater ability, or a stronger taste for education will have adults with higher education levels and also higher income—an instance of omitted variable bias. Hence the cross-sectional relationship between household income and child labor is not informative of the likely effect of policies that transfer income to families with children, because it overestimates the effects of income transfers to poor families on children' labor participation and school enrollment decisions. On the other hand, measurement error in the benefits variable generates attenuation (downward) bias in the estimated effect of income on children outcomes. This problem may be particularly relevant in the early nineties Brazilian setting when inflation was high and variable; and also when variables correlated with income, such as family background and community characteristics, are included in the regression.

Finally, this paper explores the possibility that income received by females generates different outcomes as income received by males. In agreement with the previous results by Thomas, Schoeni and Strauss (1996), Duflo (1999) and Ponczek (2007), we find that some of girls' outcomes are sensitive to the gender of the benefit receiver. Point estimates suggest that boys' outcomes also differ depending on gender of the receiver, but the differences are statistically insignificant.

Section I presents background information about child labor in Brazil. Section II presents the empirical strategy. Section III describes the data used in the empirical sections. Section IV presents the regression results. Section V examines the possibility that selection biases are driving the results instead of causal effects, finding reassuring evidence in favor of a causal interpretation. Section VI concludes.

I. Background information about child labor in Brazil

Brazil has one of Latin America's highest rates of child labor force participation. In 1995, when participation rates for children aged 10-14 in Latin America and Caribbean (LAC) were 9.8 percent, the figures for Brazil were as high as 16 percent.[7] Brazil's high rate of child labor relative to LAC averages has persisted over decades: in 1950, when child labor participation rate was 19.4 percent for the LAC average, Brazil lagged behind with a 23.5 percent child labor participation rate. However, since the early nineties, the labor force participation for boys and girls aged 12 to 14 has trended downward, while the profile for children aged 10 and 11 has remained flat (Figure 1).

The majority of Brazilian child laborers work in agriculture activities. While only 24 percent of youths 10-24 are employed in agriculture, 69 percent of the work force aged 10-11 and 55 percent of the work force aged 12-14 are in that sector (IBGE, 1997). Therefore, effective policies to reduce child labor ought to change the incentives and constraints faced by rural families.

The mirror image of Brazil's high child labor rates of children is a dismal educational record (for a collection of papers on education and income inequality in Brazil, see Birdsall and Sabot 1996). Behrman and Schneider (1996) find that secondary school enrollment rates in Brazil are respectively 7.7 percent and 16.4 percent below their "expected values" for females and males after conditioning on income levels and measures of schooling cost. Figure 2 shows the time series of school enrollment rates for children 10 to 14. Enrollment rates for girls are in general higher than for boys; enrollment rates increased for all ages during the nineties, but more sharply for the older children (12 to 14 year olds); the opposing trends in child work and school enrollment for those age groups hint that those two activities compete for children's time.

Table 1 shows an increase of 7.8 percentage points in the proportion of children 10-14 going to school and not working for pay between 1989 and 1995 (these figures exclude the Northern region because its rural areas were then out of the PNAD reach). The proportion of children going to school and also working for pay declines slightly, adding up to a total increase of 6.9 percentage points in the proportion of children going to school. During that period, the proportion of children working for pay declined by about 4 percentage points, and the increase in the enrollment rates of children working for pay from 51 percent in 1989 to 69 percent in 1995.

But the institutional environment is somewhat at odds with the observed outcomes. Schooling is compulsory in Brazil up to age 14 or completion of the eighth grade.[8] Public schools are free. Moreover, they provide free meals. Work is only allowed for children 14 and older, with apprenticeship available at age 12. Hazardous activities are only available for youths older than 18, and for some activities, older than 21. Starting in 1996, the federal government instituted the Child Labor Eradication Program (*Programa de Erradicação do Trabalho Infantil*, or PETI) with a pilot program in the charcoal producing region of the state of Mato Grosso do Sul. By 1999, this program was expanded to eight other states.[9]

II. Description of the Social Security reform

The Brazilian social security reform of 1991 provides a unique opportunity to study the effect of exogenous changes in income on household economic choices, such as elderly labor participation, human capital investments on children or marriage and living arrangements of the elderly. Because this reform provides a source of exogenous variation in benefits that is not correlated with a family's demand for investment in human capital or disutility from child work, it can be used to identify the effect of exogenous income transfers on children outcomes, such as school enrollment and labor participation measures.

This reform reduced the minimum eligibility age for rural old-age benefits for men from 65 to 60, increased the minimum benefit paid to rural old-age beneficiaries from 50 percent to 100 percent of the minimum wage, extended old-age benefits to female rural workers who were not heads of households (thereby extending the benefits to the previously uncovered elderly wives of rural workers), and reduced the age at which women qualified for benefits from 60 to 55. Because old-age benefits for rural workers are not subject to either an earnings test or retirement requirement, once a rural worker reaches the minimum eligibility age, there is no gain in delaying filing the application for old-age benefits. The only requirement is to provide the proofs of age and past rural activity that validate his or her claim to eligibility (for a list of the valid sufficient proofs of past rural activity, see de Carvalho Filho, 2000a or 2008).

The effect of the reform on benefit income for each household thus depends on the age, gender, occupation, and marriage status of each household member. More specifically, based on those characteristics, we can identify the households which potentially benefited from the reform. Those are the ones with rural workers who became eligible due to reduction in the minimum eligibility age (males age 60-64, females age 55-59); with rural workers who benefited from the increase in benefits from ½ to 1 minimum wage (males older than 65, and females older than 60); and households with more than one rural worker of eligibility age.

For example, a household with a 66 year old male rural worker married to a 57 year old female rural worker was eligible to receive total benefits in the value of ½ of the minimum wage before the reform. After the reform, the husband would receive 1 minimum wage (because of the increase in the floor benefit) and so would the wife (because of the end of the one-person per household rule and the reduction in the minimum eligibility age).

The timing of the reform can be summarized as follows: In 1988, a new Constitution mandating that reforms be done on rural social security was promulgated, and it is plausible that more informed workers became aware of the future changes yet to be implemented. Immediately after July 1991, when the ordinary law making the constitutionally mandated reform operational was passed (Law #8212/8213), benefit payments to rural beneficiaries of old-age pensions increased automatically in general from 50 to 100 percent of the minimum wage, and newly eligible rural workers (e.g. 60 to 64 year old males) began to apply for benefits. By September 1992, the month of reference of the 1992 household survey used in this paper, take-up of new benefits was still incomplete, either for bureaucratic reasons or because of delays in the spread of information. Finally, by September 1993, the month of reference of the 1993 survey, almost all of the take-up process had been completed and newly eligible workers were already receiving their benefits. In order to have a sharp comparison between pre- and post-reform, we therefore exclude the data for the 1992 survey because that is a transition year. Figure 3 shows the flow of newly granted old-age benefits over time. Although the change in the law happened in July 1991, there is no apparent increase in the yearly flow of new granted benefits before 1992, until a spike is apparent in 1993 and 1994 (*Anuário Estatístico da Previdência* 1998). The figure also shows that the yearly flow of new disability benefits shrinks with the extension of old-age benefits, suggesting that disability and old-age benefits are substitutes for the age group affected by the reform. Workers in Brazil may also receive benefits based on length of service, but the tests required for this kind of benefit are in practice prohibitive for rural workers, as shown by the small or negligible flow of length-of-service benefits in Figure 3 (only about ½ percent of all rural benefits are of the length-of-service type).

III. Data

We use the *Pesquisa Nacional por Amostra de Domicílios*, or PNAD, to make inferences about the labor participation and school enrollment outcomes of 10 to 14 year old children. The PNAD is an annual household survey with sample size of about 1/500 of the Brazilian population (about 100,000 households), designed to produce a picture of the living conditions and economic life of the Brazilian population, rural and urban. For every individual, we observe characteristics such as age, race, education, school enrollment, income from different sources, housing and living arrangements, family structure, work, fertility, migration and other topics. We observe various measures of labor supply, including hours of work, labor force non-participation and earnings.

Work related questions are asked about all individuals over 10 years old. To identify rural workers, the survey allows one to observe every worker's current occupation if any, or past occupation up to a four years recall, but because the empirical strategy in this paper does not depend on the exact assignment of occupation to each observation, we use rural location as a proxy for rural occupation.

The empirical exercise will use data for the years 1989, 1990, 1993 and 1995. The PNAD survey was not carried out in 1991. The reform was in effect after that. Data for years previous to 1989 may bring confounding factors because 1988 was a year of major changes in labor regulations due to the promulgation of the Constitution of 1988. The PNAD survey was not carried out in 1994. There are 134,350 observations for children 10 to 14 years old for the survey years 1989, 1990, 1993 and 1995. The lower limit of the age range is given by the survey design (the survey does not ask work related questions to younger children), while the upper limit represents the minimum age for legal work in Brazil (and is also in concordance with the ILO definition of child labor). Of those, we exclude the children who are heads of households or their spouses, boarders, live-in domestic employees or the live-in children of domestic employees, which amount to 776 children or 0.6 percent of all observations. The remaining 133,574 observations are the children who are related to the head of the household or are "aggregated" to the head's family. Then, we exclude 2,168 observations from household with at least one person with 12 years of schooling because those more educated households may be very different from the households affected by the reform. After those exclusions, our sample size is 131,406 (2.2% less than our initial sample).

The PNAD classifies social security benefits into two broad categories: *aposentadorias,* which comprise disability, old age and length of service benefits, and *pensões,* which comprise military and survivors' income maintenance benefits. Throughout this paper we use the word 'benefits' to denote the *aposentadoria* receipts in the PNAD survey.[10] There are many large outliers for benefits, and to avoid undue influence of a few influential extreme observations on our results, we topcode household benefits at the 99 percentile of the household distribution (i.e. R$1130 per month).

The outcomes analyzed in this paper were chosen in order to capture different dimensions of children's work and schooling choices (detailed descriptions of these variables can be found in the Appendix). "Enrolled in school" measures school enrollment/attendance (see discussion in Appendix). The variable "Worked for pay in reference week" (in short, "worked for pay") intends to capture children's involvement in the wage economy.[11] "Total hours per week, all jobs" measures the time intensity of children's labor. In the 1989 and 1990 surveys, hours worked by unpaid workers less than 15 per week were coded as zero. To ensure consistency, hours of unpaid workers below 15 per week are recoded to zero in the 1993 and 1995 surveys.[12] All the benefits values are measured in Reais of January 2002 (when R$2.38=US$1.00), using the deflator for the PNAD derived by Courseil and Foguel (2002), and correcting for geographical differences in price levels as in Ferreira and Paes de Barros (1999). Table 2 presents the means and standard deviations of some of the variables used in this paper, by gender of the child and for five different subsamples: all, mature (households with at least one person older than 50), rural location, before the reform (1989 and 1990) and after the reform (1993 and 1995). It shows how the standard deviation of the benefit variable is reduced by one third when we cap it at the 99 percentile thereby reducing the influence of observations with extremely high benefits; how the average of benefits as a ratio to total income increased from 5.2 percent to 6.5 percent after the reform for the sample of households with a boy 10-14 year old; and how mature households differ from the general population (they receive more benefits, depend more on benefits as proportion of total income, have lower total income, lower school enrollment, higher child work, are more likely to be white, and headed by a woman).

The direct effects of the reform on household benefit income and take-up rates can be gauged in a triple differences framework—i.e. comparing the changes in the differences between eligible and near eligible households in rural and urban areas (de Carvalho Filho 2008). To define eligibility, we use the age of the oldest male and female on each household (as in Edmonds 2006). Table 3 presents estimates of the triple differences for total benefits and number of benefit receivers by gender, using the age of the oldest male and female in the household, illustrating how the implementation of the reform increased the benefit income for some specific groups of households relative to similar others. Focusing on male benefits (Table 3, left side of the top panel), we find that while male benefits for rural households whose oldest male is 60-64 (made eligible by the reform) increased by R$74.5 (in Reais of Jan. 2002) between 1989-90 (before) and 1993-95 (after), for rural households whose oldest male is 55-59 (those just younger than the newly eligible), male benefits increase by R$7.3—a difference in difference of R$67.2. For urban households, we find that there was a relative increase in average benefits for the 60-64 relative to the 55-59 households of R$29.2, partly because location of residence is an imperfect proxy for rural occupation (some rural workers live in areas classified as urban), but also because other modifications may have taken place in *aposentadoria* benefits for urban workers. The difference between the difference-in-differences for rural and urban households is the triple difference estimate of R$38.1 (statistically significant at the 1 percent level). The triple differences for number of males receiving benefits (Table 3, right side of the top panel) shows also a significant impact of the reform (an increase of 0.20 in the number of male benefit receivers per household).[13]

The bottom panel of Table 3 then presents the triple difference estimates based on the age of the oldest female, comparing the changes for the households whose oldest female is 55-59 (made eligible by the reform) and 50-54 year old (those just younger than the newly eligible). The patterns are similar—there is a increase in female benefits for the rural households whose oldest female is 55-59 relative to the 50-54 ones (by R$38.6), whereas the analogous figure for urban areas was R$12.6. The difference in the relative increase in benefits between rural and urban households is the triple difference estimate of R$26.0 (statistically significant at the 1 percent level). In line with what we found for male benefits, we find significant triple differences estimates for the number of female benefit receivers in the rural households whose oldest female is in the 55-59 year old bracket.

The same triple differences framework can be applied to children's outcomes. In Table 4 we pool together boys and girls age 10-14 and present triple differences estimates of the effect of the reform on school enrollment and work for pay. We find no evidence supporting the hypothesis that children outcomes improved for families with an elderly male gaining from the reform (Table 4, top panel). We also find larger point estimates of school enrollment increases and work for pay decreases for children coresiding with an elderly female benefiting from the reform, but standard errors are wide and we cannot reject the hypothesis of no effect in this very simple framework. In the remainder of the paper, we will extend this framework to account for other changes in rural old-age benefits (e.g. the increase in benefits for the already eligible). In summary, the evidence from triple differences estimates indicates a very strong first-stage relationship between the reform and benefits (the reform indeed happened!) but only some indication that the increase in benefits related to the reform caused improvements in school enrollment and work for pay for the households with an elderly female stood to gain from the reform.

IV. Empirical strategy and results

The use of variation in household social security income to identify the effect of exogenous income on child work and school enrollment requires adequate control for the effects of living with an elderly person unrelated to old-age benefits. From one single cross-section, one cannot identify the effects of non-labor income coming from old-age benefits from the very effect of the presence of an elderly person (e.g. elderly people testify for the importance of patience and investment in human capital, children from older parents are raised in a different manner than other children), or elderly presence changes the demand for children input to household production.

In this paper, we use a triple difference strategy exploring an exogenous reform in social security to separate the effects of benefits from the effects related to the presence of an elderly person.[14] Because the reform affects some children but not others, the effects of the reform can be identified by comparing the changes in average outcomes for children affected by the reform (i.e. living with elderly who became eligible or whose benefits increased) with those for other similar children in rural areas, and then subtracting from this difference-in-differences the similar statistic for urban area children (who are less likely to coreside with a rural elderly), thus obtaining a triple difference estimate. Below we formalize this strategy in a regression framework.

A. First-Stage: From the social security reform to actual benefits received

The triple differences strategy readily translates to a regression framework. The reduced form first-stage equation shows how the social security reform changed actual benefits received by the households in our sample. The first stage equation that we estimate is:

$$
\begin{aligned}
(1)\qquad Y = &\left(age^{m}_{6064} \times rural \times after\right)\beta^{m}_{new} + \left(age^{m}_{65up} \times rural \times after\right)\beta^{m}_{raise} + \\
&+\left(age^{f}_{5559} \times rural \times after\right)\beta^{f}_{new} + \left(age^{f}_{60up} \times rural \times after\right)\beta^{f}_{raise} + \\
&+\left(age^{m}_{5559} \times rural \times after\right)\phi^{m}_{fake} + \left(age^{f}_{5054} \times rural \times after\right)\phi^{f}_{fake} + \\
&+\theta_{rural} + \theta_{after} + \theta_{r\times a} + \sum_{m\in \mathrm{A}}\left(\theta_{m} + \theta_{r\times m} + \theta_{a\times m}\right) + \sum_{f\in \mathrm{A}}\left(\theta_{f} + \theta_{r\times f} + \theta_{a\times f}\right) + u
\end{aligned}
$$

In the equation above, *Y* is the outcome of interest (e.g. household benefit receipts; or number of benefit receivers in the household); β^{m}_{new} captures the interaction between oldest male in age group 60-64 in a rural location and after the reform—those are the observable characteristics of newly eligible males that were younger than the pre-reform eligibility age, but older than the post-reform one; β^{f}_{new} captures the similar effect for females; β^{m}_{raise} and β^{f}_{raise} capture the effect of the increase in the minimum benefit from ½ minimum wage to one minimum wage for those who were already eligible pre-reform (i.e. male rural workers older than 65; female rural workers older than 60); $\phi^{f}_{fake}, \phi^{m}_{fake}$ are dummies for the triple interaction between after, rural location and the age range immediately younger than the minimum age to benefit from the reform, and are expected to be jointly insignificant if our identification assumption is valid; θ_{rural} and θ_{after} are dummies for rural location and after the reform; $\theta_{r\times a}$ stands for the interaction between rural and after; the sequences $\{\theta_m\}$ and $\{\theta_f\}$ account for the age of the oldest male and female in the household; and $\{\theta_{r\times m}\}$, $\{\theta_{r\times f}\}$, $\{\theta_{a\times m}\}$, $\{\theta_{a\times f}\}$ stand for collection of dummies for interactions between age of oldest male (female) and rural location, and oldest male (female) and after the reform.

The estimates for the first stage regressions are presented in Table FS. Because we have categorical variables in our right-hand side, we group the observations in groups defined by rural location, time, age of the oldest male and age of the oldest female in order to get the right standard errors for this regression and the IV estimates (as suggested in Angrist and Pischke 2009, pp. 313-314). The triple interactions indicating the households potentially benefiting from the reform are of the expected sign and in general significant at the 1% level. The F-statistics for the joint significance of the instruments attest for their strength. Comparing before and after the reform, rural households whose oldest male member was aged 60-64 (newly eligible male rural workers) have 0.30 more benefit receivers, of which 0.23 are males, and their total monthly benefits are higher by R$55.70 (which implies an average increase in benefits conditional on one additional benefit receiver of R$188.18, very close to the average minimum wage at constant prices in the after period, R$164.87). For rural households whose oldest female is in the age range potentially benefiting from the lower retirement age, we also find a highly significant and sizeable increase in the number of pension (ages 55-59) and in benefit receipts. The presence of an elderly male already older than the minimum retirement age before the reform is associated in general with no effect on the number of benefit receivers, but significant effects on benefit levels (see β^{raise}_{m} in columns 1-3 and 5-8). We also present the results of the regression without the winsorizaton (topcoding) of the benefits variable in column (6) – as expected, the presence of extremely high observations for benefits reduces the F-stat for the joint significance of instruments, but they are still significant at any level. Lastly, we test the validity of our instruments by adding to the regression the interaction terms indicating those almost of age to gain from the reform (male_age$_{5559}$*after*rural and female_age$_{5054}$*after*rural) and their coefficient is both jointly and individually insignificant (columns 2,7), which lends credibility to our identification strategy.

B. Reduced form effect of reform on children outcomes

We can estimate reduced form effects of the reform on children outcomes by substituting children outcomes for the left-hand side variable in equation (1) and then test for the joint significance of the excluded instruments male_age$_{6064}$*after*rural, male_age$_{65up}$*after*rural, female_age$_{5559}$*after*rural and female_age$_{60up}$*after*rural. We find significant effects from potentially gaining from the reform on girls' enrollment (increase; based on F-test for joint significance of the excluded instruments) and work for pay (decrease; based on coefficients for excluded instruments tagging to increased female benefits) and no effect for boys' outcome (Table 6). We also find that the dummies for male_age$_{5559}$*after*rural and famele$_{5054}$*after*rural that indicate the presence of those elderly just younger than the minimum age to gain from the reform are jointly insignificant as one would expect if families faced credit constraints such that the timing of income affect children activities (Edmonds 2006).

Next we test the validity of our empirical strategy by applying it to variables that are not a priori supposed to be affected by the reform (e.g. dummy for black skin color); and to variables related to the household composition (whether the child in our data is the youngest one in the household and the number of children and adults in the household), and we find that our excluded instruments are jointly and individually insignificant for each one of those variables (Table 6, columns 7-10).

C. Second-stage: From benefits to children outcomes

In the second-stage regression, where we estimate the impact of the reform on school enrollment and child labor variables, we use the triple interactions between rural, after the reform and age of oldest male and female indicating the groups gaining from the reform (age$_{6064}$*after*rural, male_age$_{65up}$*after*rural, female_age$_{5559}$*after*rural and female_age$_{60up}$*after*rural) as the excluded variables that will instrument for the level or presence of benefits in the household. The equation we estimate is:

$$y = Y\alpha + \mu_{rural} + \mu_{after} + \mu_{r\times a} + \\ + \sum_{m\in A}\left(\mu_m + \mu_{r\times m} + \mu_{a\times m}\right) + \sum_{f\in A}\left(\mu_f + \mu_{r\times f} + \mu_{a\times f}\right) + v \tag{2}$$

In the equation above, y is the dependent variable (school enrollment or child labor variables), Y is either benefit receipts or number of benefit receivers in the household; μ_{rural} and μ_{after} are dummies for rural location and after the reform; $\mu_{r\times a}$ stands for the interaction between rural and after; and $\{\mu_{r\times m}\}$, $\{\mu_{r\times f}\}$, $\{\mu_{a\times m}\}$ and $\{\mu_{a\times f}\}$ are interactions between age of oldest male (female) and rural location and after the reform.

Table 7 presents the ordinary least-square estimates for the parameter α of equation (2). We estimate a positive coefficient on benefit levels for the school enrollment equation which implies a 2-2.5% increase in enrollment for R$100 in benefits and find no impact of benefits on the labor variables. We cannot claim causality for those results because we cannot preclude that there be an omitted variable correlated to both school enrollment and benefit levels, and we worry about attenuation bias, so we move next to IV estimates.

Equation (2) above was estimated for 3 different outcomes by instrumental variables, using benefit levels and number of benefit receivers as the endogenous variable, for boys and girls samples; lumping male and female benefits together or not. The over-identification tests for the validity of instruments lend credibility to our IV strategy. The null hypothesis of valid instrument could not be rejected at the 5 percent level in any specification, but it was rejected at the 10 percent level in one out of 24 specifications, which augurs well for the validity of our instruments. As in Angrist and Imbens (1995) and Kling (2000), we interpret the IV estimates based on the reform rules as the weighted average of causal effects for the subgroup affected by the reform – children living in households with a rural elderly, which we deem particularly interesting due to the high coincidence of rural activities,poverty and child vulnerability to child labor and dropping out of school.

Tables 8 presents the instrumental variable estimates for the parameter α in a sample of children 10 to 14 year old. In the first four columns, there are estimates for the effect of 100 Reais in benefits (in Reais of January 2002). We find different results for the effect of benefit levels for boys and girls enrollment—while a benefit increase of R$100 boosts girls' school enrollment by 9.7 percentage points (significant at the 1 percent level), it has no effect for boys. This pattern is confirmed by the estimates of the effect of the number of benefit receivers—one additional benefit receiver (for the subgroup affected by the reform) increases girls' enrollment probability by over 23 percentage points (53 percentage points when the benefit receiver is a female). When we inquire whether the identity of the benefit receiver matters, as in previous literature on violations of the unitary model of the household in Brazilian data (Thomas 1990; Thomas, Schoeni and Strauss 1996), we cannot reject the null that the gender of the benefit receiver does not matter for children school enrollment.

For the labor participation variables, the estimates imply that an increase of R$100 in total benefits in the household has no effect on the probability of having "worked in the reference week for pay" for boys, but it reduces work for pay by 3.6 percentage points for girls (but not statistically significant). For girls, we also found gender differences on the effect of benefits: girls' work for pay is reduced when there are female benefit receivers or female benefits are higher, but no statistical significant effect can be found for male benefits (moreover, the effect of male benefits has the opposite signs).

The estimates for hours of work in general show no reduction in hours by boys or girls when aggregating male and female benefits, but a reduction in girls' hours in response to an increase in female benefits (-5.6 hours for each additional R$100s). We also find that marginally significant differences in the effect of male and female benefits on girls' hours of work—as in the work for pay estimates, female benefits seem to be associated with reductions in hours, but not so male benefits.

D. Assessing the robustness of the estimates

Table 9 reports IV estimates of the effect of benefit levels on the outcomes of interest for different subsamples. In this table, columns (1)-(6) report the coefficient on total benefits for boys; columns (7)-(12) for girls. Each column represents a different subsample. Columns (1) and (7) reproduce the baseline result using the full sample as in Table IV, the other columns focus on the 5 different subsamples. The "mature" subsample attempts to restrict the sample to children in households similar to the ones benefitting from the reform by dropping all households without a resident older than 50 and whose residents have 12 years of schooling or more. The Northeastern subsample focuses interest in the region that is disproportionately represented in Brazil's poverty statistics. The rural sample keeps only households located in rural areas, hence the identification is centered on the differences across the age profile, and not also between rural and urban households. The 13-14 subsample focuses on the children at greater risk of working or dropping out of school. Finally, the *head less than 4* subsample includes only the households whose head spent less than 4 years in school – which allows us to examine the claim that income transfers to the poor might not increase school enrollment by itself because the demand for education of the least educated poor is income inelastic.

The different subsamples in general carry through the result that the level of old-age benefits have no effect on boys' enrollment and work variables, in line with the results in Table 8. For girls, we find that the effect of benefit levels on enrollment rates appears robust and, interestingly, its magnitude increases in the subsamples of children from the Northeastern region and whose head of household is less educated. However, we find that the positive relationship disappears when the sample is restricted to rural households – i.e. the finding on girls' enrollment relies on a comparison between differences-in-differences in rural and urban areas.

Finally, the sign and magnitude of the effect of benefit levels on girls' work variables appears reasonably robust (again, with the exception of the rural sample), yet not statistically significant.

VI. Causal Effects or Selection Bias?

The identification strategy of this paper depends on children not moving into (or away from) households receiving the new social security benefits, because children moving due to the reform may be systematically different from other children - therefore causing the estimates to be biased due to selection problems. For example, it is plausible that the decision to send a child to live with a grandparent after the increase in benefits might be correlated with unobserved characteristics such as preference for schooling.[15]

Unfortunately in the absence of panel data, it is hard to gauge the empirical relevance of the selection of children into and out of households. The data permits us to test the null hypothesis that there were no changes in the number of children living with elderly who benefitted from the reform. This can be done in a triple differences setup similar to the one we used to identify the effect of the reform on children outcomes and old-age benefits.

We estimate the equation:

(3) $$n_e\left(a,b\right)= \left(age^{m}_{6064}\times rural\times after\right)\chi^{m}_{new}+\left(age^{m}_{65up}\times rural\times after\right)\chi^{m}_{raise}+$$
$$+\left(age^{f}_{5559}\times rural\times after\right)\chi^{f}_{new}+\left(age^{f}_{60up}\times rural\times after\right)\chi^{f}_{raise}+$$
$$+\kappa_{rural}+\kappa_{after}++\sum_{m\in A}\left(\kappa_{m}+\kappa_{r\times m}+\kappa_{a\times m}\right)+\sum_{f\in A}\left(\kappa_{f}+\kappa_{r\times f}+\kappa_{a\times f}\right)+\omega_e$$

where $n_e(a,b)$ is the number of children of ages a through b living with elderly e; the κ coefficients capture the first and second level interactions between age, rural location and after the reform; and, finally, the χ coefficients are the reduced-form effect of the reform, as in equation (1), and are the coefficients of interest. The null hypothesis that the reform did not cause changes in the composition of the elderly households benefitting from the reform can be tested by testing the null that the χ coefficients are jointly equal to zero.

We present the results in Table 10. We fail to reject the null hypothesis that the reduced-form impact of the reform on the number of boys or girls coresiding with the elderly is nil. It is also reassuring that we were able to reject this null hypothesis when we substitute children of age 15-19 for the 10-14 bracket – this indicates that our failure to reject the null hypothesis for the 10-14 year olds may have to do with their actual behavior, not the test lacking statistical power.

Another source of comfort that selection problems might not be empirically relevant is the narrow time span covered by the data used in this paper. To the extent that changes in the age distribution of income affects residential choices within the extended family, this effect is likely stronger in the long run after social norms have adapted to the new circumstances. It is reasonable that the impact on residential decisions may be muted in the short-term, only a few years after the reform came into effect.

VI. Conclusions

This paper used variation in old-age benefits received by rural workers due to a constitutionally mandated reform in social security system to identify the effect of income on labor outcomes and school enrollment of children of ages 10-14 in Brazil. The adoption of this empirical strategy based on exogenous variation in old-age benefits is justified for several reasons. First, one cannot identify the effect of non-labor income in cross-sectional comparisons between income levels and children's outcomes owing to the confounding effects of other characteristics that are correlated with income. Second, because OLS estimates of the effect of benefits might suffer from attenuation bias because of classical measurement error in the income measures (so any finding of no effect could be dismissed on those grounds). Finally and more importantly, an IV strategy allows one to identify a causal parameter of interest for policy makers and researchers – in this case, the effect of the reform on the families with an elderly in the rural areas that could potentially gain and indeed gained from the reform (the compliers). That is arguably a relevant parameter because both child labor and shortfalls in school enrollment in developing countries are more prevalent in rural areas (Edmonds 2007).

We find that old-age benefits have the effect of increasing school enrollment of girls coresiding with old-age beneficiaries, particularly girls ages 13-14. Instrumental variables estimates imply that R$100 (In Reais of January 2002) of old-age benefits received by household members increases school enrollment rates of girls by 9.7 percent, with little or no effect for boys. There is also some evidence that increases in benefits have caused reductions in work for pay and work intensity for girls, but only for female benefits. Since male benefits appear to be irrelevant for girls' schooling and labor decisions, the results indicate the existence of differences by gender of receiver and perhaps tensions between male and female adults over girls' use of time, with consequences for the design of transfer policies. If one considers school enrollment a "good" and labor participation a "bad", then male benefits are "less of a good" for girls. These results are similar to the findings by Duflo (1999) that in South Africa social pensions received by grandmothers benefits granddaughters relatively more than if received by grandfathers, and also highlight the importance of developing a collective view of the household (Thomas, Shoeni and Strauss 1996; Browning and Chiappori 1998).

Because IV estimates in this paper are based on exogenous variation in benefits, they are informative of the likely effect of policies that redistribute cash to rural families with children in Brazil. While it is up for debate whether the estimated impact of cash benefits suggest they would be a cost-effective instrument to increase school enrollment or reduce child labor for girls (monthly benefits of R$100 boosted girls' enrollment rates by almost 10 percentage points), it is remarkable that no effect was found for boys' outcomes. Given the relatively large magnitude of the benefits, the absence of a reduction in boys' child labor related to this social security reform challenges the validity of the "luxury axiom" for understanding child labor in rural Brazil. Given the limited or no impact on boys' outcomes, policymakers interested in boosting enrollment rates or reducing child work would be advised to adopt measures that change the relative payoffs and costs of those activities (such as conditioning cash benefits to school attendance) as complements to cash transfer programs.[16]

Last but not the least, one additional remark about the interpretation of these estimates: the variation in income used in this paper is not correlated with economy-wide income variation. Therefore, the effects of income we estimate do not take into account any change in attitudes or social norms towards schooling and child labor due to rising income levels across the board. As a consequence, the effects we find are likely to be underestimates of the changes in child labor and school enrollment that occur as the overall incomes of household in LDCs rise.

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Table 1

THE WORK – SCHOOL ENROLLMENT STATISTICS OF CHILDREN 10-14

	Year 1989	1990	1993	1995	CHANGE 95-89
All children 10-14					
Not Working, Not in school	12.4	12.1	9.4	8.4	-4.0
Only Working	4.9	4.2	2.4	1.9	-3.0
Working and Going to School	5.1	4.8	4.4	4.2	-0.9
Only School	77.6	78.9	83.8	85.4	7.8
Boys 10-14					
Not Working, Not in school	12.4	11.8	9.8	8.8	-3.6
Only Working	6.0	5.6	2.9	2.5	-3.5
Working and Going to School	6.5	6.1	5.6	5.3	-1.2
Only School	75.1	76.6	81.7	83.4	8.3
Girls 10-14					
Not Working, Not in school	12.4	12.5	8.9	8.0	-4.4
Only Working	3.9	2.8	2.0	1.4	-2.5
Working and Going to School	3.7	3.5	3.2	3.1	-0.6
Only School	80.1	81.2	85.9	87.5	7.4

Notes: Source of data is the PNAD household survey (except the Northern region). Working is based on the variable work for pay.

Table 2. Means and Standard Deviations, PNAD Sub-Sample of Households With Children 10 to 14 Years Old

TABLE OF MEANS: BOYS

	ALL	MATURE	RURAL	BEFORE	AFTER
Total benefits in household (In R$ of Jan. 2002)	0.422 [2.0964]	1.114 [3.0929]	0.248 [0.896]	0.39 [2.2589]	0.453 [1.9281]
Female benefits in household	0.113 [0.8881]	0.292 [1.0688]	0.097 [0.5253]	0.091 [0.8545]	0.134 [0.9186]
Male benefits in household	0.309 [1.8725]	0.822 [2.9009]	0.15 [0.676]	0.3 [2.0834]	0.319 [1.6461]
Total benefits (topcoded at 99 pctile)	0.37 [1.3683]	0.999 [2.0418]	0.243 [0.8029]	0.328 [1.3179]	0.41 [1.4136]
Ratio of total benefits to total income	0.059 [0.1845]	0.163 [0.2788]	0.063 [0.1915]	0.052 [0.1694]	0.065 [0.1978]
Total household income	10.481 [19.2894]	9.445 [20.6688]	5.433 [8.6672]	11.009 [18.3521]	9.978 [20.131]
# Benefit receivers in household	0.151 [0.4048]	0.433 [0.6021]	0.157 [0.4213]	0.151 [0.3976]	0.152 [0.4116]
# Female beneficiaries in household	0.058 [0.2406]	0.171 [0.391]	0.065 [0.2525]	0.057 [0.2394]	0.059 [0.2418]
# Male beneficiaries in household	0.093 [0.2951]	0.262 [0.4491]	0.092 [0.2928]	0.094 [0.2952]	0.093 [0.2951]
Enrolled in school	0.85 [0.3573]	0.809 [0.3934]	0.736 [0.441]	0.82 [0.384]	0.878 [0.3272]
Worked in reference week for pay	0.102 [0.3023]	0.112 [0.3158]	0.106 [0.3084]	0.122 [0.3271]	0.083 [0.2753]
Total hours per week	7.639 [15.1483]	8.871 [16.037]	15.083 [18.1994]	8.516 [16.2375]	6.801 [13.9789]
Skin color: black	0.051 [0.2198]	0.06 [0.2369]	0.047 [0.2113]	0.052 [0.2224]	0.05 [0.2173]
Skin color: brown	0.446 [0.4971]	0.495 [0.5]	0.548 [0.4977]	0.443 [0.4967]	0.448 [0.4973]
Number of adults in household	2.33 [0.9448]	2.935 [1.2396]	2.356 [0.8841]	2.36 [0.9597]	2.302 [0.9294]
Female-headed household	0.145 [0.3522]	0.171 [0.3769]	0.082 [0.2745]	0.136 [0.3431]	0.154 [0.3605]
Two parents present	0.839 [0.3678]	0.797 [0.4023]	0.897 [0.3039]	0.847 [0.3598]	0.831 [0.3751]
# Children in family	3.621 [2.0336]	3.496 [2.2981]	4.422 [2.3567]	3.864 [2.1373]	3.39 [1.9007]
Number of observations	66,177	19,428	17,297	31,461	34,716

GIRLS

	ALL	MATURE	RURAL	BEFORE	AFTER
Total benefits in household (In R$ of Jan. 2002)	0.419 [2.0706]	1.127 [3.0184]	0.259 [1.1519]	0.395 [2.2841]	0.441 [1.8385]
Female benefits in household	0.11 [0.7819]	0.287 [1.0196]	0.102 [0.5155]	0.09 [0.758]	0.129 [0.8041]
Male benefits in household	0.309 [1.8825]	0.84 [2.8163]	0.157 [0.9986]	0.305 [2.1254]	0.312 [1.6108]
Total benefits (topcoded at 99 pctile)	0.37 [1.3752]	1.012 [2.0826]	0.247 [0.8302]	0.332 [1.3365]	0.406 [1.4109]
Ratio of total benefits to total income	0.057 [0.1811]	0.16 [0.2739]	0.063 [0.1895]	0.051 [0.1682]	0.063 [0.1928]
Total household income	10.629 [19.476]	9.545 [17.0018]	5.371 [7.0648]	11.17 [22.028]	10.103 [16.5984]
# Benefit receivers in household	0.15 [0.4059]	0.433 [0.6065]	0.158 [0.4196]	0.152 [0.4022]	0.149 [0.4095]
# Female beneficiaries in household	0.058 [0.2412]	0.172 [0.3924]	0.068 [0.258]	0.058 [0.2395]	0.058 [0.243]
# Male beneficiaries in household	0.092 [0.2952]	0.261 [0.4523]	0.09 [0.2893]	0.094 [0.2973]	0.09 [0.2931]
Enrolled in school	0.874 [0.3315]	0.845 [0.3621]	0.766 [0.4231]	0.847 [0.36]	0.901 [0.2987]
Worked in reference week for pay	0.054 [0.2263]	0.063 [0.2422]	0.057 [0.2319]	0.063 [0.2436]	0.045 [0.2077]
Total hours per week	3.482 [11.1544]	4.074 [12]	6.305 [13.7248]	3.726 [11.8349]	3.244 [10.4431]
Skin color: black	0.049 [0.2167]	0.058 [0.2341]	0.045 [0.2079]	0.051 [0.2203]	0.048 [0.213]
Skin color: brown	0.437 [0.496]	0.481 [0.4996]	0.535 [0.4988]	0.432 [0.4954]	0.441 [0.4965]
Number of adults in household	2.339 [0.9563]	2.961 [1.2503]	2.355 [0.887]	2.376 [0.9774]	2.303 [0.9339]
Female-headed household	0.149 [0.3558]	0.176 [0.3811]	0.09 [0.2868]	0.139 [0.3464]	0.158 [0.3645]
Two parents present	0.838 [0.368]	0.796 [0.4031]	0.892 [0.3099]	0.846 [0.3608]	0.831 [0.3747]
# Children in family	3.611 [2.0325]	3.487 [2.2905]	4.452 [2.3612]	3.852 [2.143]	3.377 [1.8894]
Number of observations	65,059	19,013	16,072	31,376	33,683

Notes: AFTER stands for observations for 1993 and 1995. BEFORE stands for 1989 and 1990. MATURE stands for households with at least one person older than 50. RURAL stands for rural location. Monetary values are measured in Reais of January 2002 using the deflator introduced by Corseuil and Foguel (2002).

Table 3. Triple Differences on Benefit Levels and Benefit Receivers

	Panel 1. Male Benefits					
	Total male benefits			Number of males receiving *aposentadoria*		
	Before: 1989-90	After: 1993-95	Time diff.	Before: 1989-90	After: 1993-95	Time diff.
			A. *Treatment Households: Rural households*			
Oldest male is 60-64 y.o.	0.246	0.991	0.745	0.152	0.494	0.341
	[0.041]	[0.06]	[0.073]	[0.016]	[0.023]	[0.028]
Oldest male is 55-59 y.o.	0.174	0.246	0.073	0.106	0.097	-0.009
	[0.024]	[0.037]	[0.044]	[0.011]	[0.011]	[0.016]
Age diff. at a point in time:	0.072	0.744		0.047	0.397	
	[0.048]	[0.071]		[0.02]	[0.026]	
Difference-in-difference:	0.672			0.351		
	[0.086]			[0.032]		
			B: *Control Households: Urban households*			
Oldest male is 60-64 y.o.	1.472	1.724	0.252	0.409	0.513	0.103
	[0.086]	[0.082]	[0.119]	[0.016]	[0.016]	[0.023]
Oldest male is 55-59 y.o.	1.192	1.152	-0.04	0.318	0.276	-0.042
	[0.064]	[0.062]	[0.089]	[0.012]	[0.011]	[0.017]
Age diff. at a point in time:	0.281	0.572		0.092	0.237	
	[0.107]	[0.103]		[0.02]	[0.02]	
Difference-in-difference:	0.292			0.145		
	[0.149]			[0.028]		
DDD	**0.381**			**0.205**		
	[0.172]			**[0.043]**		

	Panel 2. Female Benefits					
	Total female benefits			Number of females receiving *aposentadoria*		
			A. *Treatment Households: Rural households*			
Oldest female is 55-59 y.o.	0.114	0.521	0.408	0.114	0.313	0.199
	[0.016]	[0.035]	[0.038]	[0.015]	[0.021]	[0.025]
Oldest female is 50-54 y.o.	0.06	0.082	0.022	0.058	0.045	-0.012
	[0.008]	[0.013]	[0.015]	[0.008]	[0.007]	[0.01]
Age diff. at a point in time:	0.054	0.44		0.056	0.267	
	[0.018]	[0.037]		[0.016]	[0.022]	
Difference-in-difference:	0.386			0.211		
	[0.041]			[0.027]		
			B: *Control Households: Urban households*			
Oldest female is 55-59 y.o.	0.182	0.274	0.092	0.137	0.163	0.026
	[0.015]	[0.018]	[0.023]	[0.01]	[0.01]	[0.015]
Oldest female is 50-54 y.o.	0.125	0.092	-0.033	0.089	0.052	-0.037
	[0.01]	[0.009]	[0.013]	[0.007]	[0.005]	[0.008]
Age diff. at a point in time:	0.056	0.182		0.047	0.11	
	[0.018]	[0.02]		[0.012]	[0.011]	
Difference-in-difference:	0.126			0.063		
	[0.026]			[0.017]		
DDD	**0.26**			**0.148**		
	[0.049]			**[0.032]**		

Notes: Average male and female benefits are expressed as multiples of R$100 of 2002. Standard errors are given in square brackets. Difference-in-difference-in-difference (DDD) is the difference-in-difference from the upper panel minus that in the lower panel.

Table 4. Triple Differences on School Enrollment and Market Work

Panel 1. Male Benefits

	School enrollment			Market work		
	Before: 1989-90	After: 1993-95	Time diff.	Before: 1989-90	After: 1993-95	Time diff.
	A. Treatment Households: Rural households					
Oldest male is 60-64 y.o.	0.668	0.749	0.08	0.089	0.065	-0.025
	[0.018]	[0.017]	[0.025]	[0.011]	[0.009]	[0.014]
Oldest male is 55-59 y.o.	0.666	0.757	0.091	0.083	0.072	-0.011
	[0.013]	[0.014]	[0.019]	[0.008]	[0.008]	[0.011]
Age diff. at a point in time:	0.003	-0.008		0.006	-0.008	
	[0.022]	[0.021]		[0.013]	[0.013]	
Difference-in-difference:	-0.011			-0.014		
	[0.031]			[0.018]		
	B: Control Households: Urban households					
Oldest male is 60-64 y.o.	0.865	0.882	0.017	0.119	0.075	-0.044
	[0.009]	[0.009]	[0.013]	[0.009]	[0.007]	[0.011]
Oldest male is 55-59 y.o.	0.864	0.904	0.04	0.095	0.071	-0.024
	[0.007]	[0.006]	[0.01]	[0.006]	[0.006]	[0.009]
Age diff. at a point in time:	0.001	-0.022		0.024	0.004	
	[0.012]	[0.011]		[0.011]	[0.009]	
Difference-in-difference:	-0.023			-0.02		
	[0.016]			[0.014]		
DDD	**0.012**			**0.006**		
	[0.035]			**[0.023]**		

Panel 2. Female Benefits

	School enrollment			Market work		
	A. Treatment Households: Rural households					
Oldest female is 55-59 y.o.	0.673	0.752	0.078	0.111	0.081	-0.03
	[0.019]	[0.017]	[0.025]	[0.013]	[0.011]	[0.017]
Oldest female is 50-54 y.o.	0.686	0.762	0.075	0.086	0.069	-0.017
	[0.012]	[0.012]	[0.017]	[0.007]	[0.007]	[0.01]
Age diff. at a point in time:	-0.013	-0.01		0.025	0.012	
	[0.022]	[0.021]		[0.015]	[0.013]	
Difference-in-difference:	0.003			-0.013		
	[0.031]			[0.019]		
	B: Control Households: Urban households					
Oldest female is 55-59 y.o.	0.854	0.866	0.012	0.102	0.082	-0.02
	[0.009]	[0.008]	[0.012]	[0.008]	[0.007]	[0.01]
Oldest female is 50-54 y.o.	0.844	0.89	0.047	0.112	0.085	-0.027
	[0.007]	[0.006]	[0.009]	[0.006]	[0.005]	[0.008]
Age diff. at a point in time:	0.01	-0.024		-0.01	-0.003	
	[0.012]	[0.01]		[0.01]	[0.009]	
Difference-in-difference:	-0.034			0.007		
	[0.015]			[0.013]		
DDD	**0.037**			**-0.02**		
	[0.034]			**[0.023]**		

Notes: Standard errors are given in square brackets. Difference-in-difference-in-difference (DDD) is the difference-in-difference from the upper panel minus that in the lower panel.

Table 5. Effect of the reform on benefit take-up and levels

FIRST STAGE REGRESSIONS		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Number of benefit receivers	adding fake controls	of which: Males	of which: Females	Total benefits (winsorized)	without winsorization	adding fake controls	of which: male benefits	of which: female bens.
Oldest Male Age 60-64 X Rural X After	β^m_{new}	0.296	0.303	0.231	0.0652	0.557	0.54	0.557	0.473	0.0854
		[0.045]	[0.046]	[0.035]	[0.025]	[0.147]	[0.262]	[0.149]	[0.13]	[0.037]
Oldest Male Age 65+ X Rural X After	β^m_{raise}	0.0629	0.0682	0.00818	0.0547	0.467	0.415	0.474	0.446	0.0264
		[0.035]	[0.035]	[0.025]	[0.023]	[0.12]	[0.192]	[0.12]	[0.106]	[0.034]
Oldest Female Age 55-59 X Rural X After	β^f_{new}	0.199	0.188	0.0502	0.149	0.375	0.427	0.358	0.0518	0.285
		[0.039]	[0.04]	[0.025]	[0.029]	[0.113]	[0.163]	[0.116]	[0.091]	[0.044]
Oldest Female Age 60+ X Rural X After	β^f_{raise}	0.13	0.128	0.00498	0.125	0.601	0.679	0.609	0.103	0.451
		[0.033]	[0.033]	[0.016]	[0.028]	[0.083]	[0.132]	[0.083]	[0.057]	[0.04]
Joint significance of triple interactions										
F(4,90802)		26.09	25.87	13.55	18.98	29.11	12.72	29.57	9.89	49.17
F-test joint sign. of instruments (p-value)		0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Oldest Male Age 55-59 X Rural X After	ϕ^m_{fake}		0.0515					0.149		
			[0.029]					[0.117]		
Oldest Female Age 50-54 X Rural X After	ϕ^f_{fake}		-0.0032					0.0903		
			[0.024]					[0.085]		
F-test fake instruments (p-value)			0.1900					0.1935		
Number of observations		90802	90802	90802	90802	90802	90802	90802	90802	90802
Number of groups		9512	9512	9512	9512	9512	9512	9512	9512	9512
R-squared		0.82	0.82	0.832	0.715	0.629	0.452	0.629	0.615	0.692

Notes: The PNAD data sets for 1989, 1990, 1992 and 1995 were used for the regressions above, excluding the observations for the states of the Northern region. Each observation represents one household with at least one child of ages 10 to 14, and with no member with 12 years or more of education. Robust standard errors are in square brackets. The shaded cells indicate statistical significance at the 5 percent level.

The specification is the same as equation (1) in the text. This table omits the coefficients on dummies for rural location, after the reform, age of the oldest male and female (first-level effects) and interactions between after the reform and rural location; after the reform and ages of the oldest male and female; and rural location and ages of the oldest male and female (second-level effects).

Table 6. Children outcomes and benefits eligibility.

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
REDUCED FORM REGRESSIONS											
		Enrolled in school		Worked for pay		Hours of work per week		Skin color: black	Youngest child in family	# Children in family	# Adults in family
		Boys	Girls	Boys	Girls	Boys	Girls	All	All	All	All
Oldest Male Age 60-64 X Rural X After	β^{m}_{new}	-0.0164	0.0187	0.0457	0.0471	3.34	1.57	-0.0027	0.0482	0.2040	-0.0087
		[0.048]	[0.054]	[0.036]	[0.025]	[1.85]	[1.63]	[0.02]	[0.038]	[0.199]	[0.11]
Oldest Male Age 65+ X Rural X After	β^{m}_{raise}	0.00404	0.0605	0.00588	0.00062	-0.36	0.997	0.0147	-0.0380	-0.1890	0.0142
		[0.044]	[0.034]	[0.029]	[0.024]	[1.65]	[1.23]	[0.016]	[0.03]	[0.213]	[0.11]
Oldest Female Age 55-59 X Rural X After	β^{f}_{new}	-0.0104	0.0281	0.00541	-0.0497	-1.95	-0.18	-0.0116	0.0067	0.0544	-0.1170
		[0.051]	[0.045]	[0.028]	[0.027]	[2.03]	[1.66]	[0.021]	[0.047]	[0.223]	[0.142]
Oldest Female Age 60+ X Rural X After	β^{f}_{raise}	0.00754	0.0574	-0.0212	-0.0483	0.0826	-2.28	-0.0196	0.0280	-0.0690	0.0157
		[0.037]	[0.038]	[0.024]	[0.023]	[1.53]	[1.26]	[0.015]	[0.029]	[0.237]	[0.092]
F-test joint sign. of instruments (p-value)		0.9933	0.0423	0.6232	0.0296	0.4278	0.3621	0.6771	0.3713	0.6113	0.9406
Oldest Male Age 55-59 X Rural X After	ϕ^{m}_{fake}	-0.0234	0.0339	0.0491	-0.00802	0.811	0.838	-0.0054	-0.0502	0.1160	0.0698
		[0.046]	[0.046]	[0.025]	[0.023]	[1.79]	[1.32]	[0.012]	[0.036]	[0.201]	[0.094]
Oldest Female Age 50-54 X Rural X After	ϕ^{f}_{fake}	-0.0636	0.00828	0.0106	0.00564	1.54	0.6	-0.0117	-0.0004	0.2470	-0.0871
		[0.04]	[0.038]	[0.027]	[0.022]	[1.42]	[1.09]	[0.013]	[0.029]	[0.169]	[0.082]
F-test fake instruments (p-value)		0.2568	0.749	0.0898	0.9259	0.4641	0.5788	0.4829	0.3092	0.2149	0.5279
Number of observations		66260	65146	66260	65146	66260	65146	131406	131406	131406	131406
Number of groups		399	398	399	398	399	398	797	797	797	797
R-squared		0.823	0.824	0.535	0.364	0.902	0.726	0.27	0.884	0.865	0.953

Notes: The PNAD data sets for 1989, 1990, 1992 and 1995 were used for the regressions above, excluding the observations for the states of the Northern region and the households with at least one member with 12 years or more of education. Standard errors are in square brackets. The shaded cells indicate statistical significance at the 5 percent level.

The specification is the same as equation (1) in the text. This table omits the coefficients on dummies for rural location, after the reform, age of the oldest male and female (first-level effects) and interactions between after the reform and rural location; after the reform and ages of the oldest male and female; and rural location and ages of the oldest male and female (second-level effects). The regressions are calculated on group means (as suggested by Angrist and Pischke 2008, pp. 313-314), where groups are defined by age of the oldest male and female (in 5 year brackets), after and rural location.

Table 7. OLS Estimates of the Effect of Old-Age Benefits on Children Outcomes

	OLS ESTIMATES							
	Benefit Levels (In R$100 of Jan. 2002)				Number of benefit receivers			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	BOYS		GIRLS		BOYS		GIRLS	
ENROLLED IN SCHOOL								
Total benefits	0.026		0.020		0.042		0.035	
	[0.012]		[0.01]		[0.043]		[0.037]	
Female benefits		0.003		0.049		-0.003		0.025
		[0.035]		[0.033]		[0.057]		[0.052]
Male benefits		0.033		0.016		0.089		0.048
		[0.015]		[0.011]		[0.065]		[0.052]
F-test: female = male (P-value)		0.429		0.346		0.279		0.754
WORKED FOR PAY								
Total benefits	-0.003		-0.004		0.031		0.007	
	[0.011]		[0.007]		[0.029]		[0.026]	
Female benefits		0.010		-0.009		0.050		0.010
		[0.024]		[0.023]		[0.038]		[0.038]
Male benefits		-0.005		-0.002		0.011		0.003
		[0.014]		[0.008]		[0.044]		[0.036]
F-test: female = male (P-value)		0.599		0.783		0.509		0.892
TOTAL HOURS PER WEEK, ALL JOBS								
Total benefits	-0.296		-0.211		0.959		0.895	
	[0.497]		[0.325]		[1.61]		[1.37]	
Female benefits		1.460		-0.878		3.700		0.250
		[1.43]		[1.23]		[2.37]		[2.01]
Male benefits		-0.647		-0.003		-1.950		1.800
		[0.563]		[0.378]		[2.14]		[1.88]
F-test: female = male (P-value)		0.153		0.500		0.069		0.586

Notes: The PNAD data sets for 1989, 1990, 1993 and 1995 were used for the regressions above, excluding the observations for the states of the Northern region. There are 66,260 observations for the boys and 65,146 for the girls. The regressions are calculated on group means (as suggested by Angrist and Pischke (2008), pp. 313-314). Standard errors are in square brackets. The shaded cells denote statistical significance at the 5 percent level. This table omits the coefficients on dummies for rural location, after the reform, household composition (first-level effects) and interactions between after the reform and rural location; after the reform and household composition; and rural location and household composition (second-level effects).

Table 8. IV Estimates of the Effect of Old-Age Benefits on Children Outcomes

STRUCTURAL INSTRUMENTAL VARIABLES ESTIMATES								
	Benefit Levels (In R$100 of Jan. 2002)				Number of benefit receivers			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	BOYS		GIRLS		BOYS		GIRLS	
ENROLLED IN SCHOOL								
Total benefits	0.002		0.097		-0.037		0.236	
	[0.032]		[0.036]		[0.094]		[0.111]	
Female benefits		0.038		0.098		0.049		0.531
		[0.086]		[0.085]		[0.193]		[0.283]
Male benefits		-0.032		0.101		-0.103		-0.159
		[0.08]		[0.074]		[0.149]		[0.344]
F-test: female = male (P-value)		0.642		0.986		0.589		0.227
WORKED FOR PAY								
Total benefits	0.001		-0.036		0.067		-0.082	
	[0.02]		[0.022]		[0.066]		[0.068]	
Female benefits		-0.056		-0.130		-0.054		-0.452
		[0.062]		[0.055]		[0.119]		[0.225]
Male benefits		0.054		0.047		0.159		0.412
		[0.059]		[0.046]		[0.117]		[0.221]
F-test: female = male (P-value)		0.327		.0.0459		0.278		0.036
TOTAL HOURS PER WEEK, ALL JOBS								
Total benefits	0.174		-0.849		4.300		-0.780	
	[1.2]		[1.25]		[3.48]		[3.97]	
Female benefits		-2.750		-5.580		-6.520		-15.100
		[3.9]		[2.8]		[8.46]		[10.2]
Male benefits		3.100		3.380		12.500		18.300
		[3.36]		[2.51]		[6.43]		[11.1]
F-test: female = male (P-value)		0.386		.0.0457		0.136		.0.0805

Notes: The PNAD data sets for 1989, 1990, 1993 and 1995 were used for the regressions above, excluding the observations for the states of the Northern region. There are 66,260 observations for the boys and 65,146 for the girls. The regressions are calculated on group means (as suggested by Angrist and Pischke (2008), pp. 313-314). Standard errors are in square brackets. The shaded cells denote statistical significance at the 5 percent level. This table omits the coefficients on dummies for rural location, after the reform, household composition (first-level effects) and interactions between after the reform and rural location; after the reform and household composition; and rural location and household composition (second-level effects).

Table 9. Robustness check: Effect of total benefits in household for different samples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
INSTRUMENTAL VARIABLES ESTIMATES OF THE EFFECT OF BENEFIT LEVELS ON CHILDREN OUTCOMES												
			BOYS						**GIRLS**			
Sample	All	Mature	NE	Rural	13-14	Head < 4 yrs	All	Mature	NE	Rural	13-14	Head < 4 yrs
ENROLLED IN SCHOOL												
	0.002	0.055	-0.017	-0.031	0.011	0.005	0.097	0.152	0.135	-0.013	0.169	0.115
	[0.032]	[0.047]	[0.052]	[0.059]	[0.04]	[0.04]	[0.036]	[0.052]	[0.061]	[0.037]	[0.08]	[0.046]
WORKED FOR PAY												
	0.001	-0.029	-0.018	0.005	-0.014	0.002	-0.036	-0.038	-0.080	0.043	-0.061	-0.054
	[0.02]	[0.029]	[0.036]	[0.033]	[0.03]	[0.027]	[0.022]	[0.028]	[0.037]	[0.019]	[0.047]	[0.031]
TOTAL HOURS PER WEEK, ALL JOBS												
	0.17	-1.17	0.96	5.50	-0.08	0.13	-0.85	-1.37	-3.81	4.01	-2.04	-1.84
	[1.2]	[1.75]	[1.94]	[2.59]	[1.62]	[1.54]	[1.25]	[1.6]	[1.84]	[1.4]	[2.46]	[1.55]
Observations	66,260	19,436	25,710	17,342	26,363	45,413	65,146	19,020	25,130	16,112	25,987	44,140
Number of groups	399	299	395	199	396	398	398	298	394	198	395	398

Notes: This table presents estimates of the same equation as in Table 8, but for different samples. Standard errors are in square brackets and they are clustered by household composition type The sample denoted ALL includes all children age 10-14; the MATURE subsample is obtained by dropping all households without a resident older than 50 (92181 observations) and whose residents older than 50 have 12 years of schooling or more (2016 observations); NE denotes Northeastern region; 13-14 includes only the 13-14 year olds from the ALL sample; HEAD < 4 includes only the households whose head has less than 4 years of schooling from the ALL sample.

Table 10. Has the reform changed the number of children living with each elderly?

Panel I: Men over 50

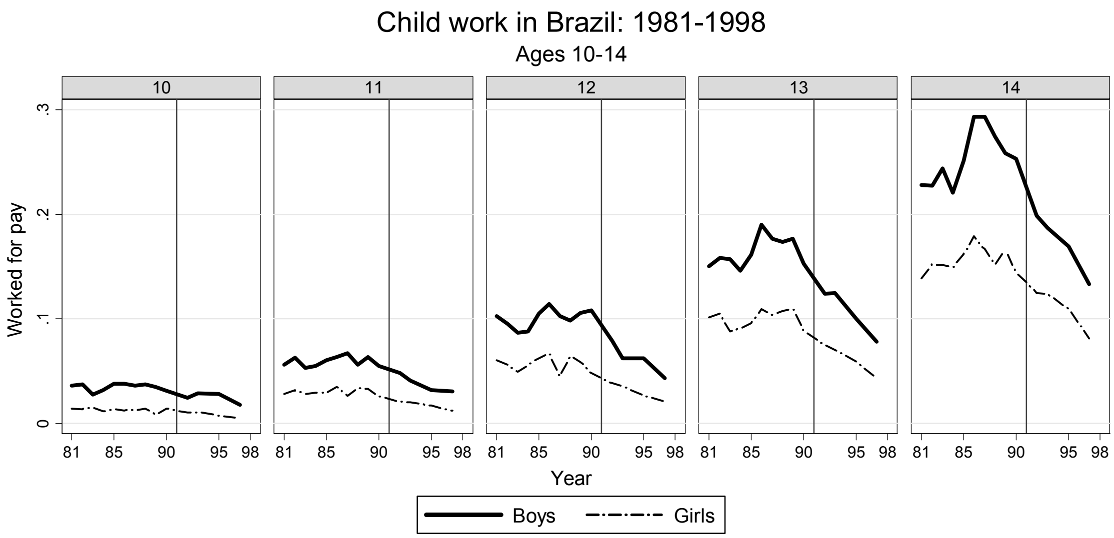
		5-9		10-14		15-19	
Dependent variable		**# BOYS**	**# GIRLS**	**# BOYS**	**# GIRLS**	**# BOYS**	**# GIRLS**
Oldest Male Age 60-64 X Rural X After	χ^{m}_{new}	0.0148	0.00709	0.0237	0.0387	-0.0144	0.018
		[0.023]	[0.02]	[0.02]	[0.027]	[0.025]	[0.025]
Oldest Male Age 65+ X Rural X After	χ^{m}_{raise}	0.00568	-0.00414	0.0112	0.0136	0.00891	0.00745
		[0.017]	[0.015]	[0.015]	[0.02]	[0.019]	[0.019]
F-Stat (p-value):		0.8159	0.828	0.5019	0.3792	0.6078	0.7745
R-squared		0.877	0.897	0.963	0.921	0.964	0.939
Groups		124	124	124	124	124	124
Observations		77,525	77,525	77,525	77,525	77,525	77,525

Panel II: Women over 50

		5-9		10-14		15-19	
		# BOYS	**# GIRLS**	**# BOYS**	**# GIRLS**	**# BOYS**	**# GIRLS**
Oldest Female Age 55-59 X Rural X After	χ^{f}_{new}	0.0271	-0.00603	0.00532	0.00127	-0.0129	-0.0367
		[0.028]	[0.024]	[0.027]	[0.027]	[0.03]	[0.026]
Oldest Female Age 60+ X Rural X After	χ^{f}_{raise}	0.00388	0.00881	-0.0203	-0.00179	-0.00428	-0.0503
		[0.022]	[0.019]	[0.021]	[0.021]	[0.024]	[0.02]
F-Stat (p-value):		0.5438	0.7046	0.3872	0.9876	0.9072	0.062
R-squared		0.1346	0.1265	0.1359	0.1392	0.1418	0.1387
Groups		124	124	124	124	124	124
Observations		90,145	90,145	90,145	90,145	90,145	90,145

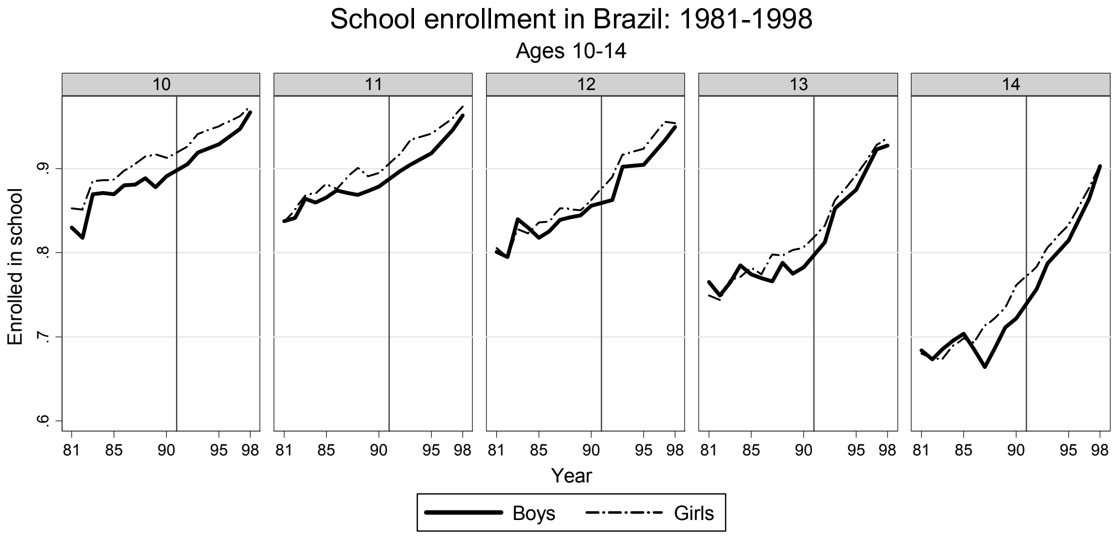
Notes: The PNAD data sets for 1989, 1990, 1993 and 1995 were used for the regressions above. After denotes the years after the reform. The sample consists of all people 50 or older, living in regions other than the Northern states, where rural households were not surveyed. The F-stat tests the null that the coefficients on the triple interactions are equal to zero. Shaded cells denote statistical significance at the 5 percent level.

Figure 1



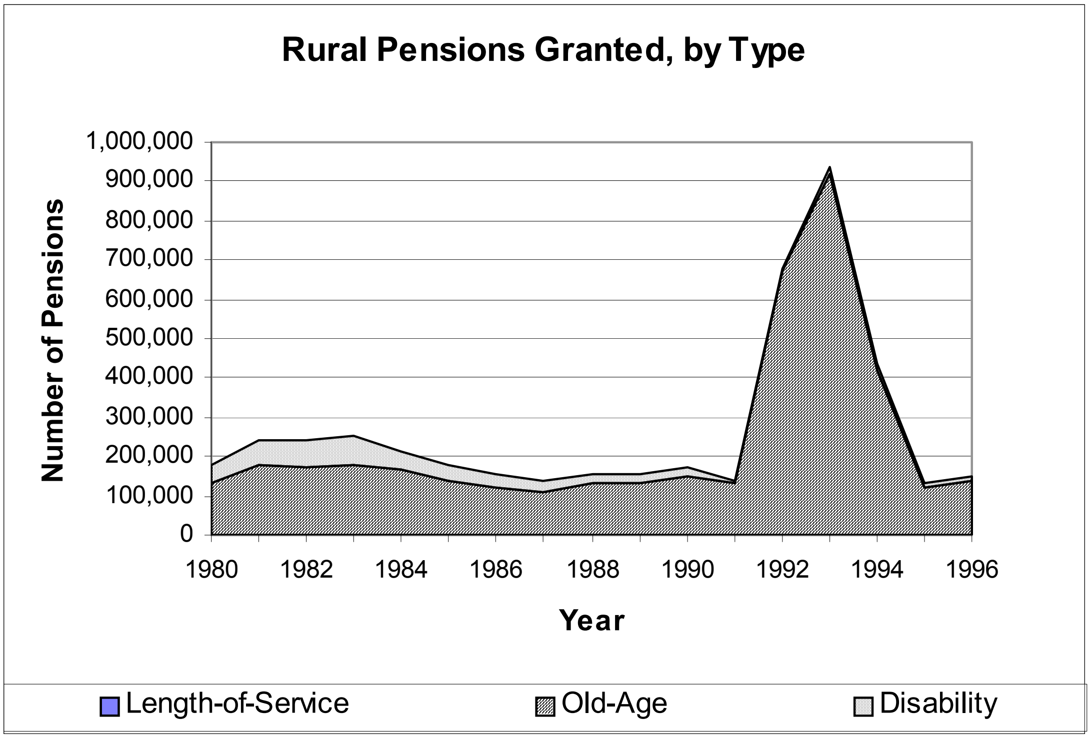
The figures above show the time series behavior of the proportion of boys and girls who "worked for pay". The vertical line on 91 divides the period in before and after the reform. Source: PNAD.

Figure 2



The figures above show the time series behavior of the proportion of boys and girls who are "enrolled in school". The vertical line on 91 divides the period in before and after the reform. Source: PNAD.

Figure 3



The figure above presents the flow of rural pensions granted for each year between 1980 and 1996. Notice that the spike starts in 1992 and lasts until 1994. Source: Anuário Estatístico da Previdência (1997), in de Carvalho Filho (2008).

Appendix. Construction of Children's Outcomes Variables

Work for pay.
The variable work for pay is equal to one if the child recorded a positive income from work; 0 otherwise. The primary variable that we used were the variables "Renda mensal todos trabalhos" (V601 for PNAD 1989 and 1990) and "Valor renda mensal todos trabalhos" (V4719 for PNAD 1993 and 1995)

Enrolled in school.
The variable enrolled in school is equal to 1 if the survey response to the question "Frequenta escola?" is not equal to 0 which is the code for "não frequenta" (V312 for PNAD 1989 and 1990); or it is equal to 2 which is the code for "sim" (V0602 for PNAD 1993 and 1995). Notice that this variable could as well be translated as "attended school". The question asks if the child "frequenta escola" without referring to a specific time frame (as the questions on work are phrased), so we interpret as referring to whether the child **eventually** attends school or is enrolled at school.

Total hours of work
For the years 1989 and 1990, we use the variable V5100 ("Horas em todos trabalhos"); for the years 1992 and 1993, we sum the variables V9058 ('Número de horas trabalhadas por semana nesse trabalho"), V9105 ("Número de horas trabalhadas por semana nesse(s) outro(s) trabalho(s) (excluindo-se o principal e o secundário)") and V9101 ("Número de horas trabalhadas por semana nesse emprego secundário"). We capped total hours at 90 per week. We then recode hours to zero if the worker is unpaid and hours are less than 15 per week to ensure consistency over time in our hours variable because in the PNADs of 1989 and 1990 unpaid hours below 15 hours per week were not recorded.

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2. Emerson and Souza (2007) find that adult earnings for the cohorts born in 1933 to 1971 in Brazil are maximized at entry age into the labor market between 12 and 14 years old.

3. Edmonds (2007, 2009) provide useful reviews of the most recent work on child labor.

4. But Kruger (2007) finds that positive shocks to coffee production increase child labor in Brazilian coffee producing counties

5. Before the reform, old-age benefits for rural workers were flat and equal to 50 percent of the minimum wage. After the reform, rural workers could choose between a minimum benefit equal to one minimum wage or calculated benefits based on their past earnings history, but the near totality of rural workers did not have past documented earnings high enough to justify a calculated benefit higher than one minimum wage, so for all practical purposes, the reform increased benefits to one minimum wage. For a more detailed account of the reform, see de Carvalho Filho (2008).

6. It is worth mentioning now that if families can borrow against future anticipated income (permanent income), schooling and child labor decisions would not depend on the timing of pension receipts (Edmonds 2006).

7. Source: ILO (1996), based on statistics organized by Ashagrie (1993), as quoted in Basu (1999).

8. Krueger (1996) argues that compulsory schooling laws are usually not enforced in developing countries.

9. For an evaluation of the PETI program, see World Bank (2001).

10. Some respondents may misclassify their *aposentadoria* and *pensão* benefits. The results are robust to aggregating those two classes of income.

11. In a previous version of this paper we included the variable "Worked in reference week?" which included work without pay in a family farm or small business, as well as some forms of household work. However, there were changes in the survey instrument questions regarding unpaid work after 1990. The changes in the survey instrument might be immaterial for our estimates if captured by the year fixed effects, but if they affect observed responses in a manner that is correlated with the presence of rural workers affected by the reform, our estimates would be biased. Thus we decided to exclude that variable from this version of the paper.

12. The estimates in this paper are not significantly affected by this change.

13. de Carvalho Filho (2008) discusses the impact of the reform on benefit take-up rates among males.

14. It is crucial that there be no changes in living arrangements for this empirical strategy to provide consistent estimates. In Section VI I argue that neither endogeneity of living arrangements nor selection problems seem to be major problems.

15. de Carvalho Filho (2000b) finds that elderly unmarried, divorced or widowed women are more likely to live alone in response to a social security reform that increased their benefits. Ardington, Case and Hosegood (2007) find in a panel data set that old-age pensions increase the labor supply of prime age male family members, in contrast to the results by Bertrand, Miller and Mullainathan (1999) in a single cross-section, and attribute it to benefits helping finance the migration of prime-aged males in search for employment.

16. Glewwe and Kassouf (2008) found that the Bolsa Escola program – the Brazilian cash transfer program conditional on school enrollment later to be rebranded as Bolsa Família – increased school enrollment for children in grades 1 through 4. Another example of a program that conditioned the receipt of cash benefits to school attendance is the Progresa program in Mexico (Gomez de Léon, Parker and Hernandez 1999; Schulz 2004). Margo and Finegan (1996) presents evidence that compulsory schooling laws were effective in increasing school attendance in the United States in the beginning of the twentieth century when combined with child labor restrictions.