The Social and Economic Impacts of South Africa's Child Support Grant

(Extended Version)

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Abstract

This paper uses exogenous variation in eligibility and grant take-up to evaluate the impacts of the Child Support Grant, an unconditional cash transfer program in South Africa, over the period 2002-2005. I find that increased probability of receiving a Child Support Grant is associated with increased school attendance, decreased child hunger, and increased broad labor force participation, while it has no identifiable effect on narrow labor force participation or employment. The magnitude of these effects is economically significant: most notably, grant receipt appears to decrease the probability that a school-age child is not attending school by over half. Although not strictly comparable, this effect is actually larger than Skoufias (2001) measures for Progresa transfer program in Mexico, which conditions grant payment on child school attendance. These results are robust across different specifications, but the CSG's effects appear to be most positive among mothers living in informal dwellings and mothers and household heads with less education. Although the grant's impact on school attendance is the same for boys and girls, the effect is much larger for children that are living with their mother. The CSG has its effect on school attendance almost entirely on the child who receives the grant, rather than being spread equally among all children in the household, which suggests that grant income is not pooled with other household income sources, contrary to previous studies conducted on the Old Age Pension.

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1. Introduction

In the past decade, South Africa's social welfare system has come to play an increasingly important role in the government's poverty reduction strategy, and its restructuring has been one of the most visible and controversial tasks undertaken by the new government. The social welfare system dates to 1928 for whites and 1944 for blacks, with differing grant amounts and eligibility for different racial groups until the government began to close these gaps during the 1980's. The current structure was created by a series of reforms in the 1990's (both pre- and post-1994), and consists of three main grants: the state Old Age Pension (OAP), Disability Grant (DG), and Child Support Grant (CSG). Over 96% of welfare beneficiaries receive one of these three grants. This system has remained unchanged since the CSG's implementation in 1998, with adjustments only to grant sizes and age eligibility. The reach of the latter two grants has expanded rapidly in recent years – the DG due to the spread of HIV/AIDS, and the CSG due to increasing public awareness and government efforts to increase take-up. By April 2005 roughly one in five South Africans were receiving a social grant from the government, of which 60% were CSGs, (National Treasury 2007, p.105) compared to one in ten receiving a grant in 2002, of which approximately 40% were CSGs (National Treasury 2005, p.57). Pensions and grants were the main source of income for 20.2% of all households in 2002 and 28.9% in 2005 (National Treasury 2007, p.101).

Despite its demonstrated role in poverty reduction (Samson et al 2004, Woolard 2003, Taylor Committee 2002), the public, policymakers, and academics often view the social protection system with a degree of skepticism. Their critiques generally fall into the category of arguments about "welfare dependency." In its strongest form, the welfare

dependency argument holds that social grants discourage employment, support lazy and/or immoral behavior, and are a short-term solution at best. These arguments often have strongly moralistic overtones and are couched in normative cultural assumptions about work, class, family structure, and gender, but they also pose some serious economic concerns: does the incentive structure of social grants have the effect of keeping recipients from taking steps that would help them escape poverty? These concerns cover areas as diverse as household formation, reproductive decisions, and labor market activity, but this last issue is the most prominent. Although the theoretical links between social grants, poverty, employment, and domestic labor are complex, widespread unemployment (26.7% in September 2005 by the official definition, 40.1% by the broad definition that includes discouraged workers) is clearly the defining feature of poverty in the country, and so it is important to understand the labor market impacts of social grants.

There is now a fairly extensive literature on the social and economic impacts of the Old Age Pension that has broadly confirmed its importance for poverty reduction and turned up mixed results regarding possible perverse labor market incentives. However, there have been fewer studies of the Child Support Grant, and none that have been able to establish a causal relationship between grant receipt and employment. There are a number of reasons for the relative paucity of studies: the CSG is a newer grant, is less than one fourth the amount of the OAP and DG, and is difficult to study on a national level with existing data. However, the CSG is also the only one of the major grants that is typically paid to a healthy person of working age (only 4% of OAP recipients remain in the labor force), and so we might expect it to have different effects. In particular, the CSG is paid to groups that are known to be vulnerable: 76.7% of CSGs are paid to

African[1] females of working age, and 26.3% are to African females under the age of 30. The official unemployment rate in this latter group is 37.3%, but when using the broad definition that includes individuals who want to work but are not actively searching, the rate skyrockets to 75.5%. The CSG is also the only grant with a means test that is stringent enough to be a binding constraint on many poor families, which might create a disincentive effect.

This paper will therefore focus on the CSG's impacts, and will deal with three different areas: labor market activity and employment, school attendance, and hunger. Each of these areas is important for immediate wellbeing, and each also addresses the question of "dependency," in different ways. The center of this paper is a model that exploits exogenous variation in grant eligibility and amount between 2002 and 2005 to create a natural experiment that I believe generates an unbiased estimate of the impact of social grants. Consistent with previous research, I find that the Child Support Grant leads to decreased child hunger and increased school attendance. Both of these effects are statistically and economically significant. In these regards, the grant is clearly fulfilling its purpose of alleviating poverty and improving the future prospects of children in poor families.

However, perhaps the most interesting result is that contrary to popular assumption, there is no evidence that the Child Support Grant lowers employment or labor force participation rates. Instead, there is strong evidence that CSG receipt increases

[1] Throughout the paper I use Statistics South Africa's racial categories, which are generally accepted in South Africa. "African" refers to black South Africans, "Coloured" refers to people of mixed-race origins, "Indian" refers to people of Indian and other Asian origin, and "White" refers to people of European descent.

[2] I use the term "CSG recipients" to refer to the adults to whom the grant is paid, although legally the child is the recipient of the grant, not the caregiver.

broad labor force participation and in certain circumstances may actually increase narrow labor force participation and employment rates slightly.[3] The CSG has no impact on the labor force behavior of the husbands of women receiving CSGs for their children.

This is an important finding for the debate about South Africa's social protection system, and sheds light on the possible effects of policy proposals such as relaxing or removing the means test, extending the Child Support Grant up to age 18, and implementing a universal, non-means tested Basic Income Grant. This paper's main contribution is that it provides what I argue is an unbiased estimate of the impact of social grants by avoiding the sources of bias that have plagued previous analyses of social grants. However, it does so at the expense of exploring the finer dynamics of the situation and separating out different effects, and so further research is necessary to uncover details and elaborate on the transmission mechanisms of these effects.

The remainder of this paper is organized as follows. Section 2 provides background on the grants and Section 3 reviews the existing literature on the social and economic impacts of social grants in South Africa. Section 4 explores the dynamics of social grants and the labor market through descriptive statistics and labor force transition matrices, and Section 5 develops and presents a labor force model based on the natural experiment created by exogenous increases in the CSG's eligibility and take-up. Section 6 uses this natural experiment to estimate the CSG's impact on hunger and school attendance, and Section 7 concludes and suggests directions for future research.

[3] The broad labor force consists of individuals who would accept a suitable job if it were offered to them. The narrow labor force is the subset of these individuals who are actively searching for work or are employed.

2. South Africa's social grants

South Africa's social grant system is unique among developing countries for its scope. The system comprises seven different grants: the Old Age Pension (OAP), Child Support Grant (CSG), Disability Grant (DG), War Veterans pension, Foster Care Grant, Care Dependency Grant, and Grant in Aid. Table 2.1 below shows the numbers of grant beneficiaries by grant type for 2001-2005.

Table 2.1 Social grant beneficiaries by type of grant, April 2001 - April 2005

Type of grant	April 2001	April 2002	April 2003	April 2004	April 2005	% growth (average annual)
Child support	974,724	1,907,774	2,630,826	4,309,772	5,633,647	55.1%
Old Age	1,877,538	1,903,042	2,009,419	2,060,421	2,093,075	2.8%
Disability	627,481	694,232	953,965	1,270,964	1,307,459	20.1%
Care dependency	28,897	34,978	58,140	77,934	85,818	31.3%
Foster care	85,910	95,216	138,763	200,340	256,325	31.4%
War veterans	6,175	5,266	4,594	3,961	3,340	-14.2%
Grant-in-aid	9,489	10,332	12,787	18,170	23,131	25.0%
Total	3,610,215	4,650,840	5,808,494	7,941,562	9,402,795	27.0%

Source: Adapted from National Treasury (2005, p.57)

In 2005, a total of 9.4 million individuals were receiving a social grant from the government, out of a total population of roughly 47 million. Over half of these were children receiving CSGs, and most other beneficiaries receive either the OAP or the DG. Political, socioeconomic, and demographic trends have contributed towards the significant increase in grants over the period 2001-2005. Although the reach of the OAP, the most well-established grant, only slightly increased, the number of beneficiaries of the DG more than doubled, largely due to the spread of HIV/AIDS, and the number of CSGs quintupled due to increases in the age eligibility limits and take-up. Total social

grant expenditure has risen from R20.6 billion (2.0% of GDP) in 2001/02 to R44.9 billi
(3.2% of GDP) in 2004/05 (National Treasury 2005, p.56).

The amount of the grants and the age eligibility for the CSG also increased gradually over the course of this period, as detailed in Table 2.2 below.
The rate of increase in grant size was just slightly faster than inflation for most of this period.

Table 2.2 Change in grant amount and eligibility, 2000-2005

Grant	Apr. 2000	Apr. 2001	Apr. 2002	Oct. 2002	Apr. 2003	Apr. 2004	Apr. 2005
OAP/DG (Rand/month)	540	570	620	640	700	740	780
CSG (Rand/month)	100	110	130	140	160	170	180
CSG age eligibility	Under 7	Under 7	Under 7	Under 7	Under 9	Under 11	Under 14

The Department of Social Development, which is responsible for administering social grants, applies a means test to applicants. For the Child Support Grant, which is paid to the child's primary caregiver,[4] the caregiver and his/her spouse must have a combined monthly income of less than R800 for applicants from urban areas who live in formal dwellings, or R1100 for urban applicants living in informal dwellings and rural applicants.[5] These limits have not been adjusted for inflation since they were set in 1998, and so in effect, in early 2007 children had to be 50% poorer to qualify than they did in

[4] In practice, the primary caregiver is usually the child's mother. If the mother is not present, other family members (usually grandparents or the father) may apply. The Foster Care Grant covers children not living with their biological family. See Appendix A for details on caregivers.

[5] Delivery of social grants is done at the level of provincial governments, which differ in the details of their application and payment procedures. Anecdotal evidence suggests that differences remain between the provinces, with some being more active in trying to expand grant take-up than others. There is also variability within provinces, especially in rural areas where a single welfare officer with little oversight may have responsibility for a broad area.

1998. If the thresholds had kept pace with inflation, in early 2007 they would have stood at roughly R1200 and R1650 (Hall 2007). However, anecdotal evidence suggests that in practice, the means test is enforced with varying degrees of strictness, often depending on the individual welfare officer.

3. Literature review

3.1 Labor market impacts

Micro-economic analyses of the labor market impacts of social transfers usually focus on labor supply, and theorize two types of negative effects: income effects and incentive effects. The theory behind income effects is simple: as people become wealthier, their marginal benefit from additional consumption decreases relative to their marginal benefit from additional leisure, and so they will tend to work less (or be less likely to search for a job). In the South African context, this theory has often been applied to the Old Age Pension (Bertrand et al 2000, 3). Although the pension targets people who are generally out of the labor force, Case and Deaton (1998) showed that pension income is usually pooled with other household income, and could therefore affect the decisions of household members of working age.

In September 2004, the OAP and DG were each R740/month, and the CSG was R170/month for each child up to a maximum of six.[6] These amounts are substantial for a poor family in South Africa. Table 3.1 below presents the median reported monthly labor income of employed individuals in September 2004, by quintile of labor income (not controlling for hours worked). Although the median wage earner was paid R1200/month,

[6] The average CSG recipient actually received 1.34 CSGs, amounting to R228.

53.6% of households report monthly household expenditures of less than R800. This gap largely reflects the prevalence of unemployment in the country, illustrated by the fact that 40.9% of households report no labor income whatsoever.

Table 3.1: Median labor income by quintile, employed workers

	Quintile of labor income				
	1	2	3	4	5
Median monthly labor income (Rand)	240	700	1200	2500	6700

Source: LFS September 2004

The incentive effects of social grants could operate both directly and indirectly. Directly, means-tested social grants disincentivize employment by requiring beneficiaries and their spouses to fall below a certain income threshold. In South Africa, this effect would apply principally to the CSG, which imposes thresholds of R800 or R1100 per month – low enough to disqualify wage earners above the 34th and 46th percentile of workers.[7] Using 2004 grant amounts, if an urban mother receiving a CSG for her two children took a job that paid R900, she would lose R340 in social grants, or 37.8% of her wages, in addition to any other taxes, transportation costs, and foregone leisure and domestic labor. Even for a poor mother in desperate need of income, it may not be worth it to take the job.

Alternatively, the direct disincentive effect may result from a problem of intra-household allocation. If the members of the household share a grant, and the amount an individual successfully claims varies inversely with her own income, the incentive to work could be reduced (Bertrand et al 2000, 3).

[7] The means test applies to the combined income of a child's primary caregiver and the caregiver's spouse. In practice, the enforcement of these limits varies.

Social grants represent a significant injection of resources into poor households, but the side effects of this might indirectly disincentivize employment by affecting the household affiliation decisions of working-age individuals. Although most household-level studies of poverty consider household formation to be exogenous, in high-poverty situations individuals will tend to join households with more resources. In South Africa, social grants represent a large proportion of income for the poor, and since OAP and CSG recipients are disproportionately concentrated in rural areas (as with the population of children and the elderly in general), this may lead to working-age individuals attaching themselves to rural households. This could occur either by migration to rural areas, or by rural youth delaying leaving home to set up their own households or to join relatives in urban areas. Since unemployment rates and the costs of job search both tend to be higher in rural areas than in urban areas, using social grants to cope with poverty could pull workers away from jobs and indirectly discourage job search (Klasen and Woolard 2005).

However, there are also several channels through which social grants, and particularly the CSG, might lead to improved labor market outcomes. On an international level, this potential of cash transfers has been recognized by a variety of observers (Ravallion 2003; Devereux and Sabates-Wheeler 2004; Samson et al 2002 & 2004). The common factor in these theories is the idea that having a steady income in the form of a grant, or being in a household that does, may make it possible for poor individuals to make high-return investments that liquidity constraints would otherwise prohibit. Some, but not all, of these investments may be directed towards seeking remunerative employment.

Most directly, social grants may play an important role in financing job search (Samson et al 2004; Kingdon and Knight 2000). Searching for a job can be expensive, both in terms of expenditures like transportation or telephone calls and the opportunity cost of not performing other remunerative or non-remunerative labor. Childcare can be another expense: in their study on a sample drawn from working class Cape Town, Shoër and Leibbrandt (2006, p.22) report that the "most significant constraint on the ability of passive searchers to pursue [more active] search methods is the obligation to do domestic duties." Having a steady income in the form of a grant, or being in a household that does, may make job search possible or improve its effectiveness. Alternatively, this income might permit workers to migrate in pursuit of employment, both by financing their move and by providing for children and other dependents that remain in the household of origin (Posel et al 2006).

A variant of the search-financing theory applies to holding a job. A grant may allow a single mother to place a young child in a crèche, or daycare center, while she is working. One possible confounding factor is that having access to some means of support may allow individuals to be more selective about accepting jobs. While this would appear to have a negative impact on employment, it may actually be economically beneficial; if individuals simply take the first job that comes along out of desperation, high productivity worker-firm matches may be precluded (Wittenberg 2002, p.1166).

The role of social grants in reducing the cost of job search may be especially important in households that have recently experienced negative shocks. In the context of HIV/AIDS, Booysen (2004, p.543) writes that social grants may "be important in mitigating certain aspects of the impact of the epidemic, *e.g.* ensuring food security,

making sure that children attend school and mitigating the burden of funeral costs, particularly in the case of households that have directly experienced illness or death and that are chronically poor." Having access to a stable income may help household members seek employment as a response to such crises.

Alternately, the steady income stream from the grant may be used to finance small enterprise creation. Lund (2002, p.684) cites studies by Ardington and Lund (1995) and Cross and Luckin (1993) showing that the pension was being used "to secure credit, hire equipment, buy improved agricultural inputs, and showed the importance of the regularity of the pension across the production cycle – ploughing, planting, weeding, and harvesting." Although social grants are clearly responsible for generating significant amounts of economic activity, as evidenced by the markets that spring up at pay points on the monthly pension disbursal day (Lund 2002, p.686), their role in funding small enterprises has gone largely uninvestigated. In its small sample, the Financial Diaries (2005) study found that between 10% and 25% of grant households are also running small businesses, although there was not enough evidence to conclude that social grants helped individuals start and run small businesses.

Social grants may also improve a worker's productivity and therefore make her more attractive to hire (Samson et al 2002, p.23). Workers that are malnourished or in poor health are likely to be less productive, and so increasing their consumption can increase their employment prospects. Similarly, increased income may allow workers to invest in training and education, although it is possible that the grants would be used mainly to fund education for children, rather than adults. Hunger and health are also

important constraints to job search: Shoër and Leibbrandt (2006, p.20) report that in their sample, health problems hindered 13% of searchers, while hunger hindered 25%.

One interesting possibility is that social grants may act as an indirect wage subsidy by reducing the necessity of sending remittances and thereby encouraging even non-recipients to work more. Remittances continue to play an important role in the South African economy, in part because of the lack of government support for the unemployed, but it is the working poor and working class who bear the heaviest burden in providing for their even poorer friends, family, and neighbors. This effectively creates a tax on income (Samson et al 2002, p.22). However, there is strong evidence that when a household receives a social grant there is a drop in the value of remittances it receives, meaning that the sender of the remittances gets to keep more of her money as a result (Jensen 2003).[8] This lowers the effective tax on labor and therefore encourages increased labor supply. However, this theory is difficult to test with existing data, because it requires information about both the household that sent the remittances and the household that received them.

Although the above hypotheses provide a rich picture of how social grants could interact with employment among poor individuals, they are difficult to examine empirically, and so our understanding of the situation is limited. Although they did not focus on social grants, Kingdon and Knight (2000, p.17) find evidence against the "luxury unemployment" hypothesis that lack of job search among the unemployed is

[8] Jensen estimated the elasticity of this relationship as 0.25-0.30. That is, "every rand of public pension income leads to a 0.25-0.30 rand reduction in private transfers from children living away from home." (2003, p.89)

voluntary, arguing that it instead reflects constraints imposed by "poverty, cost of job-search from remote rural areas, and high local unemployment."

To my knowledge, all existing studies of the labor market effects of social grants have focused on the impact of the OAP, not the CSG, with the partial exception of Samson et al (2004). The first major study was conducted in 2000 by Bertrand, Miller, and Mullainathan, who studied the impact of the OAP on the employment of prime-age workers in three-generation households (grandparents, parents, and children). Using data from the 1993 Project for Living Standards and Development (PSLSD) survey, they find a drop in the labor force participation of prime-age men when the elders in the household reach pension age. They attribute this partly to an income effect and partly to a disincentive effect related to intra-household allocation, but place heavy emphasis on the latter explanation:

> "Other findings suggest that power within the family might play an important role: (1) labor supply drops less when pension is received by a man rather than by a woman; (2) middle aged men (those more likely to have control in the family) reduce labor supply more than younger men; and (3) female labor supply is unaffected." (p.1)

According to their estimate, this effect is rather large – a ten percent rise in income from the pension leads to roughly a ten percent drop in labor supply – but their estimate is only for prime-age men in rural areas, where labor supply is already extremely low (p.20). There is additional reason to be skeptical of applicability of these numbers to the present situation in South Africa, because the survey they use was conducted in 1993, a time of rapid political and economic transition, and the pension had nearly doubled in amount for Africans in the previous three years (p.10).

Dinkelman (2004) confirms Bertrand et al's results using the 1993 PSLSD and 1998 KwaZulu-Natal Income Dynamics Survey (KIDS), which resurveyed the core members of the households from the PSLSD in KwaZulu-Natal, South Africa's largest province, five years later. She finds that male and female pensioners both exert negative impacts on adult male labor supply, while adult female labor supply is decreased by the presence of male pensioners and increased by female pensioners.

Like Bertrand et al, Klasen and Woolard (2005, p.28) find that pension income is associated with lower labor force participation rates, but they find that this effect is due to the perverse incentive created by the endogeneity of household formation: "The unemployed get stuck in rural households in order to get support from pensions and remittances and thereby reduce their search and unemployment prospects." Interestingly, they also find that while labor income is associated with higher reservation wages, pension and remittance income are not. "This provides further confirmation that the linkages between pension and remittance income and search and employment prospects operates via changes in household formation rather than directly via an increase in the reservation wage." (Klasen and Woolard 2005, p.28) They use data from the 1993 PSLSD, 1998 KIDS, and the 1995 October Household Survey (OHS) and 1995 Income and Expenditure Survey (IES).

In her 2004 study, Keller uses an extended selection correction model to allow for the joint determination of household structure and labor market status, and more or less confirms Klasen and Woolard's results, although she also finds that the household formation response induced by the presence of a pensioner operates mainly through a decrease in the likelihood of an individual leaving to set up a new household, rather than

migration into the pensioner's household. She uses cross-sectional data from the 1995 OHS/IES.

Samson et al's 2004 report for the South African Department of Social Development uses data from the September 2000, 2001, and 2002 Labour Force Surveys and the 2000 IES, unlike most other papers on the topic, which almost all take the 1993 PSLSD, 1998 KIDS, or 1995 OHS/IES as their baseline. Not only does this allow Samson et al to examine the impact of the Child Support Grant, which was instituted in 1998, it also means that the data reflect the significant changes undergone by South Africa since the end of apartheid – for example, South Africa's official unemployment rate was 12.7% in 1993, against 29.5% in 2001 (Klasen and Woolard 2005, p.2).[9] They have a variety of findings. While the results from the cross-sectional analyses are not robust and suffer from sample selection and data problems, the panel analysis concludes that in most specifications, all three main grants – CSG, OAP, and DG – have positive and significant impacts on participation and employment. However, bias from selection into grant receipt is still a problem, and so the results do not necessarily imply causality. Furthermore, the take-up rate of the CSG was still very low – only 15.1% of age- and income-eligible children in September 2000, and below 10% in the Eastern Cape, KwaZulu-Natal, and Limpopo, the three provinces that now receive the most CSGs (Samson et al 2004, p.28). While this study's findings are therefore suggestive of a positive role for social grants in determining labor market status, they are not conclusive. A similar study by Samson et al in 2002 documented that unemployed individuals living

[9] The authors note that "the figures are not entirely comparable over time…but they present the correct orders of magnitude."

in wealthier households had higher job search success rates, and interpreted this as potential evidence that lack of resources was a constraint on job search.

Keswell (2004) uses the 1998 and 2003 waves of KIDS to examine the pension's effect on membership in informal insurance associations such as food ROSCAs and community based burial societies, which are often viewed as a means of smoothing consumption, albeit one that involves economic inefficiencies. He

> …demonstrates that individuals living in communities subject to frequent negative shocks, particularly chronic illness, are less likely to join [informal assurance associations] (and thereby avoid the inefficiency) if they enjoy greater income security through access to the social pension of their mothers. Publicly provided social insurance would certainly serve to reduce these sources of inefficiency…The results presented here also suggest that previously documented negative employment elasticities associated with access to pension income should be revisited, with greater attention being paid to the mediating impact of shocks on behaviour. This paper suggests that in the face of shocks, particularly chronic illness in the household, access to pension income may improve employment possibilities by lowering the cost of job search. (p. 26)

Ranchhod (2006) examines the effect of pension receipt on the labor supply of its recipients, the elderly, and finds that there is indeed a discontinuous drop in labor supply when individuals reach pension age. He attributes this to an income and disincentive effect.

Finally, Posel et al (2006) revisit Bertrand et al's analysis of the 1993 PSLSD, but take absent household members (migrant workers) into consideration under the theory that the OAP may finance migration. In contrast to Bertrand et al, Posel et al

> …find no convincing evidence that the social pension creates disincentives for prime-age individuals to migrate to work or to look for work. Rather, where the social pension is significant, which it is in the case of female labour migrants, the effect is positive. Our results also

> suggest that pension income received by women specifically may be important not only because it helps prime-age women overcome income constraints to migration, but also because it makes it possible for grandmothers to support grandchildren. (p.852)

Unfortunately, neither Keswell nor Posel et al's data allow them to examine the impact of the CSG. Nonetheless, their studies add provocative findings to the literature on the labor market impact of social grants, reflecting their central status in reducing poverty in South Africa. Taken together, existing studies present a mixed portrait of social grants and perhaps pose more questions than they supply answers.

3.2 Social impacts

A number of authors have demonstrated the health and nutrition benefits of social grants for children. Duflo (2000) finds substantial improvements in the weight-for-height and height-for-age status of female children living with female pensioners, but little effect for male children, and no effect for male pensioners. Case (2001) finds evidence that pension income is used to upgrade household sanitation facilities, and that individuals living in the 84% of households that pool pension income are likely to be in better health, less likely to experience hunger, and less likely to experience depression. These effects are larger when more than one pensioner is present in the household. Aguëro, Carter, and Woolard (2006, p.26) show that the Child Support Grant has a positive and significant effect on child height-for-age, and estimate that the improved nutrition reflected in these height gains will yield a discounted rate of return of between 160% and 230% on CSG payments.

I am only aware of two studies that address the issue of social grants and education. In his 2004 paper, Edmonds analyzes the impact of the pension on child labor and schooling decisions, and finds that pension eligibility significantly reduces child labor and increases schooling. This effect is particularly strong when the pensioner is male: "Male pension eligibility is associated with an approximately 35 percent decline in hours worked per week and a rise in school attendance to almost 100 percent. These findings imply that because of male pension eligibility 23,000 children are attending school who would otherwise not and over 180 million fewer hours were worked by children in a 1999 [sic]." (p.4) He interprets this as evidence that child labor and school decisions reflect liquidity constraints rather than cost-benefit calculations, and sees an important role for social grants in helping households to overcome these liquidity constraints and make investments in their children's future.

Samson et al (2004, p.64) use a three-stage model to show a correlation between household CSG receipt and increased school attendance, but for reasons of data availability, their model is cross-sectional and based on data from 2000, when CSG take-up was extremely low. As a result, the possibility of bias from simultaneity and unobserved heterogeneity cannot be completely dismissed. They also use pension eligibility as an instrument for pension receipt, and find that it is significantly and positively correlated with children's school attendance.

The positive impact of social grants on schooling is also supported by anecdotal evidence. Lund (2002, p.687) describes the monthly markets that form in poor, under-served rural areas on pension payment days as vibrant sites of economic activity where pensioners pay their funeral policies, school principals come to collect school fees, and

fresh food and clothes are bought and sold. Nceba Mafongosi of the legal aid organization Black Sash recounted how his aunt, a schoolteacher, sees that her pupils who benefit from grants are better fed, clothed, and more likely to have books than their peers after grants have been paid each month (2006).

4. Descriptive analysis: social grants and the labor market

4.1 Data

This paper uses data from the General Household Surveys (GHS) of July 2002, 2003, 2004, and 2005, the Labour Force Surveys (LFS) of September 2004 and March 2005, and administrative data from the national and provincial budgets (National Treasury 2005 and 2007). The GHS is an annual household survey of roughly 100,000 individuals conducted by Statistics South Africa (the government statistics agency) that collects social, economic, and demographic data. The LFS is a semi-annual rotating panel of roughly 100,000 individuals, also conducted by Statistics South Africa, which collects very similar data to the GHS, but with more detailed information on employment and job search. An unfortunate series of coincidences prevents it from being used as a true panel survey for the purpose of studying social grants. It has poor data on social grants, asking only whether anyone in the household received each type of grant, rather than specifying which household member received the grant, and even this question is omitted in over half of the LFS survey periods. It also lacks data on the relationship of household members to each other, except for spouses, which makes it very difficult to guess who a child's primary caregiver is. The GHS, by contrast, has detailed information on social grants and household relationships. Publicly-available administrative data has very little

detail about social grant recipients, but it does report the exact number of beneficiaries of each grant by year.

A number of imputations were necessary due to incomplete and omitted data, but they are not major and are unlikely to have a significant impact on the results. Appendix A details how I made the determination of who a child's primary caregiver is. Appendix B describes the procedure used for imputing labor income for the roughly 25% of employed workers who did not report their income as an exact figure. Appendix C discusses linking individuals between different periods of the LFS to make a panel, and Appendix D discusses imputing the urban/rural distinction in the GHS of 2005.

4.2 CSG recipients and non-recipients

The next two sections undertake a descriptive analysis of Child Support Grant recipients and their interaction with the labor market. I will focus on women who are receiving CSGs for their biological children, as opposed to individuals receiving the grant for their grandchildren, nieces, nephews, or siblings.[10] There are several reasons for this. The sample size of individuals receiving the grant for someone other than their biological children is small and includes immense heterogeneity. It is also less likely that these individuals actually receive and control the grant income, since imputing caregiver status is difficult when there is not a biological parent in the household.

Over 92% of all caregivers receiving CSGs are female, and over 85% of these women are receiving CSGs for their own children (I will refer to this group as CSG

[10] All statistics in Section 4.2 are my calculations from GHS 2005 and are restricted to the African and Coloured populations, which represent 88.11% of the population and nearly all the poor in South Africa.

mothers). Among income-eligible children, nearly 63% living with their mother are receiving CSGs, compared to 35% of children living with their father, 47% of children living with a grandparent, and 36% of children whose caregiver is uncertain (see Appendix A).

Table 4.1 shows the distribution of recipient mothers by age and education, compared to mothers with age-eligible children who do not receive a grant. It also breaks the sample into mothers who are income-eligible and receiving a grant, income-eligible and not receiving a grant, not income-eligible and receiving a grant, and not income-eligible and not receiving a grant. [11]

Table 4.1: Age and education of mothers by CSG status

	N	Mean age	Under 20	20-29	30-39	40-49	50 & above
Receive grant	7015	32.6	3.1%	38.0%	36.5%	18.5%	3.9%
No grant	6337	35.1	5.2%	26.5%	34.5%	25.7%	8.1%

Income-eligible, grant	5467	32.0	3.9%	41.7%	33.5%	16.8%	4.0%
Income-eligible, no grant	3146	33.3	9.9%	35.5%	24.0%	19.4%	11.3%
Not income-eligible, grant	1548	34.9	0.3%	24.9%	47.2%	24.3%	3.2%
Not income-eligible, no grant	3191	36.8	0.5%	17.7%	44.8%	32.0%	5.0%

	N	No education	Primary	Less than matric	Matric	Tertiary
Receive grant	7015	8.7%	28.2%	44.8%	17.9%	0.3%
No grant	6337	9.0%	26.1%	36.0%	26.3%	2.7%

Income-eligible, grant	5467	8.8%	28.2%	45.6%	17.1%	0.3%
Income-eligible, no grant	3146	13.5%	31.9%	38.2%	16.0%	0.5%
Not income-eligible, grant	1548	8.5%	28.2%	42.2%	20.7%	0.5%
Not income-eligible, no grant	3191	4.4%	20.3%	33.8%	36.6%	4.9%

Source: GHS 2005. Percentages sum across.

[11] Income eligibility is based on my calculations using the GHS. A non-trivial number of CSG recipients do not appear to pass the means test – this is likely due to a combination of measurement and reporting error in income, geographic location, and housing type, as well as differences in grant administration, survey timing, and deliberate misreporting of income. In the absence of better data, it is difficult to judge how much of the mismatch between eligibility and receipt is real.

CSG recipient mothers are younger than non-recipient mothers on average, although CS receipt among income-eligible mothers under 20 is very low, which does not disprove but casts doubt on the frequently heard claim that the grant encourages teenage pregnancy.[12] Non-recipient mothers are much more likely to have completed matric[13] or have a tertiary education, although this masks significant inequalities. Recipient mothers have very similar education profiles, regardless of whether they are income-eligible, while income-eligible non-recipient mothers are the least educated group. This might indicate that lack of education is a barrier to grant take-up for some mothers.

Just over 41% of recipient mothers are married or living with their partner, while 5% are widowed, 3% are divorced, and 50% have never married. Although they are less likely to be married than non-recipient mothers, they are significantly more likely to be married and less likely to be widowed than non-recipient mothers whose income is low enough to make them eligible for the grant. Fifty-six percent are located in rural areas, as opposed to 49.8% of all mothers with age-eligible children and 58.6% of CSG-eligible mothers.

Sixty-four percent of recipient mothers receive only one CSG, while 27% receive two, 7% receive three, and the remaining two percent receive between four and six. They are one third more likely to live in households that receive old age pensions than non-recipient mothers (21.7% against 16.2%), and slightly more likely to live in households that receive disability grants (14.3% against 11.1%). Three percent of recipient mothers also receive disability grants, roughly the same proportion as non-recipient mothers. However, among income-eligible mothers, a higher percentage of non-recipients are

[12] On this, see Hassim (2005) and Steele (2006).
[13] The equivalent of an American high school diploma.

disabled and receive DGs than recipients, suggesting perhaps that their disability poses a barrier to securing grants for their children.

Table 4.2 shows the mean composition of household income, in Rand per month per capita, among mothers with age-eligible children, along with household composition and area/housing type.[14]

Table 4.2: Income, household structure, and area type of mothers by CSG status

	N	Household Size	Labor income	Grant income	Total income
Receive grant	7018	6.39	148	110	258
No grant	6340	5.75	652	46	698

Income-eligible, grant	5468	6.57	89	116	205
Income-eligible, no grant	3149	6.45	137	69	207
Not income-eligible, grant	1550	5.74	355	89	444
Not income-eligible, no grant	3191	5.07	1160	23	1184

	N	Household Size	Children under 18	Males 18-64	Females 18-59	Elderly
Receive grant	7018	6.39	3.29	0.98	1.86	0.26
No grant	6340	5.75	2.77	1.06	1.73	0.20

Income-eligible, grant	5468	6.57	3.36	0.97	1.94	0.30
Income-eligible, no grant	3149	6.45	3.14	1.03	1.97	0.30
Not income-eligible, grant	1550	5.74	3.04	1.00	1.54	0.16
Not income-eligible, no grant	3191	5.07	2.39	1.08	1.49	0.11

	N	Rural	Urban formal	Urban informal
Receive grant	7018	55.8%	32.3%	11.9%
No grant	6340	43.0%	46.6%	10.4%

Income-eligible, grant	5468	59.2%	29.4%	11.5%
Income-eligible, no grant	3149	57.6%	30.3%	12.1%
Not income-eligible, grant	1550	44.0%	42.5%	13.5%
Not income-eligible, no grant	3191	48.6%	62.7%	8.7%

Source: GHS 2005. Income figures are in Rand/month.

[14] The General Household Survey only has information on labor income and social grants, not remittances or other forms of wealth. This is a major shortcoming. Nonetheless, these figures are illustrative.

Without conditioning for income eligibility, CSG mothers live in much poorer households than non-recipient mothers, but among income-eligible mothers, recipients and non-recipients have nearly identical household per capita incomes. Not surprisingly, grant recipients have higher levels of grant income, while non-grant recipients have more labor income. Grant recipients also have larger mean household sizes, which are associated with higher levels of poverty. They tend to have more children, adult women, and elderly members than non-recipients and slightly fewer adult males, all of which decrease the household's labor market prospects. Controlling for income-eligibility decreases, but does not eliminate, these differences. Table 4.2 also shows that CSG mothers are concentrated in rural areas, and are slightly more likely to live in informal dwellings in urban areas, which is not surprising.[15]

Table 4.3 on the next page presents a variety of descriptive statistics. Grant recipients consistently show greater signs of poverty than non-recipients: they are less likely to rely on wage or remittance income, less likely to have access to basic services like water, electricity, telephones, and public transport, and tend to have fewer rooms per person in their dwelling. Interestingly, grant recipients tend to live farther from welfare offices than non-recipients, a finding that holds even when controlling for the fact that recipients are more likely to live in rural areas (not shown).

[15] "The regulations which govern grant payments define an informal dwelling as a 'house which is, whether partly or wholly, without brick, concrete, or asbestos walls.' The GHS asks only about the main material used for the walls of the dwelling. We must therefore use this as a proxy and exclude all those children who live in dwellings which have either brick, concrete or asbestos as the main material for the walls when determining which children live in informal dwellings." From Social Assistance Act of 1992 in Budlender et al (2005, p.9).

Table 4.3: Social and economic characteristics of mothers, by CSG status

	Receive grant	No grant	Income-eligible, grant	Income-eligible, no grant	Non-income-eligible, grant	Non-income-eligible, no grant
Main income source						
Wages/salaries	37.6%	63.2%	29.7%	39.4%	65.4%	86.6%
Remittances	13.2%	12.7%	13.5%	18.7%	12.3%	6.7%
Grants	31.7%	18.3%	51.3%	33.3%	17.0%	3.5%
Other	5.5%	5.9%	5.5%	8.6%	5.4%	3.2%
Housing and services						
Formal dwelling	62.6%	71.2%	61.3%	60.0%	67.3%	84.0%
Rooms per person	0.7	0.84	0.7	0.74	0.71	0.94
Electricity from grid	72.3%	79.7%	71.4%	68.7%	75.8%	90.7%
Piped water on site	50.3%	65.3%	47.7%	49.8%	59.4%	80.6%
Sewer-connected toilet	14.9%	33.9%	13.0%	17.0%	21.5%	50.6%
Regular municipal rubbish removal	36.9%	51.6%	34.1%	35.5%	46.3%	67.5%
Access to telephone	59.6%	68.1%	57.3%	56.6%	67.7%	79.5%
Nearby public transportation	72.6%	74.0%	71.9%	69.1%	75.2%	78.8%
Distance to welfare office						
0-14 min	9.8%	18.6%	9.2%	11.4%	11.9%	25.6%
15-29 min	29.5%	31.8%	28.8%	26.5%	31.9%	37.0%
30-44 min	28.0%	25.9%	27.9%	28.7%	28.5%	23.1%
45-59 min	12.4%	9.6%	12.9%	12.1%	10.7%	7.1%
60 min or more	20.3%	14.2%	21.2%	21.3%	17.1%	7.2%
Hunger and schooling						
Adult hunger	26.8%	17.2%	29.0%	26.9%	19.3%	7.7%
Child hunger	25.6%	16.3%	27.7%	25.6%	18.1%	7.2%
Household school attendance rate	96.3%	95.3%	96.0%	92.9%	97.4%	97.7%
Expenditure						
Monthly household per capita expenditure (mean)	R 145	R 333	R 126	R 148	R 215	R 517
Food share	59.8%	53.0%	61.8%	62.1%	53.0%	44.2%
Transport share	11.7%	13.9%	10.8%	11.6%	14.7%	16.1%
Housing share	5.8%	9.7%	5.5%	5.5%	7.2%	13.8%
Clothing share	8.9%	10.3%	8.2%	8.0%	11.2%	12.6%
Other share	13.8%	13.1%	13.7%	12.8%	13.9%	13.3%
Labor market						
Household narrow unemp. rate	41.4%	25.0%	49.0%	40.7%	18.9%	12.9%
Household broad unemp. rate	60.6%	40.4%	68.2%	60.4%	34.5%	22.3%
PSU narrow unemp. rate	38.3%	30.7%	39.6%	35.7%	33.7%	25.9%
PSU broad unemp. rate	55.7%	44.6%	57.6%	52.5%	48.9%	36.7%

Source: GHS 2005. Hunger figures are the percent of households reporting hunger in the past year due to lack of food. School attendance refers to children aged 7-17. PSU stands for Primary Sampling Unit, and is the smallest geographic unit reported by Statistics South Africa.

Grant recipients also have higher rates of adult and child hunger and are more likely to have had children leave the household to live in the streets, and devote a larger share of their income to food. However, they have much lower per capita expenditures, even when controlling for income eligibility, which is likely to explain much of their underperformance on the aforementioned social indicators. They also live in households and communities with higher unemployment rates.

These statistics support the idea that CSG recipients tend to be poor and live in marginalized areas. However, CSG-recipient mothers who appear not to be income-eligible do better on many indicators than the income-eligible non-recipients, meaning that there is a large group of poor mothers who are not receiving CSGs. While this could be measurement error, reporting error, or the result of incomplete information in the survey instrument, it is also a reminder that there are still important barriers to take-up among segments of the poor.

Table 4.4 presents a snapshot of the labor market status of CSG recipients compared to non-recipients. Before controlling for eligibility, recipient and non-recipient mothers are roughly equally likely to be in the labor force, although non-recipients are more likely to be employed. However, conditional on eligibility, recipient mothers are much more likely than non-recipient mothers to be in the labor force, more likely to be actively searching given that they are in the labor force, and nearly as likely to be employed. Care should be taken in making these comparisons, however, as the eligible non-recipients are a very heterogeneous group, including some mothers who are so disadvantaged that they do not receive a CSG and some who are relatively well-off but appear poor because of incomplete data or the particular structure of the means test. The

outcomes of recipients' spouses are not as strong relative to those of non-recipients, although the situation is similar.

Table 4.4: Labor force status of mothers and their spouses, by CSG status

	Mothers				Mothers' spouses			
	Out of LF	Unemp., no search	Unemp., searching	Employed	Out of LF	Unemp., no search	Unemp., searching	Employed
Receive grant	26.3%	25.4%	25.1%	23.2%	14.4%	9.3%	18.6%	57.7%
No grant	26.9%	16.7%	15.4%	41.0%	9.0%	3.7%	6.6%	80.7%

Income-eligible, grant	26.8%	28.1%	28.5%	16.7%	22.5%	14.3%	27.3%	25.9%
Income-eligible, no grant	35.3%	24.4%	21.5%	18.7%	28.4%	10.3%	16.8%	44.5%
Not income-eligible, grant	24.7%	15.9%	13.2%	46.1%	1.1%	1.1%	4.2%	93.7%
Not income-eligible, no grant	18.6%	9.2%	9.2%	63.1%	1.7%	1.2%	2.7%	94.4%

Source: GHS 2005. Percentages sum across within each group.

The high employment rates of husbands are striking, although they clearly hide significant inequalities. Whereas over 90% of husbands who are not income-eligible for the CSG are employed, very few husbands of income-eligible grant recipients are employed, and many of the income-eligible husbands are out of the labor force altogether. Income-eligible individuals are poor by definition so they would be expected to have lower employment and participation rates, but the contrast between the two groups is stark.

4.3 Labor force transition matrices

Although these statistics are useful, it would be wrong to draw conclusions about the CSG's impacts from them, since they only present a snapshot view of the labor market and mask significant heterogeneity and possible bias. It may be more revealing to compare the labor force transition patterns of recipients and non-recipients. I do this by

examining changes in labor force status across time by using the Labour Force Survey, which tracks individuals across multiple periods.[16] However, comparisons based on these transition matrices are not sufficient to prove causality, as there remain many potential sources of bias. Personal characteristics, household composition, selection into grant receipt, geographic location, the endogeneity of household formation, and other unobserved heterogeneity could all bias the results, and the expected direction of bias is ambiguous. Nonetheless, the comparison is useful in a descriptive sense.

As discussed in Section 4.1, the LFS does not have individual-level data on social grants, so instead of comparing recipient and non-recipient individuals, I will compare individuals living in households that receive at least one CSG with individuals living in households that do not receive a CSG.[17] Table 4.5 below is a transition matrix that shows the labor force status of African and Coloured females aged 15-59 and not attending school, in September 2004 (rows) and March (2005) columns. The diagonal represents individuals whose labor market state did not change, and outcomes above the diagonal indicate increased intensity of engagement with the labor market. The top matrix represents women who live in households that received at least one CSG, while the bottom matrix represents those who live in non-recipient households. So among CSG-

[16] All statistics and tables in the remainder of this section are from the joint LFS September 2004 – March 2005 panel. The results are qualitatively similar using September 2003 – March 2004, the only other time period in which a useful panel can be constructed.

[17] The LFS has social grant data for September 2004 but not March 2005, so throughout this section I refer to households that received CSGs in September 2004 as CSG-recipient households, although some of these households may not receive a CSG in March 2005 and some households may not have received as CSG in September 2004 but do receive one in March 2005. The LFS does not ask how long the household has been receiving the CSG. Some households may have been receiving it since the grant's inception in 1998, others for only a month.

recipient households, 54.1% of women who were out of the labor force in September were still out in March, while 20% transitioned into non-searching unemployment, 11.7% moved into searching unemployment, and 14.1% became employed.

Table 4.5: Transition matrix, African and Coloured females 15-59

CSG households March 2005

N=5567

		Out of LF	Unemp., no search	Unemp., searching	Employed	Total
Sept. 2004	Out of LF	54.1%	20.0%	11.7%	14.1%	100.0%
	Unemp., no search	17.8%	43.0%	24.7%	14.5%	100.0%
	Unemp., searching	15.1%	26.5%	39.5%	18.9%	100.0%
	Employed	10.6%	12.3%	10.0%	67.2%	100.0%

Non-CSG households March 2005

N=8169

		Out of LF	Unemp., no search	Unemp., searching	Employed	Total
Sept. 2004	Out of LF	63.8%	13.6%	9.3%	13.3%	100.0%
	Unemp., no search	23.8%	35.4%	25.7%	15.1%	100.0%
	Unemp., searching	16.2%	21.9%	39.9%	22.1%	100.0%
	Employed	7.8%	6.3%	6.9%	79.0%	100.0%

Source: LFS 2004 September and 2005 March. Percentages sum across.

Women in non-CSG households are more likely to stay in employment, and to move from unemployment into employment, while women in CSG households are more likely to move from being out of the labor force into participation and employment. A transition matrix for men (not shown) has similar results, although with higher overall participation and employment.

Table 4.6 attempts to reduce heterogeneity across the comparison by examining only households in the bottom two quintiles of per capita income (excluding CSG

income).[18] This comparison is more favorable for women in CSG households, who appear to outperform their counterparts in every respect but staying in employment and moving from searching unemployment into employment.

Table 4.6: Transition matrix, African and Coloured females 15-59, bottom two quintiles

CSG households March 2005

N=3505

		Out of LF	Unemp., no search	Unemp., searching	Employed	Total
Sept. 2004	Out of LF	52.2%	21.3%	11.8 %	14.7%	100.0%
	Unemp., no search	18.8%	42.4%	24.3%	14.5%	100.0%
	Unemp., searching	14.9%	27.5%	37.6%	20.0%	100.0%
	Employed	15.5%	15.7%	9.8%	59.1%	100.0%

Non-CSG households March 2005

N=2792

		Out of LF	Unemp., no search	Unemp., searching	Employed	Total
Sept. 2004	Out of LF	60.4%	15.6%	10.5%	13.6%	100.0%
	Unemp., no search	24.9%	38.6%	23.8%	12.7%	100.0%
	Unemp., searching	16.1%	24.3%	38.3%	21.3%	100.0%
	Employed	15.0%	10.6%	10.0%	64.4%	100.0%

Source: LFS 2004 September and 2005 March. Percentages sum across.

However, the group of females in non-CSG households in the bottom two quintiles comprises an unknown mix of very disadvantaged individuals who do not, for whatever reason, collect a grant for which they seem to be eligible, and better-off individuals whose income is not captured by the survey, so direct comparisons to CSG households may be misleading.

In Table 4.7, I attempt to control for more of this heterogeneity by restricting the sample to households with at least one age-eligible child (under 11 years old at that time). The results are similar, although women in CSG households now appear to outperform

[18] Because of the way the LFS reports income, some imputations were necessary to construct the income figures. See Appendix B for details.

their counterparts in everything but staying in employment. Selection bias may still be a problem, however.

Table 4.7: Transition matrix, African and Coloured females 15-59, bottom two quintiles, at least one age-eligible child in household

CSG households N=3462

		March 2005				
		Out of LF	Unemp., no search	Unemp., searching	Employed	Total
Sept. 2004	Out of LF	52.4%	21.1%	11.8%	14.7%	100.0%
	Unemp., no search	19.0%	42.5%	24.0%	14.5%	100.0%
	Unemp., searching	15.0%	27.7%	37.5%	19.9%	100.0%
	Employed	15.7%	15.8%	9.7%	58.8%	100.0%

Non-CSG households N=1795

		March 2005				
		Out of LF	Unemp., no search	Unemp., searching	Employed	Total
Sept. 2004	Out of LF	59.7%	17.7%	10.0%	12.6%	100.0%
	Unemp., no search	25.7%	39.0%	23.4%	11.9%	100.0%
	Unemp., searching	15.2%	23.6%	42.4%	18.8%	100.0%
	Employed	14.0%	11.8%	10.6%	63.7%	100.0%

Source: LFS 2004 September and 2005 March. Percentages sum across.

Another potential source of bias relates to the simultaneity of labor force decision-making within households. One household member may begin searching for a job because another has lost her job, or one spouse may withdraw from the labor market when the other gets a job. The sample in Table 4.8 is limited to households that had no one employed in September, in order to minimize the potential for simultaneity bias. Once again, women in CSG households appear to do better, although this result is still subject to various forms of bias. The results are qualitatively the same if conditioned on geographic location (metropolitan/non-metropolitan, not reported).

Table 4.8: Transition matrix, African and Coloured females 15-59, bottom two quintiles, at least one age-eligible child in household, no one in household employed in Sept. 2004

CSG households N=3462

		March 2005				
		Out of LF	Unemp., no search	Unemp., searching	Employed	Total
Sept. 2004	Out of LF	54.6%	20.3%	11.5%	13.6%	100.0%
	Unemp., no search	19.9%	41.8%	23.5%	14.9%	100.0%
	Unemp., searching	16.5%	27.6%	37.2%	18.6%	100.0%

Non-CSG households N=1795

		March 2005				
		Out of LF	Unemp., no search	Unemp., searching	Employed	Total
Sept. 2004	Out of LF	63.2%	14.3%	10.6%	11.9%	100.0%
	Unemp., no search	26.1%	37.3%	25.1%	11.5%	100.0%
	Unemp., searching	16.3%	25.9%	41.1%	16.7%	100.0%

Source: LFS 2004 September and 2005 March. Percentages sum across. The fourth row disappears because these households contain no employed individuals in September 2004, by definition.

These transition matrices certainly do not provide any support for the notion that the CSG has a dramatic negative effect on labor market performance, but cannot say with any degree of certainty if they have a positive effect.[19] The next section attempts to examine this relationship more rigorously, by eliminating bias from simultaneity and unobserved heterogeneity.

[19] I also ran a series of multivariate regressions with these labor market transitions as the dependent variable, controlling for observable heterogeneity with a standard array of personal, social, geographic, economic, and household variables. The results were qualitatively similar to those in the transition matrices and they suffer from the same problems of selection bias and unobserved heterogeneity as the transition matrices, so I have omitted them for brevity.

5. Pooled sample analysis – labor market

5.1 Methodology

In order to try to establish a more convincing causal relationship between social grant receipt and labor market outcomes, I exploit the natural experiment created by the government's expansion of the age limit at which a child loses CSG eligibility and the arguably exogenously generated increase in grant take-up since 2002.

In 2002, children under 7 were eligible to receive the grant, subject to their caregiver passing the means test. The CSG was then extended to children under nine, eleven, and fourteen years of age in April of 2003, 2004, and 2005, respectively. So in 2002, caregivers of children aged 7-8 would not have received CSGs for those children, whereas in 2003-2005 they would have been eligible to do so. Table 5.1 below shows the expansion of grant take-up as the age eligibility limit was increased. Data from 2002 had to be imputed from administrative data and GHS 2003 because the 2002 survey data lacked the necessary social grant information. Grant take-up has expanded remarkably in the survey period, among newly eligible age groups (shaded) and previously eligible age groups alike. Take-up is lower among very young children and older children. The CSG can make the biggest impact on the nutrition of the very young, so this group must be a focus of the government's efforts to increase take-up, with outreach through clinics, schools, and other social services.

The changes in age eligibility allow me to estimate an employment model for a mother that controls for the ages of her children and uses an interaction term to identify the effect of having an 8-year-old in 2002 compared to having an 8-year-old in 2003 (for example).

Table 5.1: CSG take-up by age and year, African and Coloured children living with their mothers

	Year			
Age	2002*	2003	2004	2005
0	11.9%	14.2%	23.5%	25.3%
1	30.0%	35.8%	47.3%	53.8%
2	33.1%	39.5%	54.9%	60.7%
3	35.1%	41.9%	56.1%	64.4%
4	33.4%	39.9%	56.3%	60.8%
5	36.6%	43.6%	55.8%	62.5%
6	28.4%	33.9%	54.8%	63.2%
7	0.0%	23.4%	50.4%	56.6%
8	0.0%	6.6%	42.9%	56.8%
9	0.0%	3.0%	29.6%	51.6%
10	0.0%	1.0%	10.5%	44.4%
11	0.0%	0.9%	4.0%	29.2%
12	0.0%	1.1%	0.8%	12.8%
13	0.0%	0.5%	1.1%	5.8%

Source: GHS 2002-2005 and National Treasury (2005). 2002 figures are imputed from GHS 2003 and National Treasury (2005) based on CSG growth and 2003 age patterns for 0-6 year-olds, and are assumed to be zero for 7-13 year-olds. Cells above the line represent eligibility, shaded cells represent age-eligibility expansions. Non-zero take-up for ineligible cohorts is likely due to reporting error.

The identifying assumption for this test is that the only factor influencing a mother's employment status that has changed between 2002 and 2003 and is correlated with the age of her child is the age limit for the CSG. This seems to be a reasonable assumption: while there are many reasons why one might expect the unemployment rate of mothers to vary with the age of their children, it is hard to construct a plausible argument as to why this relationship might change in the space of a few years, other than the fact that many of these women began receiving CSGs during this time.

This method's advantage is that it eliminates the problems of unobserved heterogeneity and selection bias because it is based on eligibility rather than actual receipt, and because the variation in age-eligibility was imposed by the government and is therefore exogenous to individual outcomes. It is theoretically possible that individuals could have anticipated the changes and adjusted their behavior accordingly, since the

government announced the entire series of expansions in 2001, but the severe liquidity constraints experienced by the grant's target population makes this possibility implausible. This means that only time-variant factors that are correlated with both the age of a mother's child and her employment status would influence the results. In the absence of such factors, this method can establish the true causal relationship between CSG receipt and labor market activity.

I argue that this is a plausible assumption. If there were some hidden factor biasing the results, it would have to be a force that changed the nature of the relationship between motherhood and employment over the course of the four years of the study period. The only obvious change that might fit this description is the continuing expansion of the HIV/AIDS epidemic. South Africa's HIV rate among antenatal clinic attendees increased from 26.5% in 2002 to 30.2% in 2005, an increase of 14% (Department of Health 2006, p.10). While this increase has undoubtedly had effects on the labor market, these effects would have to be correlated with the age of a mother's child in order to bias this study's results, since the aggregate year-to-year change will show up in the year fixed effect variables. Even if the effect of the increase in the infection rate is biasing, the direction of the bias is ambiguous. An HIV-positive mother or a mother with an HIV-positive child might be less able to work by virtue of her own health or the increased burden of caring for other sick household members. At the same time, medical costs might force such women into the labor force out of desperation, which would bias the results in the opposite direction. While it is not possible to dismiss HIV/AIDS as a potential source of bias, certain features of the results suggest that it is

not the dominant effect. I will elaborate on this issue in Sections 5.2 and 6.2, but set it aside for now.

At the same time as I examine the effect of the age-eligibility expansion, I also examine the effect of an increase in grant take-up among the mothers of children under seven, who have long been eligible for the CSG. This increase in take-up was driven by the government's decision to make grant roll-out a priority and improve grant registration and administration.

> President Thabo Mbeki's unequivocal commitment sent a clear message to the bureaucracy that social grants provided the central pillar for the poverty eradication strategy. In the 2002 State-of-the-Nation Address, he announced a government-led campaign to "register all who are eligible for the child grant", and in 2003 reinforced his support for the ongoing effort by publicly thanking all those "who had rolled up their sleeves to lend a hand in the national effort to build a better life for all South Africans", citing first "the campaign to register people for social grants". The system has also benefited from a Social Development Minister, Dr. Zola Skweyiya, who has effectively championed the effective implementation an extension of social grants within the Cabinet. (Samson et al 2006, p. 3)

Providing further evidence that the increase in grant take-up was driven by an exogenous government decision and not by a decrease in incomes, the number of mothers with incomes below the means test threshold remained nearly unchanged throughout the survey period, with only small fluctuations.

As Table 5.1 above shows, CSG take-up has increased by 15 to 35 percentage points between 2002 and 2005, depending on the child's age. That means that the mother of a five-year-old, for example, had a much greater chance of receiving a CSG in 2005 than in 2002, and so we would expect that grant eligibility would have a significantly larger impact on labor market outcomes. An interaction term between the number of

children under seven years of age and the year can capture this variation. The idea is the same as with the age-eligibility expansion, which can be thought of as an expansion in take-up from zero, whereas this method captures the effect of a take-up expansion from roughly 30% to 60%. To be clear, though, these interaction terms measure the effect of having a child of a certain age in a certain year, relative to the effect of having a child of the same age in 2002. They do not directly measure the effect of receiving a CSG. As discussed above, the identifying assumption that I make throughout this paper is that the only factor that is correlated with time and with having a child of a certain age is the increased probability of receiving a CSG. Under this assumption, the coefficients on the interaction terms measure the effect of the increased probability of receiving a CSG.

Unfortunately, the LFS does not have good data on relationships among household members, making it impossible to link children to their caregivers with any degree of certainty. I therefore turn to the General Household Survey, which has annual coverage from 2002 to 2005. I pool the four GHS data sets to create one master data set with roughly 400,000 person-year observations. Controlling for year and province fixed effects, household demographic variables, and individual characteristics such as education, I estimate the impact of the age of a child on a caregiver's labor force outcome using interaction terms between the year of the observation and the age of a child as an exogenous source of variation in CSG receipt. These regressions are on a series of four pooled cross sections, not on a panel, so the coefficients on the CSG interaction terms measure relative changes rather than actual transitions.

Equation (1) is a form of the regression equation that has been simplified for clarity to include only mothers of seven and eight year olds in the years 2002 and 2003.

$$(1) \qquad y_i = \beta_0 + \beta_1(children7\text{-}8)_i + \beta_2(year2003) + \beta_3((children7\text{-}8)_i * (year2003)) + \varepsilon_i$$

In this equation, y is a labor force outcome variable; *children7-8* equals the number of children aged seven or eight for whom the individual is the caregiver; *year2003* is a dummy that equals 1 if the year of the observation is 2003 and 0 otherwise; β_0, β_1, β_2, and β_3 are parameters to be estimated; and ε_1 is an error term. The coefficient on the interaction term, β_3, represents the effect of having a seven- or eight-year-old child in 2003, when he/she was CSG-eligible, relative to 2002, when he/she was not.

Equation (2) below represents the full model in its general form, which will be estimated in Section 5.2.

$$(2) \qquad y_{kitp} = \beta_0 + \beta_1 C_{itp} + v_t + \beta_2(C_{itp} * v_t) + \beta_3 H_{itp} + \beta_4 X_{itp} + \gamma_p + \varepsilon_{itp}, \qquad k = b, n, e$$

In equation (2), y_{kitp} refers to a dummy variable for labor force status k (broad participation (b), narrow participation (n), or employed (e)) for individual i in year t in province p; C_{itp} is a vector of variables that each contain the number of children in a certain age category (0-3, 4-6, 7-8, 9-10, 11-13, 14-17) for whom the individual is the caregiver; v_t represents year fixed effects; $C_{itp} * v_t$ represents the twelve interactions of the children age category variables with years in which there were either increases in take-up for previously eligible age groups or increases in the age eligibility limit[20]; H_{itp} is

[20] The child age-year interactions are: *children 0-3 * 2003, children 0-3 * 2004, children 0-3 * 2005, children 4-6 * 2003, children 4-6 * 2004, children 4-6 * 2005, children 7-8 **

a vector of household composition characteristics, including the number of children in the household for whom the individual is not the caregiver; X_{itp} is a vector of personal characteristics; γ_p represents province fixed effects; β_0 through β_4 are vectors of parameters to be estimated; and ε_{itp} is an error term. Once again, the vector β_2 contains the coefficients of interest.

The dependent variable *y* is a labor force outcome. Conceptually, the GHS allows us to put working-age individuals into one of four categories: out of the labor force; participation in the broad labor force, defined by willingness to accept a job if a suitable one were available; participation in the narrow (or strict) labor force, defined by active job search; and employment.

Figure 5.1: Labor force status conceptual framework

<table>
<tr><td colspan="4">The population</td></tr>
<tr><td colspan="2">Below minimum working age</td><td colspan="2">Above minimum working age</td></tr>
<tr><td colspan="2" rowspan="4">Non-labour-force participants (residual category consisting of those who believe they are too old to work, children too young to work, people choosing not to work or incapable of it, home-makers etc.)</td><td colspan="2">Broad labour force (broad)</td></tr>
<tr><td>Unemployed (broad)</td><td>Employed</td></tr>
<tr><td rowspan="2">Not actively seeking work</td><td>Narrow labour force (narrow)</td></tr>
<tr><td>Unemployed (narrow)</td><td>Employed</td></tr>
</table>

Adapted from Nattrass (2002). Bold text corresponds to dependent variables used in this paper.

The dependent variables used in these regressions are dummy variables that correspond to membership in the broad labor force (*broad*), narrow labor force (*narrow*), and employment (*employed*). The variable *broad* is equal to one for all individuals who are unemployed by the broad or narrow definitions, as well as the employed. *narrow* equals

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*2003, children 7-8 * 2004, children 7-8 * 2005, children 9-10 * 2004, children 9-10 * 2005, and children 11-13 * 2005.*

one only for workers who are unemployed by the strict definition (actively seeking a job or employed, while *employed* includes only the currently employed. Because the GHS does not have extensive labor market information, this study focuses on the extensive margin of labor supply (whether the individual participates or is employed) as opposed to the intensive margin (how many hours the individual works).

Table 5.2 below describes the dynamics of the labor market over time for the sample of African and Coloured mothers in the GHS. The first two rows are the standard measures of the unemployment rate, while the latter three correspond to the dependent variables used in this study. The unemployment rates of this group are higher than for the rest of the population, but this is largely due to their higher labor force participation. These figures stay remarkably stable over time, with only a slight decrease in participation and employment rates between 2002 and 2005. As long as this small change is uncorrelated with the age of a mother's children, it will be picked up by the year fixed effect variables and will not affect the results.

Table 5.2: Labor market status of African and Coloured mothers by year

	Year			
	2002	2003	2004	2005
Narrow unemployment rate	38.3%	38.0%	39.2%	38.1%
Broad unemployment rate	52.5%	55.3%	57.0%	55.6%

Broad participation rate (*broad)*	74.5%	74.4%	74.3%	73.9%
Narrow participation rate (*narrow*)	57.3%	53.6%	52.5%	52.9%
Employment rate (*employed*)	35.4%	33.3%	32.0%	32.8%

Source: GHS 2002-2005

The approach I take to defining dependent variables entails a significant loss of information – an individual with a value of 1 for *broad* could be in any of three distinct labor market states, and an individual with a value of 0 for *employed* could be actively

looking for a job or not want a job. Since these dependent variables are binary, I use a probit model, although using OLS does not change the results substantially.

Another approach would be to predict *broad*, then predict *narrow* conditional on *broad*, and finally predict *employed* conditional on *narrow*. This would retain more information and present more limited comparisons, but may yield biased results due to selection into labor force participation. One could control for this using a multi-stage regression technique such as Heckman estimation that includes probability of membership in the broad labor force as an explanatory variable in the regression on narrow labor force participation (for example). However, for identification it would be necessary to find a variable that affected broad labor force participation but not narrow labor force participation, and in practice, this is rarely possible. My dependent variable selection thus prioritizes getting unbiased estimates over detailed information about labor force dynamics.

I repeat this analysis for different groups of individuals, such as those with low education or living in rural areas. Since the vast majority of caregivers are mothers, and the effects of CSGs are likely to differ between mothers, fathers, and grandparents, I will restrict my analysis to mothers, and then examine their spouses.

5.2 Results

Mothers

Columns 1, 2, and 3 of Table 5.3 are regressions on broad labor force participation, narrow labor force participation, and employment, respectively, without the interaction terms. As with subsequent regressions, the sample consists of African and

Coloured women who are the mother of at least one child in the household.[21] The variables *Own children 0-3*, *Own children 4-6*, etc., are equal to the number of children in the given age range for whom the individual in question is the caregiver, and the variables *Others' children 0-3*, *Others' children 4-6*, etc., contain the number of children in the given age range that reside in the household but for whom the individual is not the primary caregiver.[22] Other variables include the number of adult females and male and female elderly in the household (number of adult males is the omitted category), the household size, a dummy for disability, and dummy variables for different levels of educational attainment, as well as year and province fixed effects (not reported).

The signs of the coefficients are more or less consistent with what one would expect. Interestingly, a mother's own children generally have a negative impact on her participation, while older children have a positive impact on her employment. Having others' children in the household is associated with increased participation and employment, possibly because this implies that there is a second mother in the house, and having a second mother in the household may free the first mother from enough domestic duties to seek employment.

Columns 4-6 repeat these regressions with the addition of the twelve interaction variables. The first six variables are intended to capture the impact of increased CSG take-up, while the latter six represent the impact of the age eligibility expansion.

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[21] As in Section 4, I focus on African and Coloured women because they make up 89.4% of the mothers in the country and nearly all the poor. Within this group, 88.9% are African.

[22] In addition to her own children, a mother in the sample may be the primary caregiver of children in the household who are not hers, although such cases are very much the exception. See Appendix A for details of how I assign primary caregivers.

Table 5.3: Labor Force Status of African and Coloured Mothers, Basic Model

	(1)	(2)	(3)	(4)	(5)	(6)
	broad	*narrow*	*employed*	*broad*	*narrow*	*employed*
OwnChildren0-3 * 2003				0.021	0.005	-0.008
				(2.27)**	(0.46)	(0.79)
OwnChildren0-3 * 2004				0.020	-0.003	0.001
				(2.13)**	(0.24)	(0.08)
OwnChildren0-3 * 2005				0.031	0.010	-0.002
				(3.42)***	(0.97)	(0.20)
OwnChildren4-6 * 2003				0.013	-0.018	-0.010
				(1.24)	(1.45)	(0.88)
OwnChildren4-6 * 2004				0.035	0.002	0.003
				(3.30)***	(0.18)	(0.24)
OwnChildren4-6 * 2005				0.013	-0.009	0.000
				(1.27)	(0.77)	(0.01)
OwnChildren7-8 * 2003				0.005	0.002	0.002
				(0.43)	(0.12)	(0.18)
OwnChildren7-8 * 2004				0.028	0.020	0.008
				(2.21)**	(1.43)	(0.63)
OwnChildren7-8 * 2005				-0.006	-0.020	-0.018
				(0.51)	(1.41)	(1.41)
OwnChildren9-10 * 2004				0.007	0.005	0.008
				(0.69)	(0.44)	(0.69)
OwnChildren9-10 * 2005				0.013	-0.009	-0.003
				(1.30)	(0.76)	(0.27)
OwnChildren11-13 * 2005				0.007	-0.005	0.003
				(0.97)	(0.59)	(0.39)
Own Children 0-3	-0.045	-0.076	-0.050	-0.064	-0.080	-0.047
	(10.79)***	(15.52)***	(10.56)***	(9.07)***	(9.63)***	(5.99)***
Own Children 4-6	-0.002	-0.020	-0.005	-0.016	-0.013	-0.003
	(0.37)	(3.97)***	(1.11)	(2.14)**	(1.54)	(0.42)
Own Children 7-8	0.001	-0.009	0.004	-0.005	-0.009	0.007
	(0.19)	(1.69)*	(0.87)	(0.57)	(0.90)	(0.72)
Own Children 9-10	-0.002	-0.010	0.003	-0.007	-0.009	0.002
	(0.42)	(1.86)*	(0.52)	(1.15)	(1.22)	(0.25)
Own Children 11-13	-0.010	-0.011	0.014	-0.012	-0.010	0.014
	(2.50)**	(2.29)**	(3.28)***	(2.70)***	(1.79)*	(2.77)***
Own Children 14-17	-0.014	-0.010	0.018	-0.014	-0.010	0.018
	(3.57)***	(2.10)**	(4.31)***	(3.55)***	(2.11)**	(4.32)***
Others' Children 0-3	0.015	0.004	0.020	0.015	0.004	0.020
	(3.48)***	(0.73)	(3.85)***	(3.45)***	(0.73)	(3.85)***
Others' Children 4-6	0.012	0.000	0.029	0.012	0.000	0.029
	(2.26)**	(0.06)	(4.78)***	(2.23)**	(0.06)	(4.78)***
Others' Children 7-8	0.020	-0.004	0.010	0.020	-0.004	0.010
	(3.17)***	(0.57)	(1.33)	(3.19)***	(0.57)	(1.32)
Others' Children 9-10	0.017	0.014	0.020	0.017	0.014	0.020
	(2.74)***	(1.86)*	(2.58)***	(2.73)***	(1.86)*	(2.57)**
Others' Children 11-13	0.014	0.010	0.008	0.014	0.010	0.008
	(2.61)***	(1.55)	(1.30)	(2.62)***	(1.55)	(1.28)
Others' Children 14-17	0.002	0.002	-0.002	0.002	0.002	-0.002
	(0.41)	(0.38)	(0.27)	(0.43)	(0.38)	(0.27)
Household females 18-59	0.034	0.028	0.016	0.034	0.028	0.016
	(10.72)***	(7.63)***	(4.58)***	(10.70)***	(7.64)***	(4.58)***
Household males 65 & over	-0.033	-0.061	-0.055	-0.033	-0.061	-0.054
	(4.31)***	(6.57)***	(5.92)***	(4.31)***	(6.57)***	(5.91)***
Household females 60 & over	0.024	-0.018	-0.046	0.024	-0.018	-0.046
	(4.47)***	(2.91)***	(7.34)***	(4.47)***	(2.92)***	(7.34)***
Household size	-0.016	-0.017	-0.027	-0.016	-0.017	-0.027
	(8.31)***	(7.33)***	(12.36)***	(8.29)***	(7.32)***	(12.36)***
Age	0.053	0.063	0.062	0.053	0.063	0.062
	(41.30)***	(40.52)***	(40.03)***	(41.36)***	(40.53)***	(40.03)***
Age-squared	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001
	(45.01)***	(39.83)***	(35.25)***	(45.07)***	(39.84)***	(35.26)***
Disabled	-0.463	-0.340	-0.217	-0.464	-0.340	-0.217
	(34.62)***	(24.85)***	(18.98)***	(34.68)***	(24.88)***	(18.99)***
Primary	0.060	0.082	0.061	0.060	0.082	0.061
	(9.14)***	(9.82)***	(7.54)***	(9.12)***	(9.78)***	(7.52)***
Less than matric	0.105	0.154	0.113	0.104	0.154	0.113
	(15.42)***	(18.24)***	(13.97)***	(15.37)***	(18.17)***	(13.95)***
Matric	0.197	0.290	0.260	0.197	0.290	0.259
	(29.06)***	(32.88)***	(27.77)***	(29.02)***	(32.81)***	(27.74)***
Tertiary	0.177	0.337	0.417	0.176	0.337	0.417
	(12.80)***	(19.63)***	(21.88)***	(12.78)***	(19.62)***	(21.88)***
Observations	55460	55460	55460	55460	55460	55460
Pseudo R-squared	0.13	0.10	0.12	0.13	0.10	0.12
Joint significance of interaction terms: Prob > F				0.00		

Absolute value of z statistics in parentheses * significant at 10%; ** significant at 5%; *** significant at 1%

Source: GHS 2002-2005. Marginal effects reported. Year and province fixed effects not reported.

As previously discussed, these two effects amount to the same thing: the increased chance of receiving a Child Support Grant given being the caregiver of a child of a certain age. The variable *OwnChildren7-8 * 2003*, for instance, is equal to the number of children aged seven to eight for whom the woman is the caregiver times a dummy that equals one if the observation is from 2003, zero otherwise. Its coefficient represents the effect of having a seven- or eight-year-old child in 2003, relative to the effect of having a seven- or eight-year-old child in 2002, when the child would not have been eligible for a CSG. The variable *OwnChildren0-3 * 2004* is similarly constructed, and its coefficient represents the effect of having a zero- to three-year-old in 2004 relative to 2002, when the take-up rate was much lower. Under the assumption that the changes in age eligibility and increased take-up are the only time-variant factors that are correlated with age of children and that impact labor market status, these coefficients represent the true impact of the CSG on labor market outcomes.

The coefficients on the interaction terms for broad participation are nearly all positive and, in five cases, statistically significant, whereas for narrow participation and employment they have mixed signs but are not statistically significant. The variables proxying for increased take-up are particularly strong for broad participation, and a likelihood ratio test for joint significance of the twelve interaction terms shows that they are significant at the one percent level.[23] The results are nearly identical when estimated using OLS instead of a probit model.

[23] I did joint significance tests for narrow participation and employed, but with the exception of Columns 5-6 in Table 5.6, they never approach significance, so I do not report them here. In order to gain additional statistical power, I also tried aggregating the interaction variables in various ways, such as combining all the terms for 7-8, 9-10, and 11-13 year-olds across the different years, or condensing these terms into one variable

Interpreting the coefficients and calculating the magnitude of these effects is difficult. The marginal effect of *Children4-6 * 2003* in Column 4, for example, is 0.013. The literal interpretation of this is that having a child aged 4-6 in 2003 is correlated with a 1.3 percentage point increase in broad labor force participation, relative to the effect of having a child aged 4-6 in 2002. The regression does not say anything directly about the effect of social grants, but the model's identifying assumption is that the only thing correlated with having a 4-6 year-old and with broad participation that changes between 2002 and 2003 is the increased likelihood of receiving a CSG for the child, due to the increase in take-up that I argue is driven by increased government efforts, and is therefore exogenous. Under these assumptions, the coefficient is the effect of the increased probability of receiving a CSG, so dividing by the change in the age-specific take-up rate from 2002 to 2003 gives the effect of actually receiving the grant. However, there are twelve coefficients from which to choose and no clear theoretical reason to choose one above the others. For Column 4 above, the calculated effects range from a 1.1 percentage point decrease in broad participation to a 44.0 percentage point increase, on a base of 73.5% for the sample of mothers as a whole. This large range is not surprising, since the model is quite demanding on the data, so the true effect probably lies well within these bounds. The median effect size is an increase of 5.5 percentage points (or 7.5%), and the mean effect size is a 10.8 percentage point increase (or 14.7%). Of the twelve effects I calculate, six fall in the range of 2.7 to 6.2 percentage points, while another three fall into the 12.0 to 15.4 percentage point range, and the remaining three are high and low

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that measured the amount of grant money for which a mother was eligible for that she was not in the baseline year of 2002. None of these methods yielded particularly striking results, so in the interest of clarity and transparency I opted to report all twelve interaction terms.

outliers. Although the effect size varies depending on the method chosen to calculate it, what is clear is that the effect is positive and economically significant.

The regressions in Table 5.4 repeat the regressions in Columns 4-6 of the previous table, but limit the sample to mothers with less than a matric level of education. This group is poorer on average than the full sample of mothers, and thus is a target group for the CSG. All the same variables that were in the previous set of regressions are still in this one, but I only report the interaction terms for brevity's sake, since they are the variables of interest.

Table 5.4: Labor force status, mothers with less than matric level of education

	(1)	(2)	(3)
	broad	*narrow*	*employed*
OwnChildren0-3 * 2003	0.031	0.016	0.002
	(2.82)***	(1.26)	(0.19)
OwnChildren0-3 * 2004	0.023	0.004	0.013
	(2.06)**	(0.34)	(1.09)
OwnChildren0-3 * 2005	0.041	0.020	0.007
	(3.81)***	(1.67)*	(0.67)
OwnChildren4-6 * 2003	0.014	-0.017	-0.005
	(1.17)	(1.31)	(0.39)
OwnChildren4-6 * 2004	0.046	0.011	0.005
	(3.61)***	(0.80)	(0.40)
OwnChildren4-6 * 2005	0.017	-0.008	-0.002
	(1.44)	(0.60)	(0.19)
OwnChildren7-8 * 2003	0.009	0.009	0.008
	(0.66)	(0.55)	(0.56)
OwnChildren7-8 * 2004	0.024	0.016	0.005
	(1.64)	(1.01)	(0.37)
OwnChildren7-8 * 2005	-0.014	-0.023	-0.019
	(1.03)	(1.53)	(1.40)
OwnChildren9-10 * 2004	0.008	-0.001	0.006
	(0.61)	(0.11)	(0.48)
OwnChildren9-10 * 2005	0.012	-0.012	-0.002
	(1.07)	(0.91)	(0.20)
OwnChildren11-13 * 2005	0.014	0.005	0.013
	(1.54)	(0.50)	(1.50)
Observations	43522	43522	43522
Pseudo R-squared	0.11	0.08	0.10
Joint significance of interaction terms: p-value	0.00		

Absolute value of t statistics in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
Source: GHS 2002-2005. Marginal effects at sample means reported. Controls for household composition, personal characteristics, and year and province fixed effects not reported. Joint significance test is a likelihood ratio test.

The results do not differ significantly from the full sample, although they tend to be slightly more positive for narrow participation and employment. The range of effect magnitudes is wide, with a median effect of 8.3 and a mean effect of 13.0 percentage points (equivalent to increases of 12.0% and 18.7%, respectively). These magnitudes are slightly greater than those calculated for the full sample. These results do not change noticeably when only married women or only single women are considered (not shown).

Table 5.5 breaks down the sample by geographic location. Columns 1-3 represent mothers living in urban areas, while Columns 4-6 represent mothers living in rural areas.

Table 5.5: Labor force status of mothers, urban versus rural

	Urban			Rural		
	(1)	(2)	(3)	(4)	(5)	(6)
	broad	*narrow*	*employed*	*broad*	*narrow*	*employed*
OwnChildren0-3 * 2003	0.021	0.012	0.006	0.021	-0.001	-0.016
	(1.66)*	(0.80)	(0.35)	(1.49)	(0.05)	(1.18)
OwnChildren0-3 * 2004	0.013	-0.005	0.008	0.023	-0.002	-0.002
	(1.07)	(0.32)	(0.48)	(1.61)	(0.13)	(0.12)
OwnChildren0-3 * 2005	0.015	-0.003	-0.000	0.045	0.022	0.003
	(1.21)	(0.21)	(0.03)	(3.34)***	(1.51)	(0.24)
OwnChildren4-6 * 2003	0.005	-0.012	-0.009	0.020	-0.018	-0.007
	(0.33)	(0.70)	(0.51)	(1.34)	(1.14)	(0.51)
OwnChildren4-6 * 2004	0.020	-0.009	-0.012	0.047	0.011	0.012
	(1.34)	(0.53)	(0.65)	(3.00)***	(0.69)	(0.83)
OwnChildren4-6 * 2005	0.010	-0.005	0.007	0.013	-0.015	-0.005
	(0.73)	(0.33)	(0.43)	(0.89)	(0.93)	(0.34)
OwnChildren7-8 * 2003	0.007	0.017	0.022	0.006	-0.005	-0.011
	(0.40)	(0.84)	(1.07)	(0.31)	(0.27)	(0.69)
OwnChildren7-8 * 2004	0.049	0.034	0.021	0.007	0.011	-0.004
	(2.88)***	(1.68)*	(1.02)	(0.40)	(0.56)	(0.23)
OwnChildren7-8 * 2005	-0.005	-0.023	-0.018	-0.004	-0.008	-0.012
	(0.30)	(1.19)	(0.90)	(0.22)	(0.43)	(0.72)
OwnChildren9-10 * 2004	-0.011	-0.007	-0.012	0.022	0.012	0.021
	(0.75)	(0.39)	(0.67)	(1.38)	(0.70)	(1.50)
OwnChildren9-10 * 2005	0.010	0.002	-0.003	0.014	-0.020	0.001
	(0.72)	(0.15)	(0.19)	(0.97)	(1.27)	(0.06)
OwnChildren11-13 * 2005	0.017	0.007	0.008	-0.003	-0.018	-0.001
	(1.65)*	(0.52)	(0.63)	(0.27)	(1.51)	(0.06)
Observations	28324	28324	28324	27136	27136	27136
Pseudo R-squared	0.12	0.09	0.13	0.12	0.08	0.10
Joint significance of interaction terms: p-value	0.05			0.02		

Absolute value of z statistics in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
Source: GHS 2002-2005. Marginal effects at sample means reported. Controls for household composition, personal characteristics, and year and province fixed effects are not reported. Joint significance test is a likelihood ratio test.

Although the results vary slightly, there does not appear to be any clear pattern. There are a number of significant positive coefficients, particularly in the broad participation regressions, and still no significant negative coefficients. The interaction terms in the broad participation regressions are still jointly significant, although at the five percent level.

Table 5.6 breaks the sample into those mothers who live in formal dwellings and those who live in informal dwellings. Dwelling type is generally an indicator of poverty, but it is one that is unlikely to be influenced by the receipt of the relatively small CSG.

Table 5.6: Labor force status of mothers, by dwelling type

	Formal dwelling			Informal dwelling		
	(1)	(2)	(3)	(4)	(5)	(6)
	broad	*narrow*	*employed*	*broad*	*narrow*	*employed*
OwnChildren0-3 * 2003	0.031	0.005	-0.010	0.001	0.008	-0.005
	(2.86)***	(0.35)	(0.75)	(0.06)	(0.40)	(0.28)
OwnChildren0-3 * 2004	0.020	-0.017	-0.013	0.023	0.034	0.030
	(1.83)*	(1.28)	(0.97)	(1.25)	(1.71)*	(1.74)*
OwnChildren0-3 * 2005	0.024	-0.002	-0.013	0.045	0.038	0.023
	(2.26)**	(0.16)	(1.01)	(2.69)***	(2.08)**	(1.42)
OwnChildren4-6 * 2003	0.006	-0.026	-0.026	0.034	0.002	0.022
	(0.45)	(1.76)*	(1.84)*	(1.72)*	(0.10)	(1.22)
OwnChildren4-6 * 2004	0.026	-0.010	-0.013	0.058	0.028	0.028
	(2.04)**	(0.69)	(0.89)	(2.90)***	(1.28)	(1.53)
OwnChildren4-6 * 2005	0.004	-0.015	-0.003	0.035	0.006	0.007
	(0.33)	(1.07)	(0.24)	(1.97)**	(0.28)	(0.45)
OwnChildren7-8 * 2003	0.016	0.013	0.014	-0.015	-0.016	-0.017
	(1.12)	(0.75)	(0.85)	(0.64)	(0.65)	(0.83)
OwnChildren7-8 * 2004	0.031	0.015	0.003	0.020	0.035	0.018
	(2.14)**	(0.88)	(0.17)	(0.83)	(1.38)	(0.85)
OwnChildren7-8 * 2005	0.001	-0.015	-0.013	-0.017	-0.023	-0.024
	(0.05)	(0.91)	(0.79)	(0.76)	(0.98)	(1.25)
OwnChildren9-10 * 2004	-0.007	-0.003	-0.003	0.038	0.019	0.024
	(0.56)	(0.22)	(0.20)	(1.85)*	(0.85)	(1.29)
OwnChildren9-10 * 2005	0.009	-0.018	-0.015	0.024	0.006	0.018
	(0.78)	(1.26)	(1.06)	(1.35)	(0.32)	(1.10)
OwnChildren11-13 * 2005	0.006	-0.013	0.011	0.014	0.010	-0.011
	(0.61)	(1.14)	(1.06)	(1.03)	(0.64)	(0.85)
Observations	38865	38865	38865	16595	16595	16595
Pseudo R-squared	0.12	0.10	0.13	0.12	0.08	0.09
Joint significance of interaction terms: p-value	0.05			0.01	0.25	0.15

Absolute value of z statistics in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
Source: GHS 2002-2005. Marginal effects at sample means reported. Controls for household composition, personal characteristics, and year and province fixed effects are not reported. Joint significance test is a likelihood ratio test.

Columns 1-3 are limited to mothers living in formal dwellings, while Columns 4-6 are limited to those in informal dwellings. These results for these two sets of regressions differ in important ways. The CSG interaction variables have positive and often significant impacts on broad participation (although which terms are significant sometimes differs), but whereas the coefficients for *narrow* and *employed* are generally negative and sometimes slightly significant among mothers in formal dwellings, they are mostly positive and sometimes significant among their counterparts in informal dwellings, although the terms are not quite jointly significant in either of these regressions. This evidence suggests that the CSG has its biggest impact on employment in households that tend to be poorer and hence more subject to liquidity constraints, which supports the notion that grant income has a search-financing function.

I ran another set of regressions that seek to further isolate the impacts of the CSG on poorer households by limiting the sample to mothers in households that do not have a flush toilet connected to a public sewage system, and then to mothers in households that do not have weekly rubbish removal conducted by local authorities. While these restrictions may seem unusual, they are correlated with CSG receipt and thus help us isolate our population of interest, but they are also unlikely to be affected by an individual's grant status, because they are largely determined at the community level. The results are once again strong for *broad*, but mixed for *narrow* and *employed*, and differ little from the full sample results in Table 5.3. I omit these results for brevity.

The regressions in Table 5.7 include a set of variables that contain the participation and employment rates of individuals living in other households in the mother's primary sampling unit (PSU), the smallest geographical division into which the

GHS is divided. Each PSU has a minimum of 100 households, of which ten are sampled for the GHS.

Table 5.7: Labor force status of mothers, PSU unemployment rates by urban/rural

	Urban			Rural		
	(1)	(2)	(3)	(4)	(5)	(6)
	broad	*narrow*	*employed*	*broad*	*narrow*	*employed*
OwnChildren0-3 * 2003	0.022	0.017	0.008	0.018	-0.005	-0.020
	(1.76)*	(1.11)	(0.50)	(1.31)	(0.29)	(1.45)
OwnChildren0-3 * 2004	0.014	-0.003	0.009	0.025	-0.001	-0.002
	(1.10)	(0.20)	(0.53)	(1.73)*	(0.08)	(0.12)
OwnChildren0-3 * 2005	0.014	-0.001	0.000	0.043	0.018	-0.001
	(1.20)	(0.09)	(0.02)	(3.17)***	(1.20)	(0.05)
OwnChildren4-6 * 2003	0.006	-0.009	-0.006	0.020	-0.019	-0.006
	(0.42)	(0.55)	(0.37)	(1.34)	(1.17)	(0.45)
OwnChildren4-6 * 2004	0.021	-0.006	-0.011	0.046	0.009	0.012
	(1.44)	(0.36)	(0.59)	(2.94)***	(0.57)	(0.84)
OwnChildren4-6 * 2005	0.010	-0.004	0.008	0.011	-0.021	-0.010
	(0.74)	(0.27)	(0.44)	(0.73)	(1.32)	(0.76)
OwnChildren7-8 * 2003	0.007	0.019	0.017	0.005	-0.005	-0.008
	(0.43)	(0.97)	(0.86)	(0.28)	(0.24)	(0.48)
OwnChildren7-8 * 2004	0.049	0.036	0.020	0.005	0.012	-0.006
	(2.88)***	(1.78)*	(0.98)	(0.29)	(0.59)	(0.34)
OwnChildren7-8 * 2005	-0.004	-0.022	-0.017	-0.005	-0.010	-0.013
	(0.28)	(1.14)	(0.83)	(0.31)	(0.53)	(0.82)
OwnChildren9-10 * 2004	-0.011	-0.007	-0.012	0.023	0.011	0.020
	(0.76)	(0.43)	(0.65)	(1.46)	(0.67)	(1.39)
OwnChildren9-10 * 2005	0.011	0.007	0.003	0.016	-0.016	0.000
	(0.84)	(0.45)	(0.20)	(1.08)	(1.04)	(0.02)
OwnChildren11-13 * 2005	0.017	0.008	0.008	-0.003	-0.020	-0.006
	(1.61)	(0.60)	(0.60)	(0.22)	(1.63)	(0.56)
PSU broad participation rate	0.163			0.277		
	(8.77)***			(14.01)***		
PSU narrow participation rate		0.333			0.554	
		(16.68)***			(30.80)***	
PSU employment rate			0.432			0.474
			(21.11)***			(29.80)***
Observations	28320	28320	28320	27134	27134	27134
Pseudo R-squared	0.12	0.10	0.14	0.12	0.10	0.13
Joint significance of interaction terms: p-value	0.05			0.03		

Absolute value of z statistics in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
Source: GHS 2002-2005. Marginal effects at sample means reported. Controls for household composition, personal characteristics, and year and province fixed effects are not reported. Joint significance test is a likelihood ratio test.

Despite this relatively restrictive specification, there are still a number of significant positive terms for *broad* and one for *narrow*, all of which are positive. In both urban and

rural settings, the results for *broad* are consistently positive and jointly significant, while *narrow* and *employed* are mixed and not significant.

Although the results differ slightly across different specifications, some clear patterns emerge. The first is that in every specification the CSG eligibility and take-up interaction terms have an overwhelmingly positive and often significant impact on broad labor force participation. The results for narrow participation and employment are mixed and tend not to be significant, but when terms are significant they are almost always positive. Breaking up the sample along the lines of various social and economic indicators generally preserves these results. The formal/informal dwelling distinction is the exception: although the results for broad participation are strongly positive for both groups, the results for narrow participation and employment tend to be negative for mothers in formal dwellings and positive for mothers in informal dwellings.

Although this methodology does not indicate which of the possible effects identified in Section 3 are causing these results, they do indicate that the CSG plays an important role in alleviating some constraints to labor market success. And while they do not rule out the possibility that there is a disincentive effect associated with CSG receipt, if it exists it is coexisting with a set of positive effects that is at least as strong; there is no support for the idea that the income and disincentive effects dominate.

One question about these regressions is why certain coefficients are significant and not others, and why there is some variance in estimated effects. There is no clear theoretical reason why one of the interaction terms should be significant in one year but not the next, or vice versa. However, whatever is causing this variation does not seem to be systematic, since it exhibits no clear pattern. The model specification puts a good deal

of stress on the data, since there are twelve interaction terms along with two types of fixed effects and an array of personal and household characteristic controls, for a total of 46 variables in the basic specification, and so it is not surprising that the interaction term coefficients are not always consistent. Nevertheless, they are remarkably consistent in the aggregate, especially for the broad participation regressions, and so this concern does not seem to be a reason to doubt the overall results.

As discussed earlier, the increase in antenatal HIV/AIDS prevalence across the survey period could be a potential source of bias. However, the results in this section suggest that this is not the dominant factor. If the results were being driven by HIV/AIDS, one would expect the effect to show up most strongly in the interaction terms for children ages 0-3, since these are the children who were born during the survey period and therefore have sexually active mothers. Mothers without children aged 0-3 are less likely to be sexually active, and so one would expect any effect from HIV/AIDS not to be as strong. Yet the interaction terms do not appear to differ significantly by age of the child, as we would expect if HIV/AIDS were driving the results. Again, the coefficients on the interaction terms should be interpreted relative to the comparison year, 2002, and so only the increase in, not the level of, HIV infection could bias the results.

Husbands

I repeated the above analysis on the sample of husbands of the mothers from the previous section. While there were 55460 mothers in the sample, there are only 22170 husbands. The set of control variables includes the age, education, and disability status of both the mother and her husband, as well as the set of household composition variables

and year and province fixed effects. The interaction variables are the same as before, so the implicit assumption is that CSG income only affects husbands when their wives, as opposed to other members of the household, receive it. For brevity, I do not report the results here.

The results have mixed signs and are only occasionally significant, but tend to be less positive than for mothers. There are no clear patterns. It is not surprising that the results are more ambiguous for husbands, since the grants are usually paid to their wives and so they may have little control over this income. In addition, it makes sense that husbands would experience a stronger disincentive effect than their wives: whereas the median monthly salary of a woman transitioning into employment is R600, the median for married men is R910 – potentially enough to push them over the means test threshold.[24] Relaxing the means test, or adjusting it for inflation, could help remedy any disincentive that these males experience.

6. Pooled sample analysis – hunger and school attendance

6.1 Hunger

The methodology I use to estimate the CSG's impact on hunger is very similar to the technique used to analyze labor market outcomes. Again, I interact child age with the year of the observation to take advantage of the increases in the age eligibility limit and

[24] Author's calculations from Labour Force Surveys of September 2004 and March 2005.

the push to improve the take-up rate. However, the nature of the dependent variable is slightly different, so the interaction terms and control variables also change slightly. In this analysis, hunger is a binary variable equal to one if any child in the household is reported as having gone hungry in the past year because of a lack of food, zero otherwise. Because the hunger variable is at the household level in the GHS, each household will be treated as one observation for these regressions. The sample is therefore all households with children aged 17 and under. As with the labor market regressions, this model focuses on the extensive margin of hunger (whether children in the household go hungry) as opposed to the intensive margin (how severe the hunger is).

The interaction terms are the number of children in a given age group in the household, multiplied by a binary dummy for the year of the observation. For example, *Children0-3 * 2003* is equal to the number of children aged zero to three in the household if the observation is from 2003, zero otherwise. As with the labor market regressions, the coefficients on these interaction terms represent the effect of an additional child in the given age range in the given year, relative to 2002 (or to the years before the age group was CSG-eligible, for 9-13 year-olds).

The other control variables include non-interacted household composition variables for children in different age groups, as well as for female adults and the elderly (adult males are again the omitted group), household size, age and age-squared of the household head, gender of the household head, the number of disabled people in the household, categorical education variables equal to the number of people with a given level of education in the household (no education is the omitted category), and year and province fixed effects.

Table 6.1 below shows the results for the child hunger regressions. All models a estimated with a probit – the results are slightly more positive when using OLS. Columns 2-5 break the sample into urban and rural households, households with a household head who did not finish matric, and households that reside in formal and informal dwellings. The results vary slightly across each specification, but the vast majority of terms in each regression are negative, and sometimes statistically significant. The terms are jointly significant at the 5 percent level for the full sample, at the 10 percent level for rural households, nearly significant for urban households, and at the one percent level for households whose head did not finish matric. They are not jointly significant for households living in formal dwellings, and are almost significant at the 10 percent level for households living in informal dwellings.

These results provide strong evidence for the hypothesis that CSG receipt reduces child hunger. The magnitude of this effect is economically significant. Having a child aged 7-8 in 2003, for example, is associated with a 1.4 percentage point decrease in hunger, relative to having a child aged 7-8 in 2002. Here again, I make the identifying assumption that the grant eligibility change is the only factor that changed from 2002 to 2003 and is correlated with having a child aged 7-8 and with child hunger. Adjusting for the take-up rate increase, I calculate that one child receiving a CSG appears to reduce the likelihood that any child in the household has gone hungry in the past year by 2 percentage points (6.6%) using the median effect value or 3.5 percentage points (11.4%) using the mean effect size. For households whose head does not have a matric, a CSG makes it 8.4% less likely that any child has experienced hunger in the past year, using the

Table 6.1: Child hunger

	Full sample	Urban	Rural	Less than matric	Formal dwelling	Informal dwelling
	(1)	(2)	(3)	(4)	(5)	(6)
Children0-3 * 2003	0.002	0.004	-0.000	-0.002	0.006	-0.011
	(0.29)	(0.36)	(0.01)	(0.24)	(0.64)	(0.69)
Children0-3 * 2004	0.002	-0.008	0.010	-0.001	0.007	-0.017
	(0.27)	(0.67)	(0.84)	(0.14)	(0.77)	(1.09)
Children0-3 * 2005	0.001	-0.003	0.002	-0.002	0.008	-0.015
	(0.08)	(0.29)	(0.21)	(0.20)	(0.95)	(1.03)
Children4-6 * 2003	-0.001	-0.006	0.002	0.001	-0.008	0.014
	(0.12)	(0.47)	(0.15)	(0.13)	(0.76)	(0.82)
Children4-6 * 2004	-0.004	-0.007	-0.003	-0.002	-0.015	0.018
	(0.48)	(0.56)	(0.27)	(0.16)	(1.43)	(1.00)
Children4-6 * 2005	-0.013	-0.025	-0.005	-0.007	-0.019	0.001
	(1.52)	(1.96)**	(0.42)	(0.73)	(1.88)*	(0.08)
Children7-8 * 2003	-0.014	-0.021	-0.010	-0.016	-0.010	-0.026
	(1.35)	(1.46)	(0.68)	(1.43)	(0.84)	(1.30)
Children7-8 * 2004	-0.024	-0.008	-0.039	-0.035	-0.012	-0.051
	(2.26)**	(0.52)	(2.58)**	(2.94)***	(1.01)	(2.45)**
Children7-8 * 2005	-0.011	0.010	-0.032	-0.017	0.001	-0.035
	(1.04)	(0.68)	(2.22)**	(1.55)	(0.10)	(1.81)*
Children9-10 * 2004	-0.004	0.006	-0.013	-0.006	-0.001	-0.008
	(0.44)	(0.47)	(1.00)	(0.60)	(0.07)	(0.46)
Children9-10 * 2005	-0.008	-0.012	-0.004	-0.013	-0.008	0.002
	(0.93)	(0.97)	(0.36)	(1.33)	(0.83)	(0.13)
Children11-13 * 2005	-0.025	-0.021	-0.027	-0.028	-0.016	-0.033
	(3.64)***	(2.16)**	(2.84)***	(3.81)***	(2.08)**	(2.70)***
Household children 0-3	-0.045	-0.045	-0.040	-0.040	-0.042	-0.031
	(6.89)***	(4.83)***	(4.34)***	(5.59)***	(5.70)***	(2.46)**
Household children 4-6	-0.031	-0.033	-0.024	-0.029	-0.022	-0.032
	(4.44)***	(3.40)***	(2.44)**	(3.78)***	(2.89)***	(2.42)**
Household children 7-8	-0.016	-0.038	0.008	-0.004	-0.025	0.016
	(2.07)**	(3.60)***	(0.75)	(0.48)	(2.84)***	(1.09)
Household children 9-10	-0.020	-0.038	-0.002	-0.010	-0.018	-0.018
	(3.45)***	(4.68)***	(0.18)	(1.51)	(2.77)***	(1.56)
Household children 11-13	-0.013	-0.034	0.008	-0.005	-0.013	-0.001
	(2.98)***	(5.60)***	(1.22)	(1.05)	(2.64)***	(0.16)
Household children 14-17	-0.003	-0.007	0.004	0.005	-0.005	0.008
	(0.70)	(1.47)	(0.80)	(1.21)	(1.16)	(1.18)
Household females 18-59	-0.009	-0.012	-0.004	-0.009	-0.005	-0.015
	(2.68)***	(2.60)***	(0.85)	(2.51)**	(1.39)	(2.20)**
Household males 65 & over	-0.026	-0.026	-0.036	-0.025	-0.025	-0.020
	(3.01)***	(2.10)**	(3.07)***	(2.73)***	(2.70)***	(1.18)
Household females 60 & over	-0.076	-0.075	-0.077	-0.077	-0.071	-0.080
	(11.01)***	(7.71)***	(7.82)***	(10.22)***	(9.21)***	(5.83)***
Household size	0.062	0.071	0.050	0.058	0.051	0.054
	(18.30)***	(14.24)***	(10.39)***	(15.72)***	(13.42)***	(8.38)***
Age	0.000	-0.002	0.003	0.001	-0.000	0.004
	(0.48)	(2.26)**	(2.80)***	(0.95)	(0.06)	(2.78)***
Age-squared	-0.000	0.000	-0.000	-0.000	0.000	-0.000
	(0.16)	(2.01)**	(2.33)**	(1.03)	(0.72)	(2.77)***
Female	0.097	0.102	0.074	0.096	0.091	0.087
	(20.83)***	(16.76)***	(10.40)***	(18.34)***	(17.56)***	(9.39)***
Number of disabled in household	0.028	0.031	0.021	0.028	0.029	0.032
	(6.35)***	(5.43)***	(3.13)***	(5.83)***	(6.17)***	(3.44)***
Number of people in household with primary education	-0.013	-0.008	-0.018	-0.016	-0.005	-0.018
	(4.58)***	(1.72)*	(4.71)***	(5.31)***	(1.47)	(3.53)***
Number of people in household with less than matric education	-0.047	-0.046	-0.049	-0.052	-0.030	-0.051
	(16.19)***	(10.25)***	(12.09)***	(16.68)***	(8.84)***	(9.24)***
Number of people in household with matric education	-0.110	-0.108	-0.098	-0.086	-0.086	-0.088
	(31.89)***	(22.28)***	(18.05)***	(20.55)***	(22.64)***	(11.43)***
Number of people in household with tertiary education	-0.160	-0.140	-0.185	-0.136	-0.127	-0.147
	(17.03)***	(13.33)***	(9.85)***	(9.28)***	(13.48)***	(5.89)***
Observations	60462	30526	29936	51395	41803	18659
Pseudo R-squared	0.07	0.08	0.06	0.05	0.07	0.05
Joint significance of interaction terms: Prob > F	0.02	0.18	0.06	0.01	0.43	0.11

Absolute value of t statistics in parentheses * significant at 10%; ** significant at 5%; *** significant at 1%

Source: GHS 2002-05. Marginal effects reported. Year and province fixed effects not reported. Joint significance test is a likelihood ratio test.

median effect size, or 14% less likely using the mean effect size. The phrasing used in describing these magnitudes is awkward because the hunger variable is at the household level, whereas CSGs are received by individuals. Because of this, it is actually a stronger statement than saying that receiving a CSG reduces the probability that an individual child will go hungry. If individual level hunger data were available in the GHS, the CSG would likely appear to have a larger impact on hunger than it does in these estimates. Nevertheless, these estimates demonstrate that the CSG is associated with economically significant reductions in child hunger.

6.2 School attendance

South Africa's school attendance rates are already quite high relative to other African countries, reflecting its relatively good infrastructure and its high unemployment and hence low opportunity cost of schooling. According to the GHS, in 2005, 95.8% of African and Coloured children ages 7-17 were reportedly attending school, up slightly from 94.8% in 2002.[25] Boys and girls were equally likely to be attending school, and children in urban areas were slightly more likely to be attending school. However, increasing school attendance is an important goal of social policy, and so it is important to investigate the CSG's impact on it.

The units of observation for the school attendance regressions are each school-age child (ages 7-17) for the years 2002-2005. My dependent variable is drawn from the GHS question: "Is [this child] currently attending school or any other educational institution?" The dependent variable is a binary variable equal to one if the answer to this question is

[25] The survey question simply asks if the child is currently attending school, and does not attempt to discern how frequently the child is absent from school.

yes, zero otherwise.[26] In the regression, the terms of interest are again a set of interaction variables meant to capture the impact of CSG age-eligibility expansion for children 7-13. They are equal to a dummy for whether the child is in a given age range times a dummy for year, so that *Age7-8 * 2003* equals one if the child in question is aged seven or eight and the year of the observation is 2003, zero otherwise. The other control variables include: a gender dummy for the child; non-interacted age category dummies; the number of children in the given age category in the household (excluding the child in question); number of adults and elderly in the household (adult males are the omitted group); household size; dummies indicating whether the child has his/her mother, father, or grandparent as a primary caregiver (uncertain caregivers are the omitted category); dummies for the education level of the child's caregiver; and province and year fixed effects.

Table 6.2 gives the basic results for the CSG's impact on school attendance. The coefficients of interest are all positive and are statistically significant about half the time, although every joint significance test is significant at the one percent level. All reported regressions are estimated using a probit, although the results are nearly identical using OLS. Column 1 is the basic model, and Column 2 is a re-estimate of Column 1 with an additional control variable for the school attendance rate of other children in the same primary sampling unit (roughly equivalent to a neighborhood). The coefficients of interest scarcely change, implying that the results are not due to a community effect of any kind. Column 3 controls for the school attendance rate of other children in the

[26] Unfortunately, the GHS does not distinguish between enrollment and attendance, so it is unclear which of these is measured by the question, although it seems likely that the question would be interpreted as referring to enrollment. In my discussion, I will use the words attendance and enrollment interchangeably.

Table 6.2: School Attendance, basic model and local attendance rates

	Basic model	With PSU attendance rate	With household attendance rate
	(1)	(2)	(3)
Age7-8 * 2003	0.004	0.004	0.003
	(1.29)	(1.19)	(0.79)
Age7-8 * 2004	0.016	0.015	0.013
	(5.30)***	(5.19)***	(4.49)***
Age7-8 * 2005	0.011	0.010	0.010
	(3.76)***	(3.45)***	(3.25)***
Age9-10 * 2004	0.009	0.008	0.007
	(2.17)**	(2.14)**	(1.81)*
Age9-10 * 2005	0.003	0.002	0.003
	(0.87)	(0.53)	(0.81)
Age11-13 * 2005	0.002	0.000	0.001
	(0.52)	(0.12)	(0.36)
Female	0.000	0.000	0.001
	(0.29)	(0.28)	(0.83)
Child 7-8	0.007	0.008	0.008
	(3.25)***	(3.84)***	(3.44)***
Child 9-10	0.029	0.028	0.026
	(17.89)***	(18.41)***	(16.34)***
Child 11-13	0.037	0.036	0.034
	(27.89)***	(28.31)***	(25.03)***
Household children 0-3	-0.009	-0.008	-0.006
	(10.17)***	(9.27)***	(6.89)***
Household children 4-6	-0.001	-0.000	-0.001
	(1.02)	(0.50)	(1.39)
Other children 7-8 in household	0.001	0.000	-0.001
	(0.47)	(0.42)	(1.01)
Other children 9-10 in household	0.000	-0.001	-0.004
	(0.12)	(0.54)	(3.53)***
Other children 11-13 in household	0.001	0.000	-0.003
	(1.18)	(0.36)	(3.57)***
Household children 14-17	0.002	0.002	0.002
	(2.47)**	(2.90)***	(2.42)**
Household females 18-59	0.003	0.002	0.002
	(4.21)***	(3.24)***	(2.15)**
Household males 65 & over	-0.000	-0.001	-0.002
	(0.15)	(0.54)	(0.87)
Household females 60 & over	0.004	0.002	0.001
	(3.05)***	(1.72)*	(0.60)
Household size	-0.000	-0.000	-0.000
	(0.57)	(0.47)	(0.38)
Mother is caregiver	0.020	0.018	0.013
	(12.12)***	(11.63)***	(7.87)***
Father is caregiver	0.007	0.006	0.005
	(2.54)**	(2.41)**	(1.64)
Grandparent is caregiver	0.022	0.019	0.016
	(14.21)***	(12.79)***	(9.45)***
Caregiver education: primary	0.015	0.012	0.010
	(12.57)***	(10.52)***	(8.19)***
Caregiver education: less than matric	0.034	0.028	0.022
	(26.73)***	(22.77)***	(17.18)***
Caregiver education: matric	0.033	0.029	0.024
	(23.75)***	(20.74)***	(15.27)***
Caregiver education: tertiary	0.024	0.021	0.018
	(8.00)***	(6.76)***	(5.28)***
PSU school attendance rate		0.123	
		(29.16)***	
School attendance rate of other children in household			0.078
			(39.27)***
Observations	95925	95650	75555
Pseudo R-squared	0.11	0.13	0.16
Joint significance of interaction terms: Prob > F	0.00	0.00	0.00

Absolute value of z statistics in parentheses * significant at 10%; ** significant at 5%; *** significant at 1%
Source: GHS 2002-2005. Marginal effects at sample means reported. Year and province fixed effects not reported.
Joint significance test is a likelihood ratio test.

household, and again the results become only slightly less positive. This last result implies that the CSG has its effect almost entirely on the child who receives it, rather than being spread equally among all children in the household. This contrasts with previous studies done on the OAP that have found that pension income is pooled with other sources of income by household members (Case and Deaton 1998).[27]

The magnitude of these effects is possibly quite large. The coefficient for *Age7-8 * 2003* from Column 1 of Table 6.2, for example, indicates that eligibility alone for the CSG is associated with an increase in school attendance of 0.4 percentage points. Adjusting for the take-up rate as described in Section 5.2, I estimate that actually receiving a CSG increases the likelihood a child is attending school by 2.4 percentage points, using either the median or mean effect size. The calculated effect of grant receipt from the smallest coefficient is 0.7 percentage points, and the effect from the largest coefficient is 4.4 percentage points. Since school attendance among school-age African and Coloured children is already quite high at 95.6% across the survey years, the 2.4 percentage point median estimate closes 54% of the gap in school attendance.

Table 6.3 segments the sample to see if the CSG has different effects on different groups, using the basic model from the previous table as its base. Only the interaction terms are reported, for brevity. Column 1 includes only children whose caregiver has not completed matric, Columns 2-3 break the sample into urban/rural, and Columns 4-5 divide the sample by dwelling type. In every specification, CSG eligibility has a positive and usually highly significant effect on school attendance. The results are slightly stronger for rural households and children residing in informal dwellings.

[27] See Case and Deaton (1998).

Table 6.3: School attendance, caregiver education, urban/rural, dwelling type

	Less than matric	Urban	Rural	Formal dwelling	Informal dwelling
	(1)	(2)	(3)	(4)	(5)
Age7-8 * 2003	0.003	0.009	0.001	0.004	0.003
	(0.76)	(2.24)**	(0.12)	(1.22)	(0.42)
Age7-8 * 2004	0.019	0.012	0.019	0.014	0.021
	(5.36)***	(2.97)***	(4.49)***	(4.27)***	(3.24)***
Age7-8 * 2005	0.014	0.008	0.014	0.010	0.015
	(3.79)***	(2.05)**	(3.12)***	(2.92)***	(2.29)**
Age9-10 * 2004	0.012	0.003	0.013	-0.001	0.026
	(2.57)**	(0.57)	(2.26)**	(0.26)	(3.52)***
Age9-10 * 2005	0.006	-0.002	0.007	0.001	0.008
	(1.39)	(0.34)	(1.23)	(0.12)	(1.00)
Age11-13 * 2005	0.003	0.002	0.001	-0.003	0.011
	(0.68)	(0.57)	(0.13)	(0.85)	(1.70)*
Observations	82617	42813	53112	64063	31862
Pseudo R-squared	0.10	0.11	0.11	0.12	0.09
Joint significance of interaction terms: p-value	0.00	0.07	0.00	0.00	0.00

Absolute value of z statistics in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
Source: GHS 2002-2005. Marginal effects at sample means reported. Controls for household composition, personal and caregiver characteristics, and year and province fixed effects not reported. Joint significance test is a likelihood ratio test.

Finally, Table 6.4 attempts to investigate any possible gender dynamics related to school attendance, keeping in mind Edmonds' finding that male pension income decreased child labor and increased schooling more than female pension income. The basic model from Table 6.2 also provides the base for these regressions. Segmenting by gender of the child makes little difference, although whether the child is with their mother does matter. The terms remain positive but lose joint significance for girls living with a caregiver other than their mother, and although there are two significant coefficients for boys not living with their mother, the terms are no longer significant at the 10% level. This result is important from the standpoint of intra-household allocation, because it implies that grant money is spent differently when received by a child's mother, as opposed to another person in the household.

Table 6.4: School Attendance, gender dynamics

	Girls	Boys	Girls with mother	Boys with mother	Girls not with mother	Boys not with mother
	(1)	(2)	(3)	(4)	(5)	(6)
Age7-8 * 2003	0.006	0.002	0.005	0.005	0.020	-0.005
	(1.38)	(0.46)	(1.02)	(1.02)	(1.76)*	(0.46)
Age7-8 * 2004	0.014	0.017	0.016	0.016	0.013	0.020
	(3.34)***	(4.17)***	(3.85)***	(3.73)***	(0.93)	(2.12)**
Age7-8 * 2005	0.013	0.009	0.012	0.007	0.017	0.016
	(3.16)***	(2.00)**	(2.95)***	(1.37)	(1.43)	(1.69)*
Age9-10 * 2004	0.007	0.010	0.005	0.012	0.008	0.003
	(1.31)	(1.77)*	(0.79)	(2.12)**	(0.62)	(0.27)
Age9-10 * 2005	0.003	0.003	-0.004	0.000	0.005	0.007
	(0.62)	(0.51)	(0.65)	(0.02)	(0.38)	(0.62)
Age11-13 * 2005	0.005	-0.002	0.005	-0.001	0.021	-0.004
	(1.08)	(0.52)	(1.14)	(0.12)	(1.85)*	(0.41)
Observations	47472	48453	32104	32832	5973	15621
Pseudo R-squared	0.13	0.10	0.14	0.11	0.12	0.08
Joint significance of interaction terms: p-value	0.01	0.00	0.00	0.01	0.29	0.12

Absolute value of z statistics in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
Source: GHS 2002-2005. Marginal effects at sample means reported. Controls for household composition, personal and caregiver characteristics, and year and province fixed effects not reported. Joint significance test is a likelihood ratio test.

The results in this section provide further support for the idea that these results are not being driven by the increase in HIV prevalence. The interaction terms are consistently and positively associated with school attendance, which is theoretically consistent with the impact of a social grant but not with the increased vulnerability associated with HIV/AIDS infection. Furthermore, the children that make up the sample of these regressions are aged 7-17, and so they are less likely than younger children to be directly affected by the increase in the HIV prevalence rate between 2002-2005. The fact that the results are significant and theoretically consistent with the effects of social grants is strong evidence that the econometric specification used in this paper does in fact capture the effect of the Child Support Grant, and not another factor.

7. Conclusion

Using exogenous variation in eligibility and grant take-up, this study finds that increased probability of receiving a Child Support Grant is associated with increased school attendance, decreased child hunger, and increased broad labor force participation, while it appears to have no identifiable effect on narrow labor force participation or employment. The magnitude of these effects is economically significant: after adjusting for the grant take-up rate, CSG receipt appears to decrease the probability a school-age child is not attending school by over half, and each CSG a household receives is associated with a decrease of 8-14% in the probability that any child goes hungry, depending on the methodology used to calculate the magnitude of the effect. For mothers, having a child that receives a CSG is associated with an increase of 7-14% in broad labor force participation, after adjusting for the take-up rate. These results are robust across different specifications, but the CSG does appear to affect different groups in slightly different ways. Its effects are most positive among mothers living in informal dwellings and mothers and household heads who did not complete their matric, and although the grant's impact on school attendance is the same for boys and girls, the effect is decidedly larger for children that are living with their mother. The CSG has its effect on school attendance almost entirely on the child who receives it, rather than being spread equally among all household children, which suggests that CSG income is not pooled with other income sources in the household.

The findings on hunger confirm previous studies on the CSG, and this paper provides the strongest evidence to date that the CSG increases school attendance, consistent with literature on the Old Age Pension. The magnitude of the increase in

school attendance is actually considerably larger than the effect reported by Skoufias for the Progresa conditional cash transfer program in Mexico, in which grant payment is conditional on a child's school enrollment and attendance (2001, p.53). Whereas Skoufias finds that Progresa increases primary school enrollment 0.74-1.07 percentage points for boys and 0.96-1.45 percentage points for girls from a base of 90-94%, I find that the South African Child Support Grant increases primary school enrollment by roughly 2.4 percentage points from a base of 95.6%, decreasing non-attendance by 54%. This is an important result in light of the current debate on the impacts of conditional versus unconditional cash transfers, and one that calls into question assumptions made by de Janvry and Sadoulet, who find that income from a conditional transfer is an order of magnitude more effective at increasing school attendance than a similarly sized increase in household income, and extrapolate from this to conclude that conditional transfers must be more efficient at increasing attendance than unconditional transfers (2005, p.7).

One reason for the discrepancy between my results and those of de Janvry and Sadoulet could be that whereas they simply consider general income, I consider the effect of social grants that are usually collected by mothers and are intended for children, even though there is no monitoring of how the money is spent. Both of these factors may make money from a CSG more effective than general household income at increasing a child's probability of school attendance. This hypothesis is corroborated by Columns 5 and 6 of Table 6.4, which show that the effect of the CSG on school attendance is much smaller when a child is not living with his or her mother, and by the finding that CSG income appears to be spent on the child receiving the CSG, rather than pooled with other household income. One of the perceived merits of conditional transfers is that they

incentivize behavior that has long-term rewards, like child school attendance, but they have also been criticized for being paternalistic and potentially establishing barriers to take-up among the poorest households. While there is no way to say with any certainty whether attaching conditions to the CSG would increase school attendance even further than it currently does, this study has provided evidence that significant gains in school attendance can sometimes be achieved with an unconditional grant without the administrative cost and potentially negative consequences of imposing conditions on beneficiaries.

This study's results on the labor market impacts of the CSG also present several open questions for further research. First, it is not clear why receiving a CSG should increase a mother's willingness to accept a job, but seemingly have little effect on her search behavior or employment. One might expect that the income effect would decrease all forms of participation, while any positive effects through the reduction of search cost would manifest themselves through increased job search and employment.

One possibility is that multiple effects are working simultaneously on grant recipients. For example, receiving a CSG may give a mother some income stability and alleviate her enough from domestic duties and immediate subsistence needs that she is capable of holding a job. This would account for an increase in broad participation. However, if the means test income threshold is likely to be a binding constraint for her, this willingness to work may not translate immediately into active job search and employment – she may be passively network-searching for an employment opportunity that compensates her enough for the loss of her CSG.

Alternatively, one can imagine a situation in which a mother living in a three-generation household in a rural area begins receiving a CSG for her children, which provides enough income support for her children that she can move to a city to search for a job and leave her children with their grandparents. Since the GHS does not have data on migrants, my model would not pick up this effect. Instead, the CSG would appear to be correlated with increased willingness to work (pre-migration), but once the mother actually migrated to start looking for work, she would no longer be in the same household as her children, and so the CSG would appear not to have any effect on active search or employment. Posel et al (2006) found precisely this effect for the OAP, so it may well be the case for the CSG as well.

Another possibility is that the survey instrument is not well suited to measuring job search behavior. As Shoër and Leibbrandt (2006) have shown, the simple searching/not-searching dichotomy that economists often apply to unemployment masks a great deal of heterogeneity in South Africa, and so it is possible that grant recipients alter their search patterns in ways that simply cannot be measured using the General Household Survey. If this were the case, it would not invalidate this paper's results, but would add another layer of complexity.

What is clear is that to the extent the CSG has income or disincentive effects that discourage employment and participation, it must also have offsetting positive effects whose magnitude is at least as great. Although this study cannot say what these effects are or how they operate, it suggests that extending the social safety net seems to have had no negative impact on labor supply. And although these results cannot say what the impacts of a further extension, such as making children eligible up to age 18, would be,

there is no indication that such a change would have negative impacts and it may have important positive effects as well, especially with regard to increasing school attendance. Older children are less likely to be in school and have a higher opportunity cost of not working, so making them eligible for the CSG might have an even greater impact than it does for younger children.

In addition, the results of this study can provide some evidence about the usefulness of the means test, which has become increasingly strict as inflation has eroded the real value of the income eligibility threshold. This paper does not attempt to isolate the impact of the means test on disincentivizing employment and therefore cannot quantify the potential benefits to increasing the income threshold or removing it altogether. However, these results should reassure policymakers that if they take action to reduce the disincentives inherent in the low income cutoff, there is no evidence of a negative impact on labor supply via an income effect.

These results should also inform the debate about the Basic Income Grant (BIG), a universal, non-means tested extension of the current social security system that was proposed by the Taylor Committee report in 2002. The amount of a BIG would be slightly less than the amount of the CSG, and the main beneficiaries would be same healthy adults who collect CSG payments but receive no income support of their own from the government, and so the labor market impacts of a BIG would likely be more similar to that of the CSG than the OAP. In this light, the fact that the CSG causes no drop in labor supply suggests that a BIG would not create so-called grant dependency. Its effects may even be more positive, since it would have no means test to disincentivize labor.

Clearly, further research is needed into the complex dynamics between poverty, social grants, and reproductive and remunerated labor. Nevertheless, this study presents strong evidence that the CSG does not have an identifiable negative impact on labor supply, as is often assumed, while it does lead to significant improvements in school attendance and child hunger.

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Appendix A – Assigning caregivers

The Child Support Grant is paid to a child's primary caregiver, who the Social Assistance Act of 2004 defines as "a person older than 16 years, whether or not related to a child, who takes primary responsibility for meeting the daily care needs of that child."[28] However, the General Household Survey does not ask this question, so it is necessary to assign them. This process is important for determining which household member collects the grant and whether a child is eligible to receive a grant, since the means test is applied to the caregiver and his/her spouse. In assigning caregivers and determining eligibility, I mostly follow Budlender et al's methodology from their 2005 study of the CSG means test and eligibility.

> Our assumptions, and the estimates we need for each, are:
> - All children who are living with their mothers have the mother as primary caregiver. For these children, we need to know the income of the mother, if any. If the mother is married, we also need to know the income of her husband. If the mother is not married and not widowed, we need to have an estimate for maintenance.
> - All children who are living with their father but not with their mother have the father as primarycaregiver. For these children, we need to know the income of the father, if any. If the father is married, we also need to know the income of his wife. We do not include any estimate for maintenance because we assume that fathers will seldom be the primary caregiver if the biological mother of the child is still alive.
> - All children who are not living with either parent but are the grandchild of the household head have a grandparent as primary caregiver. For these children we need to know the income of the head of household and their spouse.
> - All children who are not covered by any of the above categories have an adult woman as their primary caregiver. For these children we take the mean income of all employed adult women in the child's household. (Budlender et al 2005, p.14)

In 2005, 71.1% of children 17 and under were assigned their mother as caregiver, 2.8% were assigned their father, 16.6% were assigned to a grandparent, and 8.0% were assigned to the first adult woman on the household roster. Breaking from Budlender et

[28] Social Assistance Act of 2004, Chapter 1, Section 1

al's procedure, I assign a further 0.8% of the children who do not have an adult female in their household to an adult male in the household. The remaining 0.7% of the children live in child-only households, and are therefore excluded from receiving CSGs and hence from my labor market analysis, but not my hunger and school attendance analyses.

I calculate the income of primary caregivers and their partners in the same manner as Budlender et al, with the modifications to income reporting described in Appendix B and with the imputed urban/rural distinction for 2005 described in Appendix D.

Appendix B – Labor income imputation

The LFS and GHS do not have perfect data on either expenditure or income, but a reliable income measure is necessary to determine grant eligibility. The only question on expenditure is categorical in nature, so respondents are asked to place themselves into a broad category (R0-R399 per month, R400-R799, R800-R1199, etc.). These categories are imprecise and problematic to use, especially for parametric analysis. Household income is also poorly captured. Each employed household member is only asked her income from her "main job," and so labor income from additional jobs may not be captured. In addition, the question cannot measure variability in labor income, which one would expect to be significant among poor households. Most problematically, the survey gives respondents the option to specify a salary category rather than give an exact figure; in LFS September 2004, for example, 71% of respondents report their salary exactly, 22% report a category, and 7% say they are employed but do not report their income (a smaller number actually report being employed but having zero income). If these groups are systematically different, analyzing only the "good" data could be biasing. Furthermore, the salary categories do not align neatly with the income thresholds for the means test applied to grant recipients, so discrete income figures are needed to estimate grant eligibility rates.

Table B.1 below shows the distribution of reported salary categories (from the respondents who reported categorical incomes, henceforth referred to as "categorical respondents") against the constructed distribution of salary categories for respondents who reported exact incomes (henceforth referred to as "discrete respondents"). A simple visual comparison shows that categorical respondents tend to have much higher incomes

than discrete respondents, and so it would be biasing simply to drop the categorical respondents.

Table B.1: Distribution of incomes by reporting choice

	Income Category (R/month)												
	1-200	201-500	501-1000	1001-1500	1501-2500	2501-3500	3501-4500	4501-6000	6001-8000	8001-11000	11001-16000	16001-30000	30001 or more
Report exact figure	5.5%	15.5%	29.9%	12.4%	14.1%	7.8%	4.1%	4.1%	3.1%	1.9%	1.0%	0.5%	0.2%
Report category	4.7%	6.1%	7.4%	8.1%	12.2%	12.3%	9.9%	12.5%	9.5%	8.1%	4.3%	3.3%	1.5%

Source: LFS September 2004. Percentages sum across.

There are several possible ways to assign income values to the categorical respondents. The simplest would be to simply assign the midpoint value of each income category to each respondent who specified that category. In her analysis of the CSG, Budlender et al (2005) make this procedure slightly more sophisticated by using the logarithmic mean of the category boundaries rather than the simple mean, but this procedure still has the drawback that it results in a large cluster of individuals at a single income level. This problem could be solved by randomly assigning values within the category, but this would only be accurate even in the aggregate if the distribution of incomes within a given category were uniform. That is, if in reality incomes tend to cluster towards R200/month in the R200-R499 category and I randomly assign based on the assumption of a uniform distribution, the results will be biased. In my work with social grants in 2005, I corrected for this problem by mapping the categorical respondents onto the income distribution of the discrete respondents within each category; although the overall income distributions of categorical and discrete respondents are different, there is reason to think that the differences in these distributions would be less significant

within a given income category. This is also the approach taken by Ardington, Lam, Leibbrandt, and Welch (2006) in their recent treatment of South African census data.

Even with this last technique, the problem remains that income assignment within a category is random, but there are very important differences between earning R501/month and R1000/month, especially because the means test income cutoffs for social grants fall at R800 and R1100/month, and so random assignment is still problematic. However, it is possible to minimize this randomness by estimating a wage function for the discrete respondents and using this model to predict wages for categorical respondents. Within each salary category, I then rank categorical respondents based on their predicted wage and use this ranking to map them onto the actual intra-categorical wage distribution of discrete respondents. I will use this latter value as an individual's salary in my analysis.

The wage function that is used to generate the predicted salary ranking is run on the sample of discrete respondents, and includes a total of 55 explanatory variables, including a variety of household and individual characteristics, as well as provincial dummies. The log of salary is used as the dependent variable. A number of the explanatory variables, such as whether or not the respondent belongs to a trade union, are related to the respondent's job, and so these variables are determined simultaneously with salary. Although this would usually pose a problem in a regression, in this case I am not interested in determining causal relationships. Rather, I want to use all the information available to predict what the wage of a given categorical respondent would be had they chosen to respond with an exact figure. In short, it is the correlation of salary with other reported variables that matters; the objective is thus to maximize the adjusted R^2 of the

regression. The only available piece of data that is deliberately left out of the regression is the salaries of other household members. Although these have an impact on an individual's salary, many households contain both discrete and categorical respondents, and the salaries of the latter could not be used as explanatory variables. However, the regression does include a count of how many other household members are employed, which would capture some of this variation and is not affected by the different methods of salary reporting.

Unfortunately, not all individuals responded to each and every survey question used to construct the regression's explanatory variables, so the full model does not predict a wage for every categorical respondent. I specify a second model by removing the variables that cause the greatest number of observations to be dropped, then re-estimate the model and predict salaries for categorical respondents who were not covered by the first regression due to missing data. I repeat this procedure twice more, each time with increasingly reduced models, to capture more and more respondents. Even after these four regressions a minute number of respondents with very poor data still do not have predicted salaries, so I simply assign them the median salary for their income category.

Next, I repeat this prediction procedure for the "unknown" respondents who reported being employed but did not report their salary. Since the data for many of these respondents is of poor quality, I run a total of five regressions, and the four respondents who still lack predicted incomes are assigned the median monthly salary of R1300.

Approximately 2% of all employed respondents report receiving zero income. Whereas Budlender assumes that they are simply lying or misreporting and assigns them

an income, I do not. While some respondents may be lying, it seems more likely that the person in question had some sort of economic activity from which they drew their livelihood, but perhaps had not engaged in it recently, and should therefore have been classified as unemployed. Note that the zero income category is distinct from the “refuse” and “don’t know” categories, to whom I assign incomes.

Appendix C – Constructing the panel

The Labour Force Survey assigns each household with a unique household number (UQNR) and each person within a household with a person number, so that an individual can be tracked across surveys by combining these two numbers. If there were no attrition or breaks in the data, the LFS would survey each household five times across a 30 month time period. In practice, however, linking individual data between survey periods is more difficult. In addition to natural attrition caused by households moving, individuals leave and enter households, sometimes resulting in a shuffling of person numbers. For example, consider a household that in March consists of a middle-aged woman whose husband is a migrant worker and therefore not considered part of the household by the LFS, and her two children in their twenties. They will be assigned person numbers 1-3, respectively. Now suppose that her husband returns in June. When the household is surveyed in September, he will likely be listed as person 1, while his wife and two children are persons 2-4.

In this case, linking individuals based solely on the combination of UQNR and person number would be a fairly serious error. Especially among poor households, individuals tend to be highly mobile, so this is an important problem. This issue sometimes arises solely as a matter of reporting error, when the household member answering the survey lists the members of a household in a different order across two survey periods. Although it is not clear that error of this sort would bias the results one way or the other, it would certainly muddle the analysis.

One way to verify that an individual surveyed in March is the same individual in September is to compare characteristics such as age, gender, population group (race), language, and education, and exclude from analysis any observation for which these traits do not remain constant or, in the case of age and education, exhibit anything other than an incremental increase. There are two problems with this. First, these characteristics are subject to significant reporting error, especially when different household members respond to the survey in different periods, so that age and education especially often appear to vary significantly or even decrease. Many elderly South Africans do not know their exact ages, and adults may not know the ages or education levels of their elderly parents; these problems are magnified in larger households. Second, simply dropping observations that do not match in this way would be biasing, because poor individuals and households would be more likely to be excluded. There is good reason to believe that individuals living in poor households tend to move more frequently than is the norm for wealthier segments of society. Poor respondents are also more likely to lack documentation of their age, and may be more unfamiliar with the educational system and its different levels and classifications. Both of these factors would result in poor individuals being disproportionately excluded from analysis. Finally, poor households are typically larger than average, and so even if poor individuals had the same rates of mobility and were subject to the same degree of reporting error as wealthier individuals, one would expect poor individuals to be excluded more frequently simply because the large household size implies a larger probability that there will be a data problem with one individual that will throw off the person number links for the rest of the individuals in the household.

It is unavoidable that some individuals will move out of the panel, but it is possible to do better than simply excluding all observations with matching UQNRs and person numbers but divergent personal characteristics. The rest of this section describes the methodology used in this paper to link individuals between surveys. All statistics in this section refer to the transition between the September 2004 and March 2005 surveys, although they are similar for the transitions between other survey periods.

First, I classify as "good matches" all individuals of whom it is reported in March that they stayed in the household in September and who have the same gender, population group, language, and whose age changes by no more than two years. Visual inspection of the dataset suggested two years as a reasonable choice, and it seems to be the value that minimizes the risk of erroneously linking two different individuals while maximizing the likelihood that the same individual will be correctly identified despite reporting error (this can be thought of as minimizing both type I and type II error). There are 130,550 individual unique combinations of UQNR and person number that are in either the September or March survey or both, and if exactly 20% of the sample were rotated and there was no attrition, either natural or artificial, we would expect 87,910 matching observations. Using the procedure above leads to 43,943 individuals being classified as good matches.

Taking the sample of individuals who were not initially classified as good matches, I then match any individuals who were not reported as having stayed in September but whose other personal characteristics match and whose difference in level of education is less than three levels. This allows me to link individuals who were reported to have not stayed with the household in September but who are likely the same

person – if memories are not perfect and individuals are highly mobile, an individual wh was reported as part of the household in September but was away for part of the month or part of a following month might not be reported as having been present, six months after the fact. Given all the other restrictions on personal characteristics, this explanation seems more likely than the alternative that one individual left and another very similar individual joined the household and was assigned the same person number. The justification for choosing three levels of education was based on visual inspection of the data. I match 549 individuals in this manner.

Next, I begin to match individuals who belong to the same household but do not share a person number. These are the individuals in households that added or subtracted a person between September and March, resulting in a mismatch of person numbers. I use gender, population group, language, and age (with two years of leeway) to identify these people. However, 2,491 individuals in September are linked to the same individual in March, mostly because of the problem caused by having two individuals of the same gender close to each other in age. For example, if a household contains a 23-year-old female and a 22-year-old female in September, and a 24-year-old female and a 23-year-old female in March, both the females in September will be linked to the 24-year-old female in March. I identify these cases and "break the tie" by determining which individual has a smaller age difference with the person to whom she is being linked in March. If the age difference is identical, I use the difference in education level. For the 260 individuals who are still "tied," I break the tie randomly. The arbitrariness of this should not be terribly important, especially since over 81% of these individuals are under

15 and thus will not factor into my analysis in any case. Once these ties have been broken, a total of 17,264 individuals have been matched.

I repeat the procedure described above for individuals who are still not matched, generating an additional 492 matches. These consist mainly of individuals who were erroneously matched the first time through.

In total, 62,127 individual matches are established between September and March. Just over 70% had matching person numbers, and the remainder were linked using the procedure described above.

Appendix D – Imputing the urban/rural distinction for General Household Survey 2005

From 2002 through 2004, the General Household Survey indicates whether a household is in an urban or non-urban area, but in 2005, the sampling methodology was adjusted to use the 2001 census as its baseline, and so the only geographic indicators given by GHS 2005 are the province and district council or metropolitan area in which each household is located in. It was therefore necessary to impute an urban/rural distinction in order to conduct analysis and determine grant eligibility. The procedure used was similar to the procedure for imputing labor income described in Appendix B. Using GHS 2004 as a reference, I constructed a regression of an urban dummy on social, economic, and demographic characteristics, estimated the model for each primary sampling unit (PSU – the smallest geographic subunit into which the sample is divided), and then applied these coefficients to the data from 2005.

Of course there is no way to directly test the accuracy of the imputation, but we can use the same process on the 2004 data, then check the prediction against the actual value. When this is done, 91.58% of the PSUs are correctly imputed, and the errors are distributed evenly. The imputed urban/rural distribution is within 0.5% of the actual distribution for each province. The imputed distribution for 2005 matches the 2004 distribution, in which 54.32% of the PSUs are urban. Simple comparisons with previous years seem to support the robustness of the imputation, and although roughly 8% of the PSUs are incorrectly imputed, this should only bias the results if the errors are not randomly distributed. However, there is no indication that this is the case.