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The marginal propensity to earn, consume and save out of unearned income in South Africa

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THE MARGINAL PROPENSITY TO EARN, CONSUME AND
SAVE OUT OF UNEARNED INCOME IN SOUTH AFRICA

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The marginal propensity to earn, consume and save out of unearned income in South Africa*

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March 24, 2010

Abstract

We use a rapid introduction of an unconditional cash grant (child support) in South Africa to estimate the marginal propensity to consume and earn out of a permanent change in unearned income. We find that the marginal propensity to earn is about to -0.25 for single-adult households, and somewhat lower for households with more than one adult. A very small fraction of the grant is saved. All in all, the marginal propensities estimated here are all similar to those reported in comparable papers using US data. However, they stand in contrast to some results on conditional cash transfers in other developing countries.

JEL: J22, O23, H31

Keywords: Social policy, Poverty reduction, Labor Supply, Earnings, Savings, Expenditure, Stone-Geary utility

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1 Introduction

On an aggregated level, labor force participation is quite insensitive to permanent changes in the wage rate. That is, despite the fact that productivity has increased dramatically during the last century, labor force participation has only declined slightly.¹ Moreover, even though there are large differences in productivity across countries, there are only modest differences in labor supply. A popular economic explanation for these patterns is that increased productivity has two opposing effects. When wages increase, one additional hour of work becomes more rewarding relative non-work (leisure). However, as people become richer, they can afford to work less. In other words, the substitution effect and the income effect cancel out.

This paper is concerned with the income effect. More specifically, this paper presents estimates of the marginal propensity to earn out of a permanent shift in unearned income (commonly abbreviated MPE in the literature). To identify this effect, an unusually rapid introduction of an unusually generous cash grant is exploited – the South African child support grant. In the course of just a few years, the grant successfully increased life-time wealth for millions of households in South Africa by 10 to 30 percent. The basic idea is to compare the impact of an additional child on earnings and expenditure before and after an additional child was rewarded by a cash grant – a difference-in-difference approach.

A special feature of the South African child support grant is that it is unconditional; households are not required to do anything special to keep it. This is very useful for our purposes. Large, exogenous income changes are hard to come by. Those that are large enough to affect life-time wealth, such as Mexico’s well-known *Progresas/Oportunidades* program, typically require that recipients do something in return for the cash, like look for a job or put their children in school.² Additionally, research-motivated transfer schemes (e.g. those involving smaller pilot communities) are often transitory in nature, and does not always represent a credible commitment of future payments. Such commitments are arguably important

¹Of course, there are significant trends in hours worked across some socioeconomic dimensions, for example gender. But overall, the lack of a noticeable long-term trend in aggregate labor force participation has been classified as one of the “great ratios of economics” (Klein and Kosobud 1961, discussed in e.g. Pencavel 1986 and cited more recently in Kimball and Shapiro 2003).

²For a review of similar programs in Latin America, see the reviews in Das et al. (2005) and Rawlings et al. (2005). For a review on US welfare programs, see Moffitt (1992).

if households take future wealth into account when deciding about today’s labor supply (see for example Blundell and McCurdy 2002 for a critical discussion of what difference-in-difference estimates of labor supply really mean).

We are not the first to study the impact of the South African child support grant. However, contrary to previous literature, this paper focuses on market-evaluated outcomes – that is, earnings and expenditure, rather than e.g. schooling and nutrition.³ This approach greatly facilitates interpretation. For example, Case et al. (2005) find that six-year-olds increased their school attendance by 8.1 percentage points on average, as a result of child support.⁴ This is a statistically significant effect, and it seems large, but does it imply that cash is an efficient tool for increasing school attendance? The answer depends on how much resources households devoted to schooling prior to the reform; that is, how much school attendance is *worth*. Without knowing this, we do not really know whether a smaller state budget could have been used to achieve the same thing – for example by making the grant conditional on school attendance. Thus, by using market-evaluated outcomes, a more appropriate sense of the efficiency of the program is achieved.

Two papers using US data are particularly close to this one in spirit. Imbens et al. (2001) use a sample of lottery players in the US to estimate the marginal propensity to earn, and find propensities that range from -0.1 to -0.25 . Kimball and Shapiro (2003) use hypothetical lottery winners (i.e. they ask a sample of people what they would do in the event of winning the sweepstakes) and arrive at estimates close to -0.3 . These papers share this paper’s use of a natural (in Shapiro and Kimball’s case, controlled) experiment to estimate the marginal propensities to consume and earn, based on a life-cycle model of labor supply. Providing estimates that are directly comparable to the US literature is motivated by the tendency among economists to downplay the role of market incentives in poor countries. For example, Case and Deaton (1999) argue that the “effect of cash transfers on labour

³Most of these studies, with the exception of Case et al. (2005), are unpublished and summarized in Lund (2008). A notable working paper is Agüero et al. (2007), who study nutritional outcomes. See also the ILO-report by Budlender and Woolard (2000), on correlations between schooling, work and child support transfers, and the useful survey by Samson et al. (2004) on all forms of social grants in South Africa.

⁴More specifically, Case et al. (2005) use longitudinal data, but use cross-sectional variation in grant take-up to identify the effect.

supply is surely insignificant in many developing countries (and especially South Africa), where there are high rates of under- and unemployment”. This seems like a reasonable proposition, but still one that merits empirical investigation.

We find that the marginal propensity to earn is about -0.25 for households with one adult. For other households, it is closer to -0.4 . The marginal propensity to save is close to zero or even negative. These results are similar to the US studies cited above. However, they are somewhat different from those associated with conditional cash transfers in other developing countries (for the labor supply effects of Progresa, see Parker and Skoufias 2000), suggesting that pure cash transfers might have different effects than conditional cash transfers. Moreover, the results also seem difficult to reconcile with (some) earlier evidence on labor supply and cash pensions in South Africa. A study by Ardington et al. (2007) suggests that prime-age adults work *more* when a person in the household retires and receive cash pensions, and less when that person dies and loses the pension. In another study, however, Bertrand et al. (2000) finds a negative impact of pension on prime-age male labor supply in South Africa. However, since the presence of a pensioner in the household arguably affects behavior directly, these results are not readily comparable to ours.

The estimates presented here also pertain to the public finance literature. As pointed out by Kimball and Shapiro (2003), if the income effect on labor supply is large, and we are to maintain that permanent changes in the wage rate only have a small effect on labor supply, the substitution effect must be large as well. The substitution effect is the derivative of the Hicksian supply of labor, and most of the results within the standard Ramsey model of optimal taxation are related to the compensated elasticities (see e.g. Auerbach and Hines 2002). In some special but influential variants of this standard model, the optimal tax system entails taxing more heavily goods that have a low compensated elasticity. Thus, the results potentially point to important differences in optimal tax formulae across different type of family constellations.

The rest of the paper is structured as follows. Section 2 gives a brief introduction to the child support grant in South Africa, and presents some summary statistics and graphical evidence regarding its spread and take-up. Interpretation is discussed using a life-cycle model of labor supply in Section 3. Section 4 presents

the results and Section 5 concludes.

2 Background to the child support grant

2.1 Policy background

This section provides a brief background to the child support grant with respect to its implementation, conditionality and funding. It draws almost exclusively on the work by Lund (2008), Lund et al. (2008) and Case et al. (2005).

The child support grant was introduced in an ambition to reform the social welfare system in South Africa, which had previously been racially biased and disfavored Africans.⁵ A committee was established in 1996 to investigate all forms of child and family support. The committee was chaired by Frances Lund, and is often referred to as the “Lund Committee”. The committee outlined recommendations regarding several aspects of the grant, including its conditionality, funding and reach. Most of these recommendations were met upon implementation.

The child support grant was implemented in April 1998. In 2000, it was available to all eligible children from birth to the seventh birthday; in 2005, the cut-off age had been extended to the age of thirteen. The age extension was accompanied by an increase in the monthly payment, from R 100 to R 180, as well as public efforts to increase awareness of the grant. The income eligibility level was constant at R 13 200 per household and year. According to Case et al. (2005), take-up was modest until 2002. In 1999, 18 000 children received the grant; in 2005, the figure had increased to almost 6 million (Lund 2008).

The child support grant is given to the primary caregiver of the child – not necessarily the biological mother or father – provided that the primary caregiver passes the means test. Once the primary caregiver of the child qualifies for the grant, there are no further requirements on behavior. Thus, the grant is unconditional in the sense that recipients of the grant are not required to undertake any particular activities in order to keep the benefit (e.g. putting their children in

⁵The labels “African”, “Colored”, “Indian” and “White” are still used to categorize ethnicities in South African household surveys. We will adopt this terminology. Africans are in vast majority.

school, visit health services, work more, etc.). There are, however, a handful of technical conditions that could change the behavior of applicants before applying, including the requirement to present a birth certificate, identity documents and a health card with the child's immunization status. But these are once-for-all requirements. Unlike other well-known cash programs in developing countries, the caregiver is not monitored once he or she has been entitled to the benefit (Lund et al. 2008).

The motivation for making the grant conditional on income was to keep social spending within the fiscal constraint. From its inception, the means test appears to have been the most problematic part of the child support grant. The instructions for how to test for income eligibility have been changed and are now somewhat different from the initial instructions. In 2008, the instructions for verifying income eligibility was "if you are working, show proof of your recent income (e.g. pay slip) or make a sworn statement", which, if anything, means that earnings rather than full income is tested. It is quite possible that households are able to underreport or reduce earnings temporarily in order to gain eligibility status. However, statistical correlations using proxies for earnings, such as ethnicity and location, suggest that the grant is well-targeted towards the poor (Budlender and Woodlard 2006).

The income eligibility threshold was set to R 13 200 per year for the entire household during the period of interest (it was never corrected for inflation during this period). The size of the grant was more than R 2 000 per year in 2005, which is *at least* 15 percent of eligible household's earnings. One might wonder how such large cash transfers are at all feasible in a developing country like South Africa. Officially, the funding of the child support grant was made possible by phasing out the state maintenance grant. The state maintenance grant was larger in size but reached far less households and clearly disfavored native Africans. However, abolishing the state maintenance grant effectively meant releasing R 1.3 billion in public funds per year (Lund et al. 2008), which is not even close to cover the costs of the child support grant. The increase in state funds in South Africa since independence appears to have been driven by growth in public expenditure and by attracting foreign aid (although no foreign aid has explicitly targeted the child support grant). No increase in the tax level was admitted to fund the grant (Lund 2008).

The Lund Committee considered using non-cash policies to reach the poor, such as extending nutritional campaigns towards schools and involving community based NGOs in health and education services. These policies were deemed too costly and inefficient, however, given the poor capacity of South Africa's social authorities. The use of cash grants appears to have been decided upon simply because there were no other options available; economic issues on incentives did not play a major role in the committee's decision. Lund (2008) makes the following remarks regarding economic incentives and social benefits:

In the field of policy reform, and especially regarding cash transfers, questions regarding incentives are a big issue. What is the relationship between forms of social support and human behavior? Does the provision of cash or in-kind assets have an effect on choices about who one lives with, or about having children, or about work seeking, or about how one will finance one's retirement? The literature on incentives and social security from industrialized countries, and especially literature in economics, concentrates more on negative incentives and the "perverse" effects of social benefits on such things as household structures, fertility rates, labour-seeking behavior and productivity. Much of this literature is strongly economic, moralistic, value-laden and fairly punitive towards women. [...] It assumes that people do not want to work, and will not if given the choice. Lund (2008, p. 56)

2.2 Reach and take-up in the Income and Expenditure Surveys

2.2.1 Datasets and definitions

This paper uses data from the South African Income and Expenditure Surveys (IES), conducted in 2000 and in 2005. The two surveys have a repeated cross-section structure. The IES 2000 was conducted in October 2000, and used the recall method: a single questionnaire was administered at a single point in time. In contrast, the IES 2005 was conducted for a year, between September 2005 and August 2006. A new subsample was surveyed each month, and the respondent was asked to keep daily records of consumption and expenditure for that month

(this approach is known as the diary method in the literature). This difference in survey design could affect the mean and median level of income and expenditure across time, but it should be fully controlled for in the behavioral analysis.

The identifying variable is the number of age-eligible children in the household; that is, the number of children under fourteen years. The household respondent was asked to record the age in years for each household member in both surveys. However, in the 2005 data, the age of each household member was categorized into five-year groups (i.e. zero to four, five to nine, ten to fourteen, etc.). This operation made it impossible to distinguish between thirteen- and fourteen-year-olds in the data. We are thus forced to define age eligibility as having a child under the age of fifteen in the household, even though the actual eligibility threshold was fourteen.

Both surveys recorded income from the child support grant under the label “family grants”. On the questionnaire, it was clarified that family grants meant either child support grants or state maintenance grants. It is not possible to directly distinguish between these two sources of family grants in the data. However, this should be a minor problem, as the state maintenance grant was already partly phased out in 2000, and completely abolished in 2005. The two other major grants in South Africa, the disability grant and the social pensions, were recorded separately.

There were some changes in the measurement of income and expenditure between 2000 and 2005. The most important change was that the imputed rent of owner-occupied housing was added to both expenditure and income in the 2005 survey, following international praxis. This value was about 5.5 percent of the median household’s income in 2005. Since the size of this figure in 2000 is unknown, it is not possible to tell whether the value of housing has been constant or represents an important change in household wealth. Therefore, it is not obvious whether the value of owner-occupied housing should be subtracted from the 2005 income data when comparing the figures. However, when analyzing the behavioral effects of the child support grant in Section 5, mechanical changes in survey design will be completely controlled for. Therefore, the income and expenditure variables are kept “as is”, for both the descriptive and the behavioral analysis. For further documentation on the measurement and construction of the income and expendi-

ture variables, see Yu (2008) and the South African Central Bureau of Statistics (2008).

2.2.2 Who received family grants and how much?

Throughout the rest of this paper, all figures will be presented in constant prices (base year 2000). We will focus on African (black) households. Africans comprise about 80 percent of the South African population.

Summary statistics of income, expenditure and family composition are presented in Table 1 and 2. Between 2000 and 2005, average total income increased from R 23 000 per year to R 29 500. As is characteristic for South Africa, the story is somewhat different when looking at the median income, which rose from R 13 000 to R 16 000. Moreover, the difference between income and expenditure is negative in both years. This is typical, but the difference is lower in 2005, which could reflect the change in survey design discussed above. The median number of children per household is zero, and the mean number of children is lower than one; having more than three children in the household is rare.

Table 1: Economic and demographic characteristics of African households in South Africa 2000-2005.

	Pre-reform (2000)		Post-reform (2005)	
	Mean	Median	Mean	Median
<i>Economic variables</i>				
Income	23 005	12 960	29 528	16 118
Income excl. family grants	22 908	12 795	28 139	14 498
Disposable income	22 040	12 835	28 374	16 137
Expenditure	22 918	13 796	23 835	14 728
Earnings	17 372	6 720	21 645	7 754
Wage earnings	16 203	5 560	19 385	5 798
Family grants	97	0	1 389	0
<i>Demographic variables</i>				
Nr eligible children	1.36	1	1.34	1
Household size	4.00	3	3.93	3
Nr of males in household	2.11	2	2.00	2

Notes: Rand in 2000 prices. Earnings is wage earnings plus income from self-employment. All statistics are weighted by the inverse of the probability of selection, and are representative for all African households in South Africa.

The main variable, earnings, is defined as wage income – both full-time and part-time – plus income from self-employment. Earnings appear to have been fairly stable in South Africa between the two surveys. This might in part represent the fact that the wage rate has been virtually constant between 2000 and 2005 across a broad range of occupations and sectors (carefully documented in Berger and Yu 2007), but it might also reflect behavioral effects. The household composition (the number of children and the male/female ratio) also appears to be fairly constant across time (Table 2). This last fact is important. It suggest that the impact of the grant on fertility is sluggish, if it exists at all. The identification strategy, to be discussed shortly, requires that the treatment and control groups are stable across time (see e.g. Blundell and McCurdy 1999).

Table 2: Number of observations in the IES datasets, across number of children.

	IES 2000			IES 2005		
	Freq.	Share	Cum.	Freq.	Share	Cum.
No children	8 701	.42	.42	6 296	.39	.39
One child	3 937	.19	.61	3 273	.20	.59
Two children	3 593	.17	.78	2 998	.19	.78
Three children	2 303	.11	.89	1 757	.11	.89
Four children	1 201	.6	.95	945	.6	.95
Five children	558	.3	.98	437	.3	.98
Six children	268	.1	.99	210	.1	.99

Notes: Households with more than 6 children are not shown.

Did the family grant raise income? As seen in Figure A.1, the distribution of total income shifted to the right between 2000 and 2005. However, when excluding the family grant from the definition of total income, the difference is markedly lower (Figure A.2). As seen, households with low income net of family grants are relatively more numerous in 2005 than in 2000. All graphs are weighted so as to be representative for the entire African population in South Africa. Thus, in the absence of behavioral effects, the increase in family grants is almost completely driving the total increase in income, except for high income earners.

Table 3: Mean value of family grants to households in South Africa, across number of eligible children and time

	Pre-reform (2000)		Post-reform (2005)	
	Mean	Median	Mean	Median
No children	37	0	217	0
One child	92	0	1 044	0
Two children	135	0	1 946	1 709
Three children	144	0	3 162	3 377
Four children	186	0	4 067	3 489
Five children	281	0	5 019	5 090
Six children	185	0	5 869	5 455

Notes: Rand in 2000 prices. A 'child' is less than fifteen years old.

As seen in Table 3, the family grants appear to be a linear function of the number of children in the household. In contrast, there is no such pattern in 2000. However, the increase in family grants is less than the statutory value of the benefit. This could reflect that not all eligible children got access to the grant. Another factor that might confound the true impact of the grant is that primary caregivers do not necessarily live in the same household as their children. A final issue is that households fail to report the correct number of children residing in the household, perhaps because the child is temporarily residing elsewhere.

Having children in the household adds substantially to household income in 2005, but not in 2000, as illustrated by the cumulative distributions of income (Figure A.3 and A.4). The benefit of having a child is highest among low-income earners. However, there is no obvious kink point, as would be expected if the cut-off income value of child support eligibility (R 13 200) was binding. Thus, contrary to the presence of children, the absence of cash does not seem to be a strong signal for child support entitlement. This does not mean that child support is equally important for high-income earners as it is for low-income earners, however. As seen in Table 4, the *relative* impact of the child support grant is markedly higher among low-income households compared to high-income households.

Finally, the impact of the reform on current unearned income is studied by regressing family grants on a post-reform dummy, the number of eligible children in the household and the interaction between these two variables. Two approaches are employed, OLS and median regression. The results are reported in Table 5.

Table 4: Mean change in absolute and relative size of family grants between 2000 and 2005, across number of children and income quartile.

	Lowest quartile	Lower middle quartile	Upper middle quartile	Highest quartile	All African households
No children					
Absolute change	365	203	73	113	188
Relative change	0.029	0.010	0.003	0.001	0.011
One child					
Absolute change	1 364	1 023	984	621	998
Relative change	0.249	0.083	0.047	0.012	0.098
Two children					
Absolute change	2 845	1 794	1 846	999	1 871
Relative change	0.392	0.141	0.087	0.021	0.160
Three children					
Absolute change	3 408	2 825	3 360	2 558	3 037
Relative change	0.461	0.204	0.143	0.048	0.214
Four children					
Absolute change	4 577	3 768	3 971	2 969	3 821
Relative change	0.548	0.255	0.163	0.060	0.260
Five children					
Absolute change	5 192	4 873	4 872	3 663	4 649
Relative change	0.516	0.290	0.194	0.080	0.270
Six children					
Absolute change	6 370	5 504	5 358	5 525	5 689
Relative change	0.629	0.338	0.218	0.119	0.326

Notes: Absolute value represents the mean change in family grants in South African rands (2000 prices) between 2000 and 2005. Relative value represents the mean change in the relative size of family grants to total household income.

Table 5: The impact of the child support grant on unearned income.

	Median regression (1)	OLS (2)
Number of children x Post-reform	860.5*** (1.947)	909.7*** (15.85)
Number of children	9.47e-12 (8.34e-11)	38.00*** (10.42)
Post-reform	5.27e-10 (3.56e-7)	118.3*** (31.41)
Constant	-1.78e-10 (1.55e-10)	43.47** (20.55)
Observations	35956	35956

Notes: Sample includes all African household, except households with more than six children or more than seven adults. Median regression reports bootstrapped standard error in parenthesis; OLS regression reports heteroscedasticity-robust standard errors in parenthesis.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The estimates suggest that the child support grant increased current unearned income by about R 850-900 per child and year.

3 Model and empirical specification

3.1 Set-up

Our goal is to estimate the impact of unearned income on earnings. The basic idea is to compare the impact of an additional child before and after a child was rewarded by a grant; a simple difference-in-difference strategy. This strategy will take into account that additional children creates additional needs for the household at given income levels (a substitution effect), and avoids some additional problems arising from the measurement of life-time wealth and omission of unobservable variables. In this section, a simple framework for interpreting the results is outlined.

We shall focus on two aggregates of goods, leisure (l_t) and consumption of

market goods (c_t), and abstract from uncertainty about future utility.⁶ Consider a representative household with the following preferences:

$$U_t(l_t, c_t, t) = \sum_{t=1}^T (1 + \rho)^{-t} [\alpha_l \ln(l_t) + \alpha_c \ln(c_t - \gamma_c(n_t))], \quad (1)$$

where T is the life span of the household, ρ is the psychological discount rate and $\gamma_c(\cdot)$ represent the “basic need” for consumption, which, in turn, is a function of the number of children living in the household, n . Intuitively, an additional child can increase the need for certain child goods or services that do not add to the household’s utility. It seems reasonable that $\frac{\partial \gamma_c(n)}{\partial n} > 0$ and that $\gamma_c(0) > 0$, but these properties are not necessary for the empirical analysis. This utility representation is known as the Stone-Geary utility function, and closely resembles that studied in Imbens et al. (2001).

Let us define the time endowment as $1 = l_t + L_t$, where L_t is labor supply. With no initial assets and no bequests, the intertemporal budget constraint can then be written

$$\sum_{t=1}^T (1 + r)^{-t} [w_t + g_t(n)] = \sum_{t=1}^T (1 + r)^{-t} [w_t l_t + c_t], \quad (2)$$

where w_t is the value of the time endowment and $g_t(n)$ is exogenous income arising from having children in the household. The Stone-Geary representation of utility give rise to a linear demand system in earnings and expenditure. Denoting earnings at a particular point in time $y_\tau = w_\tau(1 - l_\tau)$, maximization of (1) subject to (2) yields

$$y_\tau = w_\tau - \alpha_l \left(\frac{(1 + r)}{(1 + \rho)} \right)^\tau \frac{\sum_{t=1}^T (1 + r)^{-t} [w_t + g_t(n) - \gamma_c(n)]}{\left[\sum_{t=1}^T (1 + \rho)^{-t} \right]}, \quad (3)$$

and

$$c_\tau = \gamma_c(\cdot) + \alpha_c \left(\frac{(1 + r)}{(1 + \rho)} \right)^\tau \frac{\sum_{t=1}^T (1 + r)^{-t} [w_t + g_t(n) - \gamma_c(n)]}{\left[\sum_{t=1}^T (1 + \rho)^{-t} \right]}. \quad (4)$$

⁶ Assuming no uncertainty is made in order to arrive at explicit solutions of the consumption function. It is possible to incorporate uncertainty using other functional forms, however, without losing the essential results.

Looking at equations (3) and (4), our primary interest is the parameters α_l and α_c . This interest comes from the empirical observation that labor supply appears to be invariant to long-run changes in the wage rate, and that this insensitivity can *not* be explained by a high propensity to save. The Stone-Geary model offers two alternative explanations to such a pattern. The first is simply that α_l is small and the amount of labor supplied to the market will be close to the time endowment regardless of wages and income. This explanation is perhaps closest to the remarks by Case and Deaton (1999) cited in the introduction: that labor supply in South Africa is determined by other factors than income and prices. The alternative explanation, closer to mainstream economics, is that α_l is positive, but that the income effect and the substitution effect cancel out.⁷

3.2 Identification

If we think of the rapid introduction of child support as an “income innovation”, unanticipated before the reform, the derivative of earnings with respect to the number of children in the household will capture an income effect in 2005, but not in 2000.⁸ Consider two overlapping generations, observed at different dates (before and after the reform) but at the same point in time relative their respective life cycles. Suppose this relative point in time is τ . To ease exposition, suppose also that the income innovation takes place at date τ for the post-reform sample. Let us denote the two generations using $i = post, pre$ and define

$$g_t^i(n) = \begin{cases} n \times grant & \text{if } i = post \text{ and } t = \tau, \tau + 1, \dots, \tau + S \\ 0 & \text{otherwise.} \end{cases} \quad (5)$$

In (5), S is the period of age eligibility (equal to 14 years if the child is given the grant from birth) and $grant$ is the constant size of the grant per year and

⁷See Kimball and Shapiro (2003) and the discussion in Cahuc and Zylderberg (2004, Chapter 1.3.1).

⁸Of course, one might argue that the reform could have been anticipated as early as in 1996, when the Lund Committee was starting its work. However, the background discussion and the data studied in Section 2 suggest that massive awareness of the grant was not achieved until 2001-2002. See also the discussion in Lund (2008).

child. Thus, earnings for generation i is:

$$y_\tau^i = w_\tau^i - \alpha_l \left(\frac{(1+r)}{(1+\rho)} \right)^\tau \left[\sum_{t=\tau}^T (1+\rho)^{-s} \right]^{-1} \\ \times \left[\sum_{t=\tau}^T (1+r)^{-t} (w_t^i - \gamma_c(n) + g_t^i(n)) \right].$$

Provided that the needs function $\gamma_c(\cdot)$ is constant across time, we have:

$$\frac{\partial y_\tau^{post}}{\partial n} - \frac{\partial y_\tau^{pre}}{\partial n} = -\alpha_l \lambda_\tau,$$

where

$$\lambda_\tau = grant \times \left(\frac{(1+r)}{(1+\rho)} \right)^\tau \left(\frac{\sum_{s=\tau}^S (1+r)^{-s}}{\sum_{t=\tau}^T (1+\rho)^{-t}} \right), \quad (6)$$

which is the discounted present value of child support per child and year. It can be readily verified that this identifying structure also pertains to the preference parameter of expenditure. That is:

$$\frac{\partial c_t^{post}}{\partial n} - \frac{\partial c_t^{pre}}{\partial n} = \alpha_c \lambda_\tau.$$

3.3 Empirical specification

We observe the two samples in 2000 and in 2005. The main empirical specification is

$$y_i = \theta_1 + \theta_2 D_{2005} + \theta_3 n_i - \alpha_l \lambda_\tau (n_i \times D_{2005}) + \boldsymbol{\theta}_3 \mathbf{X}'_i + \varepsilon, \quad (7)$$

where the subindex i denotes the household unit. D_{2005} is a post-reform dummy and \mathbf{X}_i are additional exogenous controls (dummies for province, rural/urban settlement and number of adults). An analogous equation, with expenditure on the left-hand side, is used to estimate $\alpha_c \lambda_\tau$:

$$c_i = \beta_1 + \beta_2 D_{2005} + \beta_3 n_i + \alpha_c \lambda_\tau (n_i \times D_{2005}) + \boldsymbol{\beta}_3 \mathbf{X}'_i + u. \quad (8)$$

3.4 Discussion

An underlying assumption is that the number of children, n , is exogenous. At least in the short run, this appears to be a reasonable assumption as there are obvious lags in the production of children. Notably, there was no baby boom in South Africa between the two periods of interest (Table 1). The fact that the production of children, as opposed to consumption and earnings, react sluggishly to economic incentives is the main rationale for using its variation for identification (rather than, say, receipt of family grants).

However, the structure in the previous section assumes more than a sluggish adjustment of child production. It also assumes that the number of children is *constant* throughout the life-cycle. This is a strong assumption, because completed fertility could very well be affected by the child support grant, even if short-run fertility remains constant. However, provided that future fertility only enters λ_τ , an important simplification can be made. By normalizing $\alpha_c + \alpha_l = 1$, we have

$$\frac{\alpha_l \lambda_\tau}{\alpha_c \lambda_\tau + \alpha_l \lambda_\tau} = \alpha_l. \quad (9)$$

Equation (9) implies that the preference parameters can be derived without making auxiliary assumptions about the parameters in λ_τ (as in Imbens et al. 2001). Nor is it necessary to estimate some form of first-stage relationship between the reform and total wealth, which, given the above considerations, is only imperfectly observed in the data (for instance, the estimates in Table 5 represents the impact of the reform on current income, which is not total life-time wealth). All that is necessary is the reduced form effect of an additional child on earnings and expenditure. Notice also that although equality (9) might seem trivial, it does require that the data include *all* consumption items, and not just a few durables. The fact that the data contain that much information is an important advantage relative existing literature.

We shall interpret α_l as the marginal propensity to earn out of one additional *non-saved* rand (i.e. the MPE at given savings). Again, an analogous procedure is used to derive α_c .

3.5 Estimation method

The child support grant was targeted to low-income earners, which is a caveat to identification because earnings are endogenous. The problem is reinforced by the income dispersion in South Africa, which is considerable even among Africans. The sample statistics of earnings (Table 1) suggest that the average household in 2000 was *not* entitled to the grant, but that the median household was. The fact that top-income earners have such a dramatic impact on average earnings suggest that OLS might do a poor job capturing the demand system of a theoretical Stone-Geary household, which is linear in earnings and expenditure. The average trend in earnings, for example, will be very much influenced by movements in the top decile. Truncating the data, or running separate regressions across different segments of the dependent variable, is not a satisfactory solution, as it would instead raise issues about selection (Heckman 1979).

The preferred estimates are obtained using median regression (least absolute deviation from the median) on the full sample. Median earnings within each year-child cell are far from the statutory threshold of R 13 200.⁹ Thus, in its simplest form (without any covariates), median regression will be completely insensitive to movements around this threshold. This is attractive, because such movements might not capture income effects, but rather the incentive to become eligible, which is something else.¹⁰ Moreover, the behavior of the median household is arguably more relevant for policy, as it was the number of poor households, not average earnings, that motivated the reform. The results from the median regressions will be compared to the more conventional approach of running OLS on a trimmed sample (excluding the top decile of earnings and expenditure).

⁹Median earnings is highest among households with no children in 2005: R 7 569.

¹⁰In other words, we have a strong a theoretical reason for attaching a zero weight to what the literature call “vertical outliers” (Rousseeuw and Leroy 1987). See also Koenker and Hallock (2001).

4 Results

4.1 Main results

For all estimates, the “full sample” refers to all African households, although very large households are also dropped.¹¹ The main results are presented in Table 6. The first specification does not include any additional controls. Thus, the predicted values from the first specification can be readily derived using the information in the table. For example, on this sample with no more than six eligible children, earnings are never predicted to be negative, although households with exactly six children are predicted to have close to zero earnings in 2005.¹²

The second specification includes dummies for province, settlement and family composition (coefficients not shown). These additional controls are relatively few, but since the reform involved such a large and permanent increase in income, many of the standard controls found in the literature (e.g. schooling) are endogenous. The estimated effect of the reform on earnings is about -300 , which is to be interpreted as the negative of $\alpha_l\lambda$. The effect appears to be robust to the inclusion of province-specific effects, settlement and controls for family composition.

The third specification controls for the number of children in the household using dummies, although the interaction term is still linear. In contrast, the fourth specification is completely non-parametric, and includes only dummy variables for mutually exclusive and exhaustive categories on the right-hand side of the regression equation. This specification allows for non-linear treatment effects, and is also robust to censoring of the dependent variable at zero earnings. Specification five, which again includes controls for household characteristics and locality, suggests that the reform effect is diminishing with the number of eligible children.

Estimates of the impact of the reform on expenditure are reported in Table 7, which follows exactly the same structure as Table 6. All estimates in Table 7 have the expected sign, and are statistically significant. In specification one, two and three, estimation is done with the restriction that the effect is linear, which give us

¹¹More specifically, households with more than six children or more than seven adults. This meant reducing the sample by about one percent.

¹²The fact that the baseline regression predicts no negative values is notable from an econometric viewpoint, because it implies that the censored least absolute deviation estimator, introduced in Powel (1984), would yield exactly the same point estimates.

Table 6: The impact of the child support grant on earnings. Median regression.

	(1)	(2)	(3)	(4)	(5)
Number of children x Post reform	-228.1*** (52.33)	-297.1*** (44.49)	-374.2* (194.5)		
Number of children	-1000.0*** (53.75)	-278.5*** (40.17)			
One child x Post reform				-89.06 (463.1)	-582.3* (352.4)
Two children x Post reform				-1532.8*** (393.2)	-1352.3*** (237.7)
Three children x Post reform				-1137.7 (726.4)	-1439.7*** (390.6)
Four children x Post reform				-2164.8*** (704.2)	-1880.8*** (432.4)
Five children x Post reform				-1569.5 (1195.1)	-1819.4*** (381.3)
Six children x Post reform				-1809.5* (1014.1)	-1756.4*** (630.3)
Post reform	168.7 (252.1)	631.6*** (116.1)	843.3* (488.0)	369.5** (187.5)	1186.3*** (290.8)
Constant	7200.0*** (121.7)	14353.4*** (520.1)	14522.6*** (625.4)	7200.0*** (87.47)	14466.8*** (496.9)
Additional controls	No	Yes	Yes	No	Yes
Dummy controls for nr of children	No	No	Yes	Yes	Yes
Observations	35956	35956	35956	35956	35956

Notes: Bootstrapped standard errors in parenthesis. Additional controls include dummies for province, settlement (urban/rural), adults in household, and adult males in household.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: The impact of the child support grant on expenditure. Median regression.

	(1)	(2)	(3)	(4)	(5)
Number of children x Post reform	823.5*** (80.09)	606.2*** (79.95)	584.8*** (84.05)		
Number of children	300.5*** (60.83)	212.0*** (51.05)			
One child x Post reform				2604.4*** (420.1)	2096.7*** (275.5)
Two children x Post reform				2281.1*** (469.0)	1965.9*** (382.9)
Three children x Post reform				2550.6*** (437.3)	2097.8*** (375.1)
Four children x Post reform				3466.1*** (609.8)	1953.8*** (480.4)
Five children x Post reform				3932.2*** (689.2)	3311.7*** (625.7)
Six children x Post reform				4622.7*** (1045.1)	3132.5*** (633.4)
Post reform	-341.6 (209.7)	332.4** (157.0)	371.4* (197.5)	-879.2*** (232.8)	-74.47 (159.4)
Constant	12601.0*** (100.5)	12727.3*** (368.8)	12571.4*** (342.1)	12187.0*** (214.1)	12867.6*** (324.8)
Additional controls	No	Yes	Yes	No	Yes
Dummy controls for nr of children	No	No	Yes	Yes	Yes
Observations	35956	35956	35956	35956	35956

Notes: Bootstrapped standard errors in parenthesis. Additional controls include dummies for province, settlement (urban/rural), adults in household, and adult males in household.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

an estimated effect of about R 600-800. Including additional exogenous controls in the regression has a statistically significant effect on the estimates. Moreover, the dummy-based models (specification four and five) reveal that the marginal effect on expenditure is diminishing and markedly higher for the first child.

4.2 Additional heterogeneity and robustness analysis

4.2.1 OLS on a truncated sample

One might ask how the estimates above compare to the more conventional OLS approach. However, as mentioned, running OLS on the full sample would effectively mean that we estimate the average effect of the reform on a sample in which the average household is not eligible to receive the grant (by a fair margin). However, by removing the top decile of the earnings distribution and expenditure distributions, we end up with a much more policy relevant sample for the OLS estimation. As seen in specification in Table 8, these trimmed OLS estimates are also close to the median regression estimates.

4.2.2 Heterogeneity across household structure

Although the theoretical model laid out in Section 2 does not imply differential effects across different values of, say, the time endowment, intuition suggests that there might be. For instance, it might be more difficult to adjust labor supply for a sole household provider compared to a household in which several adults resides. In Table 9, the sample is divided across households with different number of adult members. In these regressions, we again control for province and locality.

The results in Table 9 suggest that expenditure among single-adult households is more responsive to the reform relative other household compositions. The difference in point estimates are economically significant, although the low number of observations in some subgroups make the estimates somewhat imprecise. Notice, however, that the impact of the reform on earnings is approximately the same across family composition. Thus, households with many adults save more than others. We will return to this issue in Section 5.

Table 8: The impact of the child support grant on median earnings and expenditure. OLS on truncated sample.

	Earnings		Expenditure	
	(1)	(2)	(3)	(4)
Number of children x Post reform	-133.1* (75.50)	-264.1*** (71.92)	764.1*** (67.94)	694.1*** (64.05)
Number of children	-724.5*** (50.58)	-480.2*** (50.85)	141.6*** (45.90)	11.99 (44.92)
Post reform	-490.6*** (164.7)	193.7 (158.2)	-586.1*** (141.4)	-66.01 (133.4)
Constant	9807.2*** (106.1)	13984.5*** (360.7)	14264.5*** (97.23)	14624.0*** (323.1)
Additional controls	No	Yes	No	Yes

Notes: OLS regression results, truncated at the 90th percentile in earnings and expenditure. Each panel and column represent separate regressions. Additional controls are dummies for province, settlement (urban/rural), adults in household, provided that the specific the sample restriction allows for variation in these variables. Robust standard errors in parenthesis.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9: The impact of the child support grant on median earnings and expenditure. Subsample results.

	Earnings			Expenditure		
	One adult	Two adults	More than two adults	One adult	Two adults	More than two adults
	(1)	(2)	(3)	(4)	(5)	(6)
No. of children	-290.9*** (107.8)	-337.4*** (115.7)	-280.8** (130.0)	955.9*** (148.2)	543.5*** (130.2)	442.0*** (144.0)
No. of children x Post reform	-125.3* (70.41)	-390.0** (153.1)	-397.1*** (101.9)	160.0 (147.0)	42.00 (145.8)	-299.0*** (106.5)
Post reform	456.5* (271.5)	1075.0*** (214.7)	522.3 (445.3)	-408.9* (242.0)	516.5* (308.9)	983.7*** (377.5)

Notes: Median regression results. Each panel and column represent separate regressions. All models include dummies for province and locality (rural/urban) and a dummy for the presence of kids in the household. Bootstrapped standard errors in parenthesis.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.2.3 The effect on labor supply

In Table 10, a linear probability model (OLS with a binary response variable) is employed to study the impact of the reform on the probability that the household make positive earnings. The outcome variable in these regressions could be viewed as a measure of labor force participation. As seen, the impact of the reform on labor supply appears to run partly via the extensive margin, as there is a clear, although not very large, effect on participation. These estimates are closer to the empirical labor supply literature, which typically deals with participation. However, contrary to the earnings and expenditure equations estimated in the previous section, they cannot be used to back out the Stone-Geary preference parameters.

5 Interpretation

The marginal propensities associated with the estimates in Table 9 are reported in Table 11. The propensities are obtained using equation (9). The results imply that α_l is about 0.25–0.4, which is close to the estimates in Kimball and Shapiro (2003), although they are larger than the baseline estimates in Imbens et al. (2001). This is perhaps surprising, given that incentive issues of labor force participation are sometimes considered to be less important in developing countries (Case and Deaton 1999).

To comprehend these figures, we must note that α_l and α_c represent the marginal propensity to earn and consume an additional *non-saved* rand. Comparing the estimates obtained in this section with the marginal impact of the reform on unearned income (Table 5), it is notable that the reduction in earnings plus the increase in expenditure is not statistically different from the increase in family grants. However, single-parent households have a negative propensity to save, whereas households with many adults have a positive propensity to save. Whether this makes single-parent households impatient or not depends on their life expectancy relative the length of the child support grant – that is, to what extent the child support grant represented a permanent shift of household income. It is theoretically possible that savings react negatively to the reform. Intuitively, households can disave or borrow against the promise of future income if the psy-

Table 10: The probability of making positive earnings. Ordinary least squares.

	(1)	(2)	(3)	(4)
Number of children x Post reform	-0.00898*** (0.00345)	-0.0121*** (0.00333)		
Number of children x Post reform	-0.0343*** (0.00226)	-0.0243*** (0.00237)		
One child x Post reform			-0.0634*** (0.00894)	-0.0700*** (0.00899)
Two children x Post reform			-0.0660*** (0.00930)	-0.0606*** (0.00941)
Three children x Post reform			-0.102*** (0.0113)	-0.0802*** (0.0113)
Four children x Post reform			-0.140*** (0.0152)	-0.0999*** (0.0148)
Five children x Post reform			-0.205*** (0.0223)	-0.152*** (0.0216)
Six children x Post reform			-0.181*** (0.0320)	-0.131*** (0.0312)
Post reform	0.00493 (0.00647)	0.0235*** (0.00638)	0.00639 (0.00733)	0.0262*** (0.00730)
Constant	0.724*** (0.00424)	0.864*** (0.0120)	0.729*** (0.00477)	0.869*** (0.0121)
Observations	35956	35956	35956	35956
Additional ctrls	No	Yes	No	Yes
Dummy ctrls for nr of children	No	No	Yes	Yes
Observations	35956	35956	35956	35956

Notes: Dependent variable equals one if household make positive earnings, zero otherwise. Heteroscedasticity-robust standard errors in parenthesis. Additional controls are dummies for location (rural/urban), province and number of adults in household.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 11: Derived marginal propensities to consume and earn out of unearned income

	One adult (1)	Two adults (2)	More than two adults (3)
$-\alpha_l\lambda$	-290.9	-337.4	-280.8
$\alpha_c\lambda$	955.9	543.5	442.0
$\alpha_l\lambda + \alpha_c\lambda$	1246.8	880.9	722.8
α_l	0.23	0.38	0.39
α_c	0.77	0.62	0.61

chological discount factor, ρ , is large enough. Thus, the critical issue is how large ρ is.

However, the data do not appear well suited to estimate ρ . As noted in Section 2.2, precise information on the childrens' ages is absent. Moreover, there are some important non-linearities involved in the discounted present value of future wealth that make estimation difficult. Instead, we present some numeric calibrations of the psychologic discount rate ρ under no savings, using equation (6). The calibration is done using an interest rate equal to 0.05. The result from this calibration is presented in Table 12.

Table 12: Derived psychological discount rates

	Years of age eligibility		
	15	10	5
Remaining life span			
60	0.096	0.129	0.231
45	0.095	0.129	0.231
30	0.089	0.126	0.231
15	0.050	0.097	0.219

Notes: Calculations based on an interest rate equal to 0.05 and no savings, and under the assumption of constant future fertility.

The lowest implied discount rate is $\rho = 0.05$. This discount rate is obtained under the assumption that the grant is expected to last for fifteen years and this

exactly matches the remaining life span. In this case, λ equals unity and the grant represents a one-to-one increase in permanent income. In contrast, the highest calibrated psychological discount rate is equal to about 0.2, which seems to suggest some impatience. However, this calibration assumes that households expect the child support grant to last for only five years, but that they will live on for an additional sixty years. It represents the upper-bound of possible discount rates. A discount rate equal to approximately 0.1 seems to fit reality best, which is twice the size of the interest rate but does not appear extreme.

6 Concluding remarks

This paper provides estimates of the marginal propensity to earn and consume out of unearned income in South Africa. The estimates suggest that labor supply is more sensitive to long-run changes in unearned income than previous literature on cash transfers in developing countries have found (see e.g. Skofias and Parker 2005). However, the estimates presented here are closer to those obtained in Imbens et al. (2001) and Kimball and Shapiro (2003). This is perhaps less difficult to reconcile than it may first seem, since the nature of the cash regime studied in this paper more closely compares to the US papers.

As noted in Case et al. (2005), an important issue in these kind of papers is selection: the extent to which households can *become* eligible to receive the grant. This concern was the rationale for using demographic factors and time as means of identification, rather than cross-sectional variation in grant receipt. Moreover, we used median regression to evade the problem of endogenous earnings around the income eligibility threshold. Admittedly, when it comes to the net effect of the reform on earnings and consumption, there might still be problems of endogeneity, in particular as regards future children. However, as noted in Section 3.4, the preference parameters of consumption and leisure are retrieved under the assumption that the substitution effect of an additional child on earnings is constant in the short run, but not necessarily in the long run, which seems plausible. These parameters are what matters when it comes to out-of-sample relevance.

The results have implications for public policy. For example, the estimates

tell us that a quarter of each additional rand in child support was *not* used to buy school material or health products, but to increase parental leisure. Thus, the successful impact of the child support grant on schooling, reported in Case et al. (2008), was realized even while eligible households earned less. Of course, this is not necessarily a bad thing. Parental leisure could be beneficial for children. Moreover, from a neoclassical viewpoint, cash transfers would certainly be the first-best policy alternative if only the welfare of the primary caregiver was considered. But from a fiscal perspective, leaving aside administrative obstacles, the politically approved outcome (increasing child nutrition and child schooling) could perhaps have been achieved using less funds and more conditionality.

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A Table Appendix

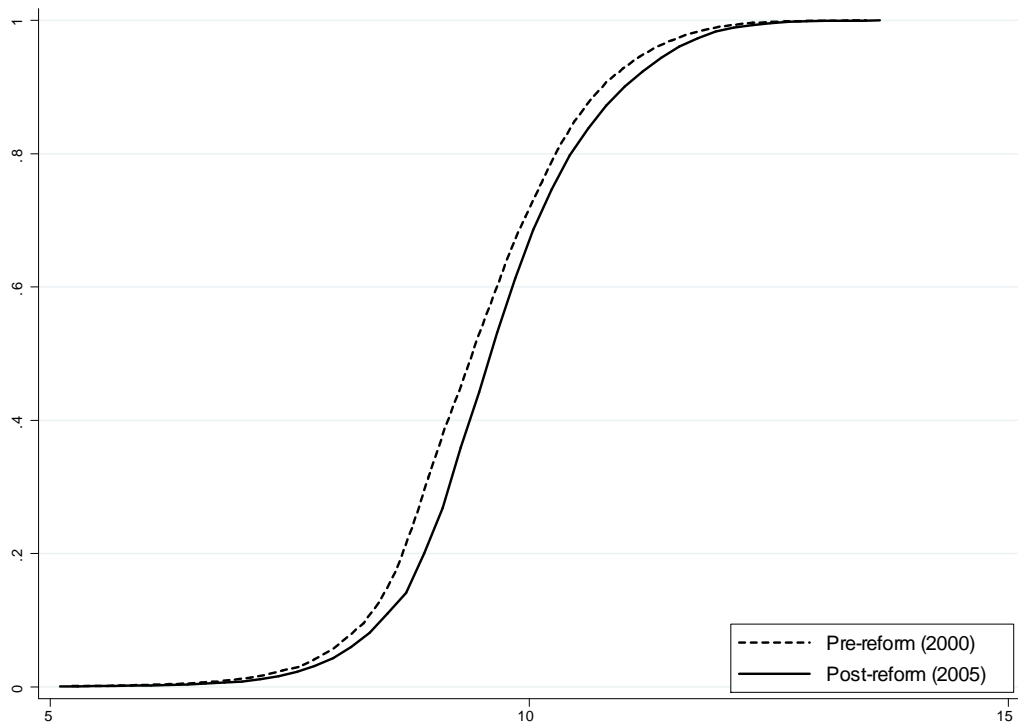


Figure 1. Cumulative distribution of (log) income in South Africa.

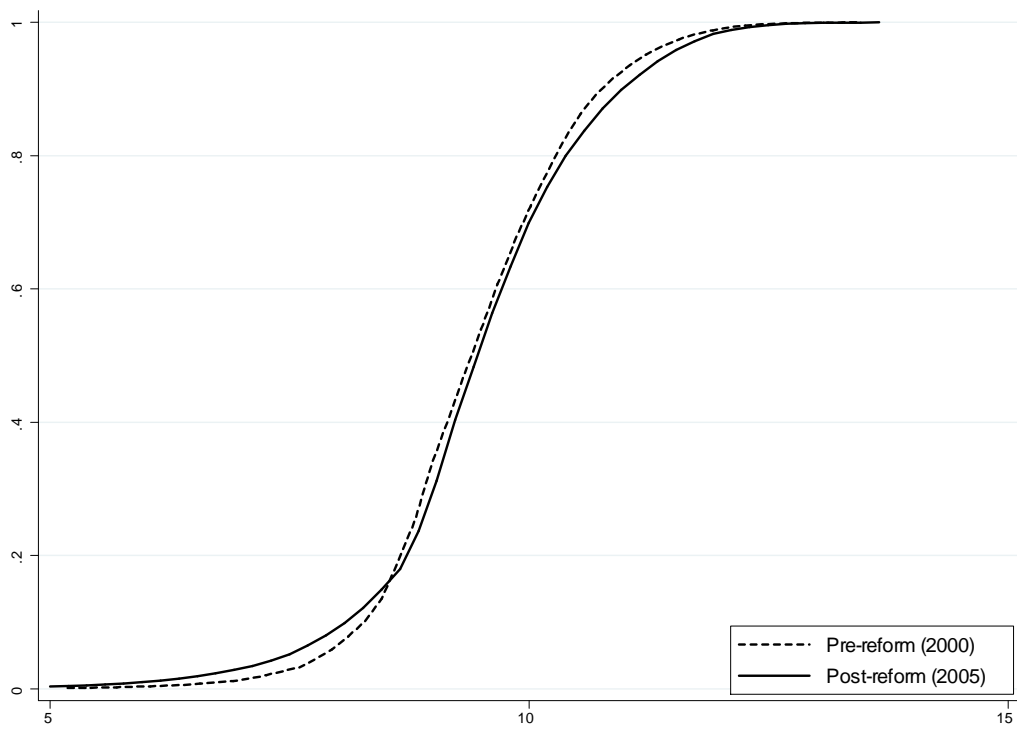


Figure 2. Cumulative distribution of (log) income in South Africa, excluding family grants.

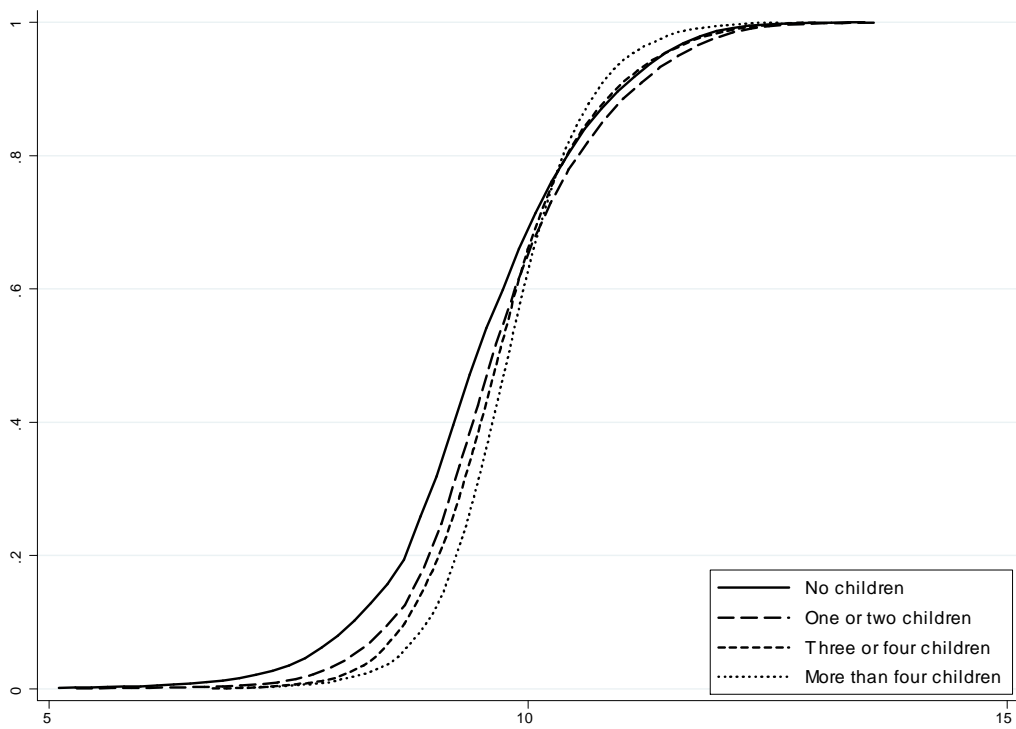


Figure 3. Cumulative distribution of (log) income in 2005.

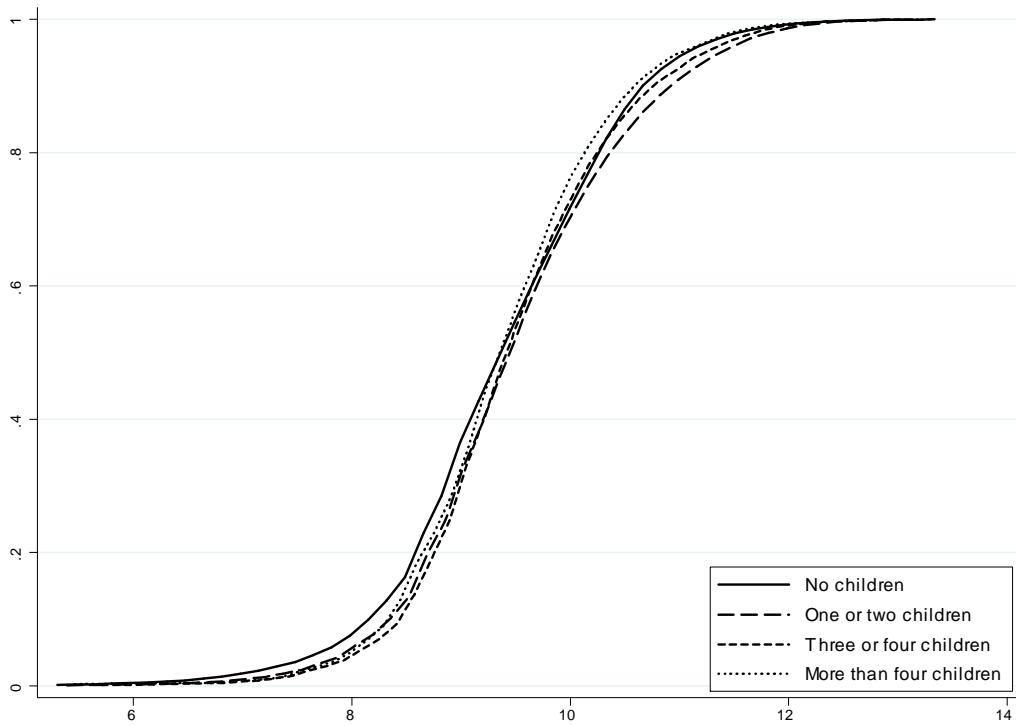


Figure 4. Cumulative distribution of (log) income in 2000.

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