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The impacts of unconditional cash transfers on schooling in adolescence and young adulthood: Evidence from South Africa

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Abstract

I study an expansion of a South African social grant program that provided unconditional cash transfers to adolescents for the first time. Over the period 2009 to 2012, age eligibility for the child support grant was progressively extended from children under 14 to children under 18 years old. I use a difference-in-difference identification strategy that exploits the cross birth cohort variation in adolescent grant eligibility generated by these age eligibility changes to examine how unconditional cash transfers affect schooling outcomes in adolescence and young adulthood. I find that adolescent grant eligibility increases enrollment and attainment, with the effects concentrated among females, rural individuals, and those with the lowest numerical literacy. I explore education spending as a channel through which the child support grant affects education outcomes.

JEL codes: H53, I25, O15

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1 Introduction

Human capital is an important pre-requisite for achieving long-term economic growth and improved standards of living. While school enrollment rates have been increasing in developing countries, secondary school enrollment in sub-Saharan Africa remains below 50% (Glewwe and Muralidharan 2016). It is thus crucial to understand which government policies can increase enrollment and education attainment. Cash transfer programs have become increasingly widespread in developing countries and the additional income to households could lead to improved education outcomes. While Latin American countries have tended to favour conditional cash transfers, unconditional cash transfer programs are particularly prevalent in sub-Saharan Africa.

There is now a large body of literature evaluating the impact of conditional cash transfers on educational outcomes–for example, the García and Saavedra (2017) meta-study reviews 94 studies from 47 conditional cash transfer programs in developing countries–but there is a relative paucity of studies evaluating the effects of unconditional cash transfers. This paper sheds light on how unconditional cash transfers can influence education outcomes and contributes to the debate on the necessity of conditionality in cash transfer programs by evaluating the impact on educational outcomes of one of the largest unconditional cash transfer programs in developing countries. The South African child support grant is Africa's largest–and the developing world's fourth largest–unconditional cash transfer program, in terms of both the absolute number of individuals (10.8 million) and the share of population (21%) covered (World Bank 2014).

The child support grant (CSG) is available to age eligible children whose caregivers pass the means test and represents a sizeable income transfer for recipient households. In 2010, the value of the CSG was 40% of median monthly per capita income (Woolard and Leibbrandt 2010), and among the poorest households, the child support grant accounts for an average of 40% of total household income (Delany et al. 2008). When the grant was initially rolled out it was only available to children under the age of 7, but the age eligibility threshold was extended over the years, and currently all children under the age of 18 are eligible for the CSG.

My identification strategy rests on these changes in the age eligibility cutoffs for the child support grant over the years which generate exogenous variation in CSG eligibility across cohorts. The focus of this paper is on the changes in the period 2009-2012 that extended the grant to adolescents for the first time beginning with 14 year olds in 2009, 15 year olds in 2010, 16 year olds in 2011 and finally 17 year olds in 2012. These extensions result in cross birth cohort variation in CSG eligibility during adolescence, with the 1994 and 1995 birth cohorts experiencing adolescent CSG eligibility while the 1992 and 1993 cohorts do not. I use this variation in CSG eligibility across cohorts to identify the effects of adolescent CSG eligibility on contemporaneous and longer-term enrollment and attainment using a difference-in-difference strategy with the youngest cohorts as the treated group and the oldest cohorts as the control group.

In the South African system, education is compulsory until the age of 15 or the completion of Grade 9, whichever comes first, and enrollment is close to universal before age 15 (Anderson, Case, and Lam 2001) although grade repetition is high. Dropout rates are higher at higher grades, and successful progression rates are stable at 65-80% until Grade 9 but fall dramatically for Grades 10-12 (Branson, Hofmeyr, and Lam 2014). This suggests that CSG eligibility during adolescence can have large impacts on enrollment and grade progression as this is a time when dropout and repetition rates are highest.

I examine the effects of adolescent CSG eligibility on contemporaneous education outcomes, i.e. during adolescence, and in the longer term when the affected cohorts are 18-23 years old. I find that adolescent CSG eligibility does not have an impact on contemporaneous enrollment but does increase the years of education completed during adolescence. Enrollment in young adulthood is significantly higher, and there is evidence of additional years of completed education during young adulthood as well. The effects on education attainment are concentrated among females, those living in rural areas, and those who score in the lowest quintile of a numeracy test at baseline. There is some evidence that the improvements in education attainment translate into increased learning, particularly among rural individuals. I explore educational spending as a potential channel through which adolescent CSG eligibility could affect education outcomes.

The remainder of this paper is organised as follows. Section 2 reviews the existing literature. Section 3 describes the child support grant in detail. Section 4 describes the data sources and samples used in the estimation and outlines the empirical strategy. The main results are presented in Section 5, along with differential results by gender, geographic location and numerical literacy. Section 5 also explores whether the improved education outcomes translate into increased learning. Section 6 examines potential channels through which adolescent CSG eligibility could affect education outcomes and the validity of the difference-in-difference estimation strategy. Finally, Section 7 concludes.

2 Related Literature

Theoretically, unconditional and conditional cash transfer programs have different effects on educational outcomes. Both programs increase household incomes imparting an income effect on schooling demand, but only conditional cash transfer programs have a substitution effect through decreasing the price of schooling. While the theoretical impacts are well established, there is much less empirical evidence on the effects of unconditional cash transfer programs than conditional programs.[1]

Progresa is arguably the most widely studied conditional cash transfer program in the developing world. The schooling component is the most important cash-transfer aspect of the program–families receive a monthly cash benefit conditional on a child's enrollment and attendance through to the third grade of secondary school. In one of the earliest studies of the program, Schultz (2004) finds that Progresa increased enrollment rates with the largest effects for those who have completed primary school and are transitioning to secondary school. Behrman, Parker, and Todd (2009) find that 5.5 years of exposure to Progresa increased education attainment, but an exposure length of 1.5 years had no significant impact for children who were younger than 8 prior to the intervention. However, for older children aged 9-15 years old pre-intervention, both exposure lengths had a significant positive impact on grade attainment with years of education increasing almost linearly with years of exposure (Behrman, Parker, and Todd 2011).

There are a handful of empirical studies that evaluate unconditional cash transfer programs, with two recent studies based on randomised control trials. Baird, McIntosh, and Özler (2011) is an experimental study that compares conditional and unconditional cash transfers to adolescent Malawian girls, and finds that the conditional program has significantly larger effects on school enrollment and performance than the unconditional program, but teenage marriage and pregnancy rates were much lower in the unconditional program. Benhassine et al. (2015) study a 'labeled' cash transfer program in Morocco that provided cash transfers labeled as assistance for education costs but not conditional on any education outcomes. They find significant increases in enrollment due to the program, and no difference in the impacts of a 'labeled' transfer compared to a conditional one.

In the South African context, there are studies uncovering the effects of the old age pension grant on education outcomes. The old age pension is the

[1] For more detailed reviews of the empirical evidence of cash transfers on schooling outcomes see Baird, Ferreira, et al. (2014) and Glewwe and Muralidharan (2016).

country's second next largest grant, after the child support grant, in terms of beneficiaries and covers 3.1 million individuals (National Treasury 2015), but total government spending on the old age grant is higher than on the child support grant since the monthly Rand amount of the grant is roughly four times that of the child support grant. Edmonds (2006) finds large increases in schooling attendance and decreases in child labour when households reach pension eligibility. Duflo (2003) finds that old age pension eligibility improves the health status of granddaughters living with their grandmothers, but finds no effect for males. The positive effect of the old age grant on the health of female children could improve their schooling outcomes, a channel documented by Currie (2009).

There are also studies that directly investigate the impact of the child support grant in South Africa. In a rural area of KwaZulu-Natal, Case, Hosegood, and Lund (2005) find that grant receipt is associated with an increase in school enrollment of 6 and 7 year olds. Heinrich, Hoddinott, Samson, et al. (2012) find that early receipt of the child support grant has a positive impact on educational attainment – at age 10, those who began receipt at birth had completed significantly more years of education than those who only began receipt at age 6. Coetzee (2013) finds that while longer CSG receipt does not have a significant impact on enrollment of 5-14 year olds, those receiving the CSG for 30-60% of their lives are significantly less likely to repeat a school grade. Eyal and Burns (2019) find that the child support grant reduces the intergenerational transmission of mental illness from parents to adolescents. This could be another channel through which the grant improves education outcomes for adolescents.

Finally, reducing school fees is a different way to reduce the costs of schooling. Beginning in 2007, South African schools in the poorest quintiles were declared no-fee schools. Using a regression discontinuity approach based around the poverty score for schools, Borkum (2012) finds that the elimination of fees had no significant impact on enrollment for both primary and secondary schools.

My identification strategy is most similar to that of Duflo (2003) and Akee et al. (2010). Duflo (2003) exploits the age-eligibility cutoff of the South African old age grant to compare households with pension age-eligible individuals to those with elderly individuals aged just below the age cutoff. Akee et al. (2010) estimate a difference-in-difference specification that compares cohorts with differing lengths of exposure to a large exogenous increase in household income generated by the timing of the building of a casino on a Native American reservation. They find significant positive long-run effects on the educational attainment of young adults, and find the effects are largest among the poorest households.

3 Background: The South African Child Support Grant

The child support grant was introduced in South Africa in October 1998, and was intended to target the poorest 30% of households. Prior to the introduction of the child support grant the main grant available for child care in South Africa was the State Maintenance Grant, but the distribution of this grant was highly unequal. In particular, the number of Black African children covered was relatively low, given their generally lower standards of living (Lund Committee 1996). To address the inequality of the State Maintenance Grant and provide support to the poorest children, the child support grant was introduced in South Africa in 1998 to replace the State Maintenance Grant.

The child support grant is a means-tested grant available to every age eligible child in a family. The grant was intended to "follow the child" so that it is available to the primary caregiver, who may not necessarily be the biological mother. To increase access to the grant, it is available with no additional conditions (such as immunisations or school attendance) tied to it. The income eligibility threshold was initially set at R800 for urban areas, and R1,100 for rural areas and remained unchanged until 2008, which prevented many from accessing the grant as the income threshold did not rise to match inflation (Delany et al. 2008). Finally, in 2008 the means test was changed such that the income threshold is set at 10 times the value of the grant.

Originally the grant was available to children from the ages of 0 to 7, but has been extended progressively and is now available to all children under the age of 18. The following table indicates the exact changes in the age eligibility threshold of the grant over the years:

The focus of this paper is on the changes in the period 2009-2012 that extended the grant to adolescents for the first time. In 2008, it was announced that the grant would be extended to 14 year olds in the following year. Based on evidence that the child support grant had significantly reduced child poverty, a phased extension of the grant to all those under the age of 18 was announced in 2009. The grant was extended to 15 year olds in 2010, 16 year olds in 2011 and finally to 17 year olds in 2012. No further age extensions of the grant have been enacted. The extensions over the period 2009-2012 resulted in longer grant eligibility duration for cohorts having already been eligible for the grant rather than new eligibility for cohorts who had not previously been eligible.

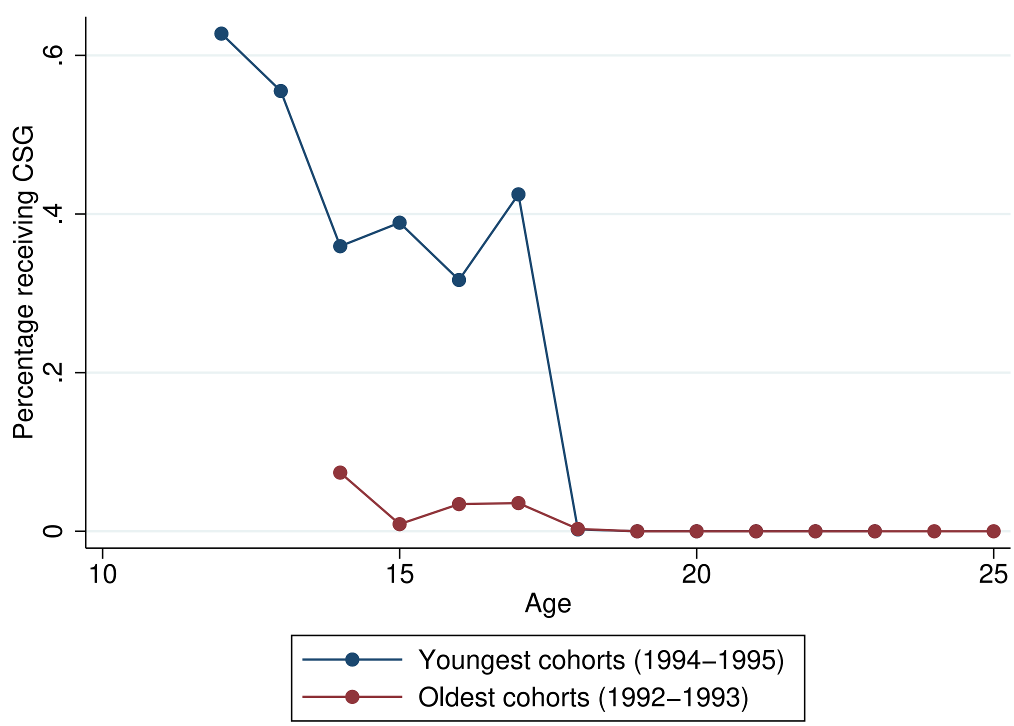
Beginning Date	Age Limit
April 1998	7
April 2003	9
April 2004	11
April 2005	14
January 2009	15
January 2010	16
January 2011	17
January 2012	18

Source: National Treasury (multiple years).

For cohorts born before the introduction of the grant, these age-eligibility changes generate exogenous variation in their eligibility for the grant during adolescence, defined as the period from age 14 until age 18. The cohorts born in 1992 and 1993 remain just above the 2009-2012 age threshold cutoffs and so do not experience any grant eligibility during their adolescence despite being eligible for the grant when they are 12 and 13 years old. The cohort born in 1995 is also eligible for the grant at ages 12 and 13, and are always just below the 2009-2012 age cutoffs making them eligible for the grant during their entire adolescence from the age of 14 until they turn 18. The 1994 birth cohort is also eligible for the grant at ages 12 and 13 but lose their grant eligibility when they turn 14 in 2008. However, they regain it a few months later when they fall under the new higher age threshold cutoffs so this cohort experiences grant eligibility for most, but not all, of their adolescence. I exploit this cross birth cohort variation in eligibility duration during adolescence to identify the effects of the grant on educational attainment of individuals born in 1992-1995.

Figure 1 shows the differences in grant take-up between the youngest (born in 1994-1995) and oldest (born in 1992-1993) cohorts. The oldest cohorts are not observed in the NIDS dataset before age 14, while the youngest cohorts are observed from age 12 onwards. It is evident that the grant take-up rate is close to zero for the oldest cohorts during their adolescent years (when they are age ineligible for the grant), but is significantly higher at 40-60% for the youngest cohorts who are age eligible during these years. It is also evident from Figure 1 that the youngest cohorts were also receiving the grant in their pre-adolescent years when they were 12 and 13 years old. It is worth noting that the oldest cohorts also experienced grant eligibility at these ages but this is not visible in the NIDS dataset since they are only observed from age 14 onwards.

Figure 1: Child support grant take-up by age



Sample includes all Black African and Coloured children born in 1992-1995 who are income eligible for the child support grant. Each point is the percentage of children receiving the grant as a percentage of all children in those cohorts whose caregivers qualify for the means test in 2008. The age eligibility threshold was 14 in 2008, and was raised to 15 in 2009, to 16 in 2010, to 17 in 2011, and to 18 in 2012. Observations are weighted using the survey weights.

4 Data and Empirical Methods

The National Income Dynamics Study (NIDS) (Southern Africa Labour and Development Research Unit 2018) is a nationally representative longitudinal survey that tracks South African individuals over time. The first wave of the survey was conducted in 2008, the second in 2010/11, the third in 2012, the fourth in 2014/15 and the fifth wave in 2017. After the first wave of data collection, the age threshold of the child support grant was raised in yearly increments from 14 to 18. Thus NIDS contains data for cohorts before, during and after the new availability of the CSG during adolescence allowing me to examine both the contemporaneous and longer-term effects of grant eligibility.

I restrict the NIDS sample to those born in the years 1992-1995 and who were income eligible for the CSG in 2008. Income eligibility is determined through a means test applied to the income of a child's primary caregiver and his/her spouse. The means test threshold changes over time, and in 2008 it was R2,100 (R4,200) for a single (married) caregiver. For children younger than 15, the NIDS questionnaire explicitly asks for the identity of the child's primary caregiver. For children where there is no direct information on their primary caregiver, I assign their resident mother (or father if there is no resident mother) as the primary caregiver. For the remaining children with no identified primary caregiver or resident parent, I use the income of the household head and their spouse to determine income eligibility for the CSG. I further restrict the estimation sample to only Black African and Coloured children given the low levels of income eligibility among the White and Indian/Asian population.[2]

While the oldest cohorts are only observed in the NIDS dataset from the age of 14 onwards, the first wave questionnaire asks about education outcomes in both the previous and current year. The questions about the previous year (2007) provide data on pre-adolescent education outcomes for the oldest cohorts when they are 13 years old. Thus, I include the data on education outcomes for the oldest cohorts from 2007 in the estimation sample and impute the values of the household control variables to their 2008 values for these observations.

[2]Black Africans and Coloureds together comprise about 85% of South Africa's population.

4.1 Identifying the Effects of the Child Support Grant: Empirical Strategy

To identify the effects of the child support grant on educational outcomes in later life, I use a difference-in-difference strategy. The older cohorts (born in 1992 and 1993) were never eligible for the child support grant during their adolescence, while the younger cohorts (born in 1994 and 1995) were. The 1995 birth cohort was eligible for the child support grant for their entire adolescence from the ages of 14 to 18, while the 1994 cohort was eligible for part of their adolescence.

I compare educational outcomes for individuals who experienced CSG eligibility during their adolescence to individuals who did not. The two younger cohorts are the treatment group and the two older cohorts are the control group. The timing of the treatment is during the years of adolescence–from the age of 14 until an individual turns 18 and is no longer eligible for the CSG–that occur after 2008. For the 1995 cohort their years of adolescence coincide completely with the years of policy changes (2009-2012). For the 1994 cohort most, but not all, of their adolescence occurs during the years of policy changes since they would have turned 14 and aged out of CSG eligibility in 2008, the year before the age threshold expansions began, but regain eligibility when they fall under the new higher age thresholds in later years. Specifically, I estimate the following difference-in-difference equation with individual fixed effects for individual i in year t:[3]

$$educ_{it} = \alpha_i + \beta_1 DuringCSG_{it} + \beta_2 AfterCSG_{it} + \delta_t + \lambda_a + X'\mu + \epsilon_{it} \quad (1)$$

$DuringCSG_{it}$ captures the treatment effect of CSG exposure during adolescence, and is equal to 1 for the treatment cohorts in their post-2008 adolescent years. Since the first wave of NIDS is in 2008 and the second wave in 2010/11, these adolescent years are observed from 2010 onwards. For the 1995 cohort, $DuringCSG_{it} = 1$ when they are aged 14 to 17, while for the 1994 cohort $DuringCSG_{it} = 1$ when they are aged 15 to 17. For the 1992 and 1993 cohorts, $DuringCSG_{it} = 0$ always. $AfterCSG_{it}$ is an indicator variable equal to 1 for the treatment cohorts when they are age 18 and over, and captures the effect of adolescent CSG exposure on outcomes in young adulthood during

[3]Fixed effects estimation of the difference-in-difference equation is preferred since OLS estimation in the presence of selective attrition of the panel is inconsistent (Lechner, Rodriguez-Planas, and Fernández Kranz 2016). Since NIDS is an unbalanced panel, i.e. not every individual is observed in all periods, I present only the individual fixed effects estimates of the difference-in-difference equation.

the four to five years after CSG eligibility ends. The treatment indicator is subsumed by the individual fixed effects α_i, and the δ_t are a full set of year dummies that flexibly control for time trends. The λ_a are age fixed effects so that the identification of the treatment effect is driven by differences between treated and untreated individuals of the same age. For example, 20 year olds who were treated (the youngest cohorts) are compared to 20 year olds who were not treated (the oldest cohorts). Robust standard errors are clustered at the level of the individual throughout (Bertrand, Duflo, and Mullainathan 2004). I use the survey weights in all regressions.

The vector X contains household size, number of other children eligible for the CSG in the household, the number of males and females (separately) eligible for the old age grant in the household, indicators for province of residence and urban location. The age eligibility threshold for the old age grant for males was slowly lowered from 65 to 60 over 2008-2010 to bring it in line with the age threshold for females. This significant policy change coincides with my treatment period and so it is essential to check that the results are robust to the inclusion of variables capturing the changing eligibility for old age grants at the household level. If the older cohorts represent a valid control group for the younger cohorts, then the inclusion of these variables related to the old age grant should not affect the results since both cohorts should be equally affected by the changes.

The two education outcome variables are an indicator for current enrollment and the number of years of completed education. The number of years of completed education is a stock variable so that any contemporaneous treatment effect of adolescent grant eligibility will persist into young adulthood, i.e. β_2 should be at least equal to β_1. Any additional longer-term effect of adolescent CSG eligibility, over the contemporaneous effect, on the years of completed education would be reflected in a β_2 that is statistically significantly greater than β_1.

The main effects of child support grant eligibility in adolescence on educational outcomes are captured by β_1 and β_2 in equation (1). Variation in birth timing relative to the age-eligibility changes in the child support grant over the period 2009-2012 identifies β_1 and β_2. The key identification assumption for this research design is that the younger cohorts experience similar trends to the older cohorts in the absence of adolescent CSG eligibility. In Section 6.2 I evaluate the validity of this assumption. Since I exploit eligibility and not take-up of the grant here, I am in effect estimating an intention-to-treat effect without relying on the potentially endogenous decision of whether to take up the grant. Since not all eligible children receive the grant (see Figure 1), these estimates represent a lower bound on the actual effect of the grant.

4.2 Data Description

Table 1 presents descriptive statistics for the main sample of Black African and Coloured children born in the years 1992 through 1995 from the first wave of NIDS in 2008. The last two columns give the differences in means across the youngest (treatment) and oldest (control) cohorts. It is evident that there is a significant difference in the years of completed education across the treatment and control cohorts but this difference is due to the mechanical effect of age on education level. The oldest cohorts are around two years older and have completed two years more education than the youngest cohorts. This difference highlights the need for the age fixed effects in the regression equation (1) to ensure that comparisons are made within age groups and not across them.

Table 1: Summary statistics at initial survey wave in 2008

	Youngest cohorts (born 1994-1995)			Oldest cohorts (born 1992-1993)			Tests of equality of means	
	Mean	SD	N	Mean	SD	N	Diff in means	SE of diff
Education outcomes								
Enrolled, indicator	0.977	0.151	1,141	0.959	0.197	1,015	0.017	0.011
Years of completed education	5.852	1.416	1,141	7.777	1.963	1,015	-1.926***	0.109
Household characteristics								
Total monthly wage income, Rands	1,943	2,109	617	2,285	3,966	562	-342	296
Number of other CSG eligible children	1.960	1.729	1,141	1.928	1.675	1,015	0.033	0.100
Number of pension eligible females	0.281	0.466	1,141	0.234	0.443	1,015	0.048*	0.026
Number of pension eligible males	0.103	0.307	1,141	0.104	0.307	1,015	-0.001	0.018
Household size	6.766	3.378	1,141	6.619	3.251	1,015	0.147	0.210
Urban location, indicator	0.391	0.488	1,141	0.434	0.496	1,015	-0.043	0.032
Individual characteristics								
Age in years	12.844	0.666	1,141	14.802	0.706	1,015	-1.958***	0.042
Black African, indicator	0.934	0.249	1,141	0.933	0.250	1,015	0.001	0.015
Female, indicator	0.481	0.500	1,141	0.549	0.498	1,015	-0.068**	0.030

Sample includes all Black African and Coloured children born in 1992-1995 who are income eligible for the child support grant. Observations are weighted using the survey weights. * denotes significance at the 10% level, ** at the 5% level, *** at the 1% level.

There are no significant differences between the cohorts on any of the household characteristics, except for the number of pension eligible females. The average household monthly income is not significantly different although it is slightly higher for the oldest cohorts, and missing for a large number of observations. Household composition is very similar across both cohorts–there are no significant differences in the number of other CSG eligible children (i.e. excluding the 1994-1996 cohorts), the number of pension eligible males, and household size. The number of pension eligible females in the household is slightly higher for the youngest cohorts and this difference is statistically significant at the 10% level. Roughly 40% of both the treatment and control cohorts live in urban areas with the majority living in rural areas. The racial makeup of both cohorts is the same comprising 93% Black African and 7% Coloured. There is a significant difference between the cohorts with regard to gender–55% of the oldest cohorts are female while 48% of the youngest cohorts are female so that females are somewhat overrepresented among the oldest cohorts.

5 Results

5.1 Main effects of adolescent child support grant eligibility on educational outcomes

Table 2 presents the main results from estimating equation (1) for the two education outcomes–an indicator for current enrollment in columns (1) and (2), and the number of years of completed education in columns (3) and (4). The $DuringCSG$ rows give the estimated coefficient β_1 from equation (1) and captures the contemporaneous effect of CSG eligibility during adolescence, and the $AfterCSG$ rows give the estimated coefficient β_2 from equation (1) and captures the effect of adolescent CSG eligibility on education outcomes in young adulthood. The t-statistics are for a test whether $\beta_1 = \beta_2$ in equation (1) against the one-sided alternative that $\beta_2 > \beta_1$ and are particularly relevant for the years of education outcome that is a stock variable. If β_2 is significantly greater than β_1, then this indicates an additional longer-term effect of grant eligibility in young adulthood rather than a persistence of the contemporaneous effect captured by β_1. All estimations include individual fixed effects. Columns (1) and (3) do not contain any additional household level controls while columns (2) and (4) contain household size, number of other children eligible for the CSG in the household, the number of males and females (separately) eligible for the old age grant in the household, indicators for province of residence and urban location. Robust standard errors clustered at the individual level are given in parentheses below the estimated coefficients and all regressions use the survey weights.

There is no contemporaneous effect on enrollment during adolescence but

Table 2: Contemporaneous and longer-term effects of child support grant eligibility during adolescence on educational outcomes for the full sample

	Enrolled		Years completed	
	(1)	(2)	(3)	(4)
DuringCSG	0.006	0.006	0.325**	0.312**
	(0.029)	(0.029)	(0.142)	(0.142)
AfterCSG	0.122***	0.119***	0.479***	0.467***
	(0.036)	(0.036)	(0.153)	(0.153)
t-statistic (AfterCSG=DuringCSG)	3.171***	3.101***	1.483*	1.503*
Implied ToT: DuringCSG	0.015	0.016	0.848	0.815
Implied ToT: AfterCSG	0.319	0.310	1.249	1.218
Household controls	N	Y	N	Y
Mean of dep var	0.688	0.688	9.002	9.002
Observations	10,569	10,569	10,343	10,343
No. of individuals	2,156	2,156	2,156	2,156

Sample includes all Black African and Coloured children born in 1992-1995 who are income eligible for the child support grant. DuringCSG gives the estimated coefficient β_1 from equation (1) and captures the contemporaneous effect of CSG eligibility during adolescence. AfterCSG gives the estimated coefficient β_2 from equation (1) and captures the effect of adolescent CSG eligibility on education outcomes in later years. The *t*-statistic is for a test that the effects AfterCSG and DuringCSG are equal, i.e. $\beta_2 = \beta_1$ in equation (1) against the one-sided alternative that $\beta_2 > \beta_1$. Household controls in columns (2) and (4) are household size, number of other children eligible for the CSG in the household, the number of males and females (separately) eligible for the old age grant in the household, indicators for province of residence and urban location. All regressions include individual fixed effects. Robust standard errors clustered at the individual level in parentheses. All regressions are weighted using the survey weights. * denotes significance at the 10% level, ** at the 5% level, *** at the 1% level.

individuals who experienced adolescent CSG eligibility are 11.9 percentage points more likely to be enrolled during their young adulthood (Table 2 column (2)). However, the estimated effects on years of completed education in columns (3) and (4) indicate that while there is no contemporaneous effect of adolescent CSG eligibility on enrollment, there is a positive effect on attainment. Individuals who are eligible for the CSG during their adolescence have completed an extra third of a year of education in their years of adolescent eligibility and this gap widens to almost half a year more in their young adulthood (Table 2 column (4)). The t-statistics in columns (3) and (4) indicate that the estimated $AfterCSG$ effect is statistically significantly greater than the $DuringCSG$ effect at the 10% level so that there is evidence of an additional longer-term effect, over the contemporaneous effect, of adolescent CSG eligibility on educational attainment.

The effects are sizeable relative to the sample averages. Adolescent CSG eligibility increases enrollment during young adulthood by 17.3% (0.119/0.688) and the years of completed education in young adulthood by 5.2% (0.467/9.002). Contemporaneously, adolescent CSG eligibility increases years of completed education by 3.5% (0.312/9.002). The effects on education outcomes in young adulthood are statistically significant at the 1% level, and the effects on education attainment in adolescence are significant at the 5% level.

The average take-up rate of the child support grant during adolescence among the eligible youngest cohorts is 38% (see Figure 1). I scale the intention-to-treat estimates from the regressions by this fraction to obtain an estimate of the treatment effect in the "Implied ToT" rows of Table 2. The estimated treatment effect of adolescent CSG eligibility is sizeable–it is 0.8 years on contemporaneous education attainment, 1.2 years on longer-term attainment and 0.3 percentage points on longer-term enrollment.

The estimates from the regressions without household controls (columns (1) and (3)) to those with controls (columns (2) and (4)) are very similar which further validates the difference-in-difference design. Recall that the set of household controls includes the number of males and females (separately) eligible for the old age grant in the household and that the male age eligibility threshold of the old age pension was slowly lowered from 65 to 60 over 2008-2010. The inclusion of control variables to capture this significant policy change over the treatment period does not affect the results, thereby providing support for the common trend assumption.

5.2 Discussion of the main effects

The results in Table 2 show that adolescent CSG eligibility increases longer-term enrollment in young adulthood but has no effect on contemporaneous enrollment. The lack of an effect on adolescent enrollment is unsurprising since the elimination of school fees among the poorest schools, that occurred at around the same time as the period examined in this paper, has also been shown to have had no effect on enrollment in either primary or secondary schools (Borkum 2012). The results are different, however, from earlier studies that show that the CSG and old age grant increased contemporaneous enrollment in rural areas (Case, Hosegood, and Lund 2005; Edmonds 2006).

I compare the estimated effect on enrollment in young adulthood with these earlier estimates of the effects of social grants on enrollment at different childhood ages since, to the best of my knowledge, there are no directly comparable estimates for the young adulthood effect. In a rural area of KwaZulu-Natal, Case, Hosegood, and Lund (2005) find that grant receipt is associated with an 8.1 percentage point increase in school enrollment of 6 year olds, and a 1.8 percentage point increase for 7 year olds, while the results in this paper show that adolescent CSG eligibility increases enrollment in young adulthood by 11.9 percentage points. Male pension eligibility increases school attendance among rural adolescents aged 13-17 years old by 12.3 percentage points (Edmonds 2006), and the estimates in Table 2 indicate that adolescent child support grant eligibility increases enrollment in young adulthood by 11.9 percentage points. Thus, the estimated enrollment effects in young adulthood are much larger than the effects of the CSG on younger children and comparable to the effects of the old age pension on adolescents even though the old age pension provides a far greater Rand amount.

The results in Table 2 also indicate that adolescent CSG eligibility increases contemporaneous and longer-term education attainment by 0.3-0.5 years. The estimated effects on education attainment in Table 2 are much larger than the previously estimated effects of the child support grant for younger children, and comparable to the effects of the old age pension among similarly aged children. Among 10 year olds, those who received the CSG from birth completed 0.14 more years of schooling than those who only began receipt at 6 years old (Heinrich, Hoddinott, Samson, et al. 2012) and this effect is smaller than the 0.3-0.5 years of additional education found for adolescent CSG eligibility in this paper. This is consistent with the idea that an intervention during adolescence will have a much larger impact on schooling outcomes than earlier interventions since it occurs at a time when individuals are much more likely to repeat a grade. Edmonds (2006) provides evidence that one additional year of male pension eligibility increases the years of completed schooling among rural children aged 5-17 years old by one tenth of a year. Table 2 gives the estimated effects of three to four years of adolescent child support grant eligibility so that the treatment effect of 0.3-0.5 years of education are on par with the estimated effects of an additional 3-4 years of pension eligibility. This is despite the fact that the monthly Rand amount of the old age pension is roughly four times that of the child support grant suggesting that a smaller grant targeted at children, even if it is unconditional, can have similar impacts on education outcomes as a larger transfer to the household.

The positive estimated effects on grade attainment during adolescence in Table 2 are similar to the estimated effects of Mexico's widely studied conditional cash transfer program, Progresa. Behrman, Parker, and Todd (2011) study the longer-term effects of Progresa 5.5 years after program implementation on schooling attainment for different age groups, with those aged 11-12 pre-intervention being the most comparable with the age groups studied in this paper since they would have been treated during their adolescence. Boys (girls) aged 11-12 years before the program who receive 1.5 years of program exposure have completed 0.24 (0.18) more years of schooling. Longer exposure of 5.5 years increases schooling attainment by 0.93 years for boys and 0.75 for girls aged 11-12 years before the program. The length of additional CSG exposure studied in this paper is three to four years that lies between the two exposure lengths of 1.5 and 5.5 years studied by Behrman, Parker, and Todd (2011), and the effects estimated in this paper of 0.3 to 0.5 more years of education lie between their estimated effects for these treatment lengths. The similarity of the estimates suggests that the unconditional transfers can have similar impacts to conditional transfers on education outcomes, in contrast to the findings of Baird, McIntosh, and Özler (2011) who find that a conditional cash transfer has a larger effect of education outcomes than an unconditional cash transfer in Malawi, but similar to those of Benhassine et al. (2015) who find no difference between the effects of a 'labeled' and conditional transfer in Morocco.

5.3 Differential effects of adolescent child support grant eligibility on education outcomes

The results in Table 2 show that while adolescent CSG eligibility does not have a significant impact on contemporaneous enrollment it does increase education attainment in the adolescent years with the gap increasing during young adulthood, and has a positive effect on enrollment during young adulthood. Table 3 presents differential results based on gender, geographic location, and numerical literacy for enrollment in Panel A and years of completed education in Panel B. Previous research has demonstrated differential effects of South African social grants depending on gender and geographic location. Duflo (2003) finds that old age pension eligibility improves the health status of granddaughters living with their grandmothers, but finds no effect for males. Edmonds (2006) demonstrates that old age pension eligibility results in large increases in schooling attendance and reductions in labour hours of rural children but has no effect on urban children. Accordingly, I present results in Table 3 from estimating equation (1) with each variable interacted with a dummy for each subsample. Columns (1) and (2) give the results with female dummy interactions, and columns (3) and (4) give the results with urban dummy interactions. To investigate whether the program had any differential effects by baseline ability, I create dummies that categorise individuals into quartiles based on their performance in the numeracy test administered in wave 1 of the NIDS survey. Columns (5)-(8) give the results from interactions with these quartile dummies–column (5) gives the results for individuals in the first (lowest) quartile of numeracy test scores, column (6) the second, column (7) the third, and column (8) for the fourth (highest) quartile.

Table 3: Differential contemporaneous and longer-term effects of child support grant eligibility during adolescence on educational outcomes for subsamples

	Females (1)	Males (2)	Rural (3)	Urban (4)	Num Q1 (5)	Num Q2 (6)	Num Q3 (7)	Num Q4 (8)
Panel A: Enrollment								
DuringCSG	-0.032	0.059	0.039	-0.068	-0.023	-0.074	0.124*	-0.031
	(0.042)	(0.041)	(0.034)	(0.053)	(0.093)	(0.081)	(0.070)	(0.076)
AfterCSG	0.070	0.167***	0.141***	0.044	0.237**	0.002	0.361***	0.153
	(0.046)	(0.053)	(0.038)	(0.069)	(0.109)	(0.094)	(0.092)	(0.114)
t-stat (After=During)	2.088**	2.097**	2.507***	1.682**	2.254**	0.833	2.751***	1.463*
Household controls	Y	Y	Y	Y	Y	Y	Y	Y
Mean of dep var	0.682	0.694	0.705	0.662	0.678	0.678	0.703	0.746
Observations	5,437	5,132	7,121	3,448	1,299	1,107	1,169	832
No. of individuals	1,105	1,051	1,441	715	266	219	235	169
Panel B: Years completed								
DuringCSG	0.643***	-0.071	0.268*	0.254	1.176**	-0.235	-0.026	-0.083
	(0.198)	(0.195)	(0.148)	(0.280)	(0.596)	(0.604)	(0.272)	(0.383)
AfterCSG	0.581***	0.345	0.536***	0.217	0.988**	0.201	0.557*	0.238
	(0.206)	(0.215)	(0.164)	(0.297)	(0.481)	(0.665)	(0.290)	(0.398)
t-stat (After=During)	-0.446	3.069***	2.235**	-0.210	-0.718	1.811**	2.553***	1.127
Household controls	Y	Y	Y	Y	Y	Y	Y	Y
Mean of dep var	9.329	8.660	8.824	9.262	8.806	9.474	9.465	9.413
Observations	5,312	5,031	6,992	3,351	1,276	1,079	1,139	819
No. of individuals	1,105	1,051	1,441	715	266	219	235	169

Sample includes all Black African and Coloured children born in 1992-1995 who are income eligible for the child support grant. DuringCSG gives the estimated coefficient β_1 from equation (1) and captures the contemporaneous effect of CSG eligibility during adolescence. AfterCSG gives the estimated coefficient β_2 from equation (1) and captures the effect of adolescent CSG eligibility on education outcomes in later years. The t-statistic is for a test that the effects AfterCSG and DuringCSG are equal, i.e. $\beta_2 = \beta_1$ in equation (1) against the one-sided alternative that $\beta_2 > \beta_1$. Household controls are household size, number of other children eligible for the CSG in the household, the number of males and females (separately) eligible for the old age grant in the household, indicators for province of residence and urban location. All regressions include individual fixed effects. Robust standard errors clustered at the individual level in parentheses. All regressions are weighted using the survey weights. * denotes significance at the 10% level, ** at the 5% level, *** at the 1% level.

Looking at columns (1) and (2) of Table 3, it is evident that the enrollment effects are driven by males while the attainment effects are driven by females. Males who experience adolescent CSG eligibility are 16.7 percentage points more likely to be enrolled in their young adulthood but there is no significant effect for females. Despite the lack of an enrollment effect, females have completed almost two thirds of a year more of education during their years of adolescent CSG eligibility. There does not appear to be any additional longer term effects above this contemporaneous effect for educational attainment among females. On the other hand, there does not appear to any significant contemporaneous impact of adolescent CSG eligibility on the education attainment of males but there is some evidence of a longer term effect. Males with adolescent CSG eligibility have completed about one third of a year more of education in their young adulthood, although this effect is only statistically significant at the 11% level.

The large positive effect on educational attainment for adolescent females is particularly notable since the South African child support grant provides the same Rand amount to all children, irrespective of gender. This is in contrast with the well known conditional cash transfer program Progresa that provides larger transfers to adolescent females than to males but the impacts of the program on educational attainment are generally similar across both genders, or slightly smaller for females (Behrman, Parker, and Todd 2011).

Similar to the effects of old age pension eligibility in Edmonds (2006), I find significant positive effects of adolescent CSG eligibility on enrollment and education attainment among individuals in rural areas (Table 3 column (3)) but no significant effects for urban individuals (column (4)). Individuals living in rural areas who experience adolescent CSG eligibility are 14 percentage points more likely to be enrolled during their young adulthood, and have completed just over a quarter of a year more education in their adolescence and just over half a year more in their young adulthood. For rural individuals, the longer-term effect on years of completed education is statistically significantly greater than the contemporaneous effect at the 5% level. Thus, there is strong evidence of an additional longer term effect of adolescent CSG eligibility on educational attainment for this subsample. The estimated effects on enrollment among rural individuals in this paper are slightly larger than those estimated by Edmonds (2006) for the old age pension.

The positive effects on education enrollment and attainment are concentrated among individuals in the lowest (Table 3 column (5)) and second highest (column (7)) quartiles of numeracy test scores. Individuals with adolescent CSG eligibility who score in the lowest quartile on the numeracy test in wave 1 of the NIDS survey accumulate one extra year of education during their adolescence. This effect is much larger than the average estimated effect or the estimated effects for any of the other quartiles, and is large relative to their average years of education that is lowest among all numeracy test score quartiles. Relative to the low average 8.8 years of completed education, treated individuals in the lowest quartile of numeracy scores complete 13.4% (1.176/8.806) more years of education. There is no evidence of any additional longer-term effect on educational attainment for the lowest numeracy score quartile, but they are 23.7 percentage points more likely to be enrolled during their young adulthood. Individuals in the third numeracy score quartile who experience adolescent CSG eligibility are 12.4 percentage points more likely to be enrolled during their adolescence and 36.1 percentage points more likely to be enrolled during their young adulthood. This very large enrollment effect in young adulthood also translates into an additional 0.6 years of completed education in young adulthood.

5.4 Do the improved education outcomes translate into increased learning?

The results in the previous sections demonstrate that adolescent CSG eligibility has a positive impact on education attainment with the effects concentrated among females, those in rural areas and those in the lowest quartile of numerical literacy. In this section, I explore whether this increase in grade attainment translates into skills. Ideally, this would be measured using test scores but unfortunately the numeracy test used in the previous section was not repeated in later survey waves. The fifth wave of the survey conducted in 2017 did include a battery of five questions designed to test financial literacy covering four topics–numeracy (interest), inflation, compound interest, and risk diversification. The financial literacy topics on inflation, compound interest and risk diversification are likely more reflective of familiarity with banking and investment than skills taught at school, and so I present results for only the numeracy question that likely better reflects knowledge gained in school.[4] The numeracy question is "Suppose you need to borrow R100. Which is the lower amount to pay back: R105 or R100 plus three percent?"

To get a sense of how adolescent CSG eligibility affected skills attainment, I investigate whether the treated (youngest) cohorts perform better on this nu-

[4]The estimated coefficients from regressions of equation (2) with an indicator for financial literacy–defined as answering a correct question in at least three of the four financial literacy topics (numeracy, inflation, compound interest, and risk diversification)–as the dependent variable are generally negative, except for the male, rural and numeracy score quartile 4 samples, and only the coefficient for the female sample is significant at the 10% level.

meracy question than the control (oldest) cohorts by estimating the following equation for the main estimation sample in wave 5, i.e. $t = 2017$:

$$numq_i = \alpha + \gamma Youngest_i + X'\mu + \epsilon_i \tag{2}$$

where $Youngest_i$ is an indicator that is equal to 1 for the youngest (treated) cohorts and 0 for the oldest (control) cohorts. The coefficient γ measures the difference across the treatment and control cohorts in the year 2017 when they are ages 21 to 25 years old. The vector X contains the same controls as in equation (1) with the addition of indicators for female and Black African. Of course, since this is a simple difference regression the coefficient γ does not identify the effects of adolescent CSG eligibility on financial literacy skills, but the main results in this paper do identify increased educational attainment due to adolescent CSG eligibility that may contribute better financial literacy skills among the youngest cohorts compared to the oldest cohorts. The dependent variable, $numq$, is binary and equal to 1 if the numeracy question is correctly answered so that equation (2) is estimated using a probit specification. Table 4 presents the results from these estimations.

The youngest cohorts, who experienced adolescent CSG eligibility, are 9% more likely to answer the numeracy question correctly, although this result is not statistically significant. There is also some evidence of a positive effects for males and females with the point estimate being slightly larger for females. There is a large and statistically significant effect for rural individuals with adolescent CSG eligibility who are 14% more likely to correctly answer the numeracy question, while the estimated effect for the urban sample is very close to zero. The results for the different numeracy score quartiles are more varied–there is almost no effect for the two highest quartiles, a negative effect for the first quartile and a positive effect for the second quartile although the latter two are not significantly different from zero.

6 Mechanisms and Identification

In this section, I explore some of the mechanisms that might explain the results found in Section 5. I also evaluate the identification assumptions of the difference-in-difference strategy used in this paper.

6.1 Mechanisms

The main empirical result of this paper is that while adolescent CSG eligibility does not increase contemporaneous enrollment, it does increase educational

Table 4: Differences in numeracy in wave 5

Sample	Numeracy question (1)	Mean (2)	N (3)
All	0.044	0.488	1,614
	(0.037)		
Males	0.038	0.479	761
	(0.053)		
Females	0.046	0.496	853
	(0.052)		
Rural	0.074*	0.532	1,091
	(0.039)		
Urban	-0.005	0.424	523
	(0.071)		
Numeracy score, quartile 1	-0.055	0.393	207
	(0.087)		
Numeracy score, quartile 2	0.071	0.403	175
	(0.115)		
Numeracy score, quartile 3	0.000	0.573	180
	(0.113)		
Numeracy score, quartile 4	0.000	0.447	121
	(0.000)		

Sample includes all Black African and Coloured children born in 1992-1995 who are income eligible for the child support grant. The estimates are of the γ in equation (2). Additional controls are indicators for female and Black African, household size, number of other children eligible for the CSG in the household, the number of males and females (separately) eligible for the old age grant in the household, indicators for province of residence and urban location. Robust standard errors clustered at the household level in parentheses. All regressions are weighted using the survey weights. * denotes significance at the 10% level, ** at the 5% level, *** at the 1% level.

attainment during adolescence and leads to increased enrollment and attainmentt during young adulthood. The additional grant income could influence education outcomes by allowing households to invest in schooling inputs, such as purchasing of school supplies, child time and effort spent in school and doing homework, and/or choosing a better school. I investigate education expenditure, parental residence and employment, and fertility as potential mechanisms that could lead to improved education attainment.

The NIDS dataset contains information on five categories of education spending–school fees, uniforms, books, transport and other–at the individual level. This allows me to directly test how different types of education spending respond to adolescent CSG eligibility by estimating the difference-in-difference with fixed effects regression equation (1) with the log of each of the five spending categories for individual i at time t as the dependent variables. Table 5 gives the results from these regressions run for the subsample of enrolled individuals, i.e. the results are conditional on enrollment so that any differences in spending are not mechanically due to differences in enrollment.

While only one of the coefficients in Table 5 is statistically significant, the estimated magnitudes suggest they might still be economically meaningful. There is large and statistically significant positive effect of adolescent CSG eligibility on spending on transport to school during adolescence–those who experience adolescent CSG eligibility spend 88% more on school transport spending. Although none of the other estimates in column (1) are statistically significant, they are large in magnitude and suggest increased spending on fees, books and other educational spending during adolescence, conditional on enrollment, for those who experience adolescent CSG eligibility. These increased expenditures suggest that the child support grant income is being channelled toward the intended beneficiary and has a positive impact on their educational outcomes.

On the other hand, with the exception of a small positive effect on fees spending, the estimated effects on educational spending during young adulthood are all negative although none are statistically significant (column (2) of Table 5). These effects during young adulthood correspond to a time when neither group is actually receiving the child support grant and so do not reflect the spending of grant income. It is not clear why educational spending in young adulthood would be lower for those who experienced adolescent CSG eligibility, but the estimates in column (2) are neither very large nor statistically significant.

Another mechanism is suggested by the results of Heinrich, Hoddinott, and Samson (2017) who find that adolescent CSG receipt reduces the likelihood of having ever been pregnant and the number of sexual partners among females.

Table 5: Effects of child support grant eligibility during adolescence on individual educational spending

	DuringCSG (1)	AfterCSG (2)	Mean (3)	No. of obs (4)	No. of indiv (5)
Fees	0.565	0.012	2.890	5,523	2,047
	(0.426)	(0.571)			
Uniforms	-0.259	-0.123	3.891	5,483	2,029
	(0.407)	(0.499)			
Books	0.532	-0.133	1.631	5,025	1,982
	(0.392)	(0.477)			
Transport	0.875*	-0.014	1.390	5,069	1,984
	(0.471)	(0.624)			
Other	0.680	-0.220	2.728	4,917	1,980
	(0.515)	(0.644)			

Sample includes all Black African and Coloured children born in 1992-1995 who are income eligible for the child support grant. DuringCSG gives the estimated coefficient β_1 from equation (1) and captures the contemporaneous effect of CSG eligibility during adolescence. AfterCSG gives the estimated coefficient β_2 from equation (1) and captures the effect of adolescent CSG eligibility on education spending in later years. Household controls are household size, number of other children eligible for the CSG in the household, the number of males and females (separately) eligible for the old age grant in the household, indicators for province of residence and urban location. All regressions include individual fixed effects. Robust standard errors clustered at the individual level in parentheses. All regressions are weighted using the survey weights. * denotes significance at the 10% level, ** at the 5% level, *** at the 1% level.

This reduced fertility during adolescence may explain the increased educational attainment found for females in Table 3.

6.2 Identification

In this section, I formally test the parallel trends assumption that underlies the difference-in-difference empirical strategy and present placebo regressions that demonstrate that there are no significant effects for cohorts who were unaffected by the policy changes.

6.2.1 Testing the parallel trends assumption

The validity of the difference-in-difference estimation strategy rests on the assumption that the difference between the treatment and control groups in the absence of any intervention is constant over time. I check this assumption using several years of data from another source–the Labour Force Survey (LFS) that was conducted twice yearly from 2000 to 2007 and assembled as part of the Post Apartheid Labour Market Series (Kerr, Lam, and Wittenberg 2017). The LFS contains information on enrollment and years of completed education for the years before the youngest and oldest cohorts enter adolescence when they are eight to twelve years old. Using this data, I estimate the following equation to formally test the parallel trends assumption:

$$educ_{it} = \alpha + \sum_{a=9}^{12} \beta_a Youngest_i \times Age\_a_{it} + \gamma Youngest_i + \delta_t + \lambda_a + X'\mu + \epsilon_{it} \quad (3)$$

where the treatment indicator $Youngest_i$ is interacted with indicators for each of the pre-adolescent ages from 9 to 12 years old (age 8 is the omitted category). If the parallel trend assumption holds, then the β_a should all be zero. As in equation (1), the δ_t are a full set of year dummies and the λ_a are age fixed effects. The vector X contains the same controls as in equation (1) except for the urban location indicator which is not available in all waves of the LFS. In addition, X contains a female indicator and the total household earnings.

There is no information on the date of birth but I construct the treatment and control cohorts based on age at the time when the LFS is conducted in February/March and September so that the youngest (treated) cohorts are those born between February/March 1994 and February/March 1996 and the oldest (control) cohorts are those born between September 1991 and September

1993. This overlaps reasonably well with the cohorts in the main estimation sample where the youngest cohorts are born in 1994 and 1995 and the oldest cohorts in 1992 and 1993.[5] There is no relational information in the LFS so I cannot determine the identity of a child's caregivers for the means test. To overcome this, I restrict the sample to all Black African children since 90% of Black African children in the main estimation sample are income eligible for the child support grant.

Table 6 gives the results from estimating equation (3) on this sample for the two education outcomes–an indicator for current enrollment in column (1) and the years of completed education in column (2). All regressions are weighted using the updated cross entropy weights based on the method developed in Branson and Wittenberg (2014) and standard errors are clustered at the household level throughout. None of the estimated coefficients are statistically significantly different from zero, and the point estimates are generally very close to zero, confirming the existence of parallel trends for the treatment and control groups prior to the treatment period studied in this paper. This provides validation of the difference-in-difference design.

6.2.2 Placebo regressions

In Table 7 I present results for placebo regressions that treat the cohorts born in 1992 and 1993–the control cohorts in the main empirical strategy–as if they experienced CSG eligibility over the years 2009-2012, with the 1990 and 1991 cohorts as the control cohorts. In effect, the placebo regressions assume a different pattern of age eligibility threshold changes than those that actually occurred, so that $DuringCSG$ is equal to 1 for the 1992 and 1993 cohorts in the 2010-2012 survey years, i.e. when the 1992 cohort are 17-19 years old and the 1993 cohort are 16-19 years old, and $AfterCSG$ is equal to 1 for these cohorts when they are 20 years old and over. Since there is no actual difference in CSG eligibility over this period between the 1992/93 cohorts and 1990/91 cohorts, the coefficients on $DuringCSG$ and $AfterCSG$ should be zero if the identification strategy is valid.

None of the estimated effects in Table 7 are statistically significant providing further support for the identification strategy used in this paper. More-

[5] The General Household Survey (GHS) is the other household survey available from 2002 and, like the Labour Force Survey, it does not contain information on the date of birth. However, since the GHS was conducted in July of each year it is not possible to construct treatment and control cohorts that correspond as well to those used in the main estimation sample. For example, the treatment cohort could be constructed as those born between July 1994 and July 1996 but this excludes all those born in the first half of 1994 and includes all those born in the first half of 1996.

Table 6: Test of the parallel trend assumption using an earlier dataset

	Enrolled	Years completed
	(1)	(2)
YoungestXAge9	0.001	-0.003
	(0.008)	(0.053)
YoungestXAge10	0.010	0.056
	(0.010)	(0.077)
YoungestXAge11	0.006	0.002
	(0.011)	(0.094)
YoungestXAge12	0.004	-0.136
	(0.013)	(0.103)
Mean of dep var	0.977	2.959
No. of observations	58,542	60,287

Sample includes all Black African children born in September 1991 to September 1993 and February/March 1994 to February/March 1996. The coefficient estimates are of the τ_a in equation (3). All regressions contain controls for gender, household earnings, household size, number of other children eligible for the CSG in the household, the number of males and females (separately) eligible for the old age grant in the household and indicators for province of residence. Robust standard errors clustered at the household level in parentheses. All regressions are weighted using the updated cross entropy weights. * denotes significance at the 10% level, ** at the 5% level, *** at the 1% level.

Table 7: Placebo regressions

	Enrolled		Years completed	
	(1)	(2)	(3)	(4)
DuringCSG	0.022	0.024	-0.116	-0.111
	(0.055)	(0.055)	(0.135)	(0.135)
AfterCSG	-0.057	-0.062	0.052	0.067
	(0.042)	(0.041)	(0.194)	(0.192)
Household controls	N	Y	N	Y
Mean of dep var	0.560	0.560	9.712	9.712
Observations	9,917	9,917	10,028	10,028
No. of individuals	2,054	2,054	2,054	2,054

Sample includes all Black African and Coloured children born in 1990-1993 who are income eligible for the child support grant. DuringCSG gives the estimated coefficient β_1 from equation (1) and captures the contemporaneous effect of placebo CSG eligibility. AfterCSG gives the estimated coefficient β_2 from equation (1) and captures the effect of placebo CSG eligibility on education outcomes in later years. Household controls in columns (2) and (4) are household size, number of other children eligible for the CSG in the household, the number of males and females (separately) eligible for the old age grant in the household, indicators for province of residence and urban location. All regressions include individual fixed effects. Robust standard errors clustered at the individual level in parentheses. All regressions are weighted using the survey weights. * denotes significance at the 10% level, ** at the 5% level, *** at the 1% level.

over, the magnitudes of the estimated coefficients in the placebo regressions are generally much smaller than the estimated effects in Table 2. These results indicate that the adolescent CSG eligibility variables are not picking up on any other changes occurring over this period, but do identify the effects of adolescent CSG eligibility on contemporaneous and longer-term education outcomes.

7 Conclusions

It is often suggested that conditional cash transfers are preferred to unconditional ones, especially in developing countries (Orazem and King 2007). However, the results in this paper do not support this suggestion. Eligibility for the unconditional child support grant during adolescence increases contemporaneous educational attainment and enrollment during young adulthood, with evidence of additional increased educational attainment in young adulthood as well. There is further evidence that the transfer positively influenced education outcomes for groups that might be most vulnerable to dropout and grade repetition: The effects on education attainment are largest for females, those living in rural areas, and those who score in the lowest quintile of a numeracy test at baseline. There is some evidence that the increased educational attainment translates into increased learning, measured by performance on a numeracy question, especially among rural individuals.

The improvements in education outcomes from increased adolescent CSG eligibility are larger than those estimated from eligibility earlier in life by Case, Hosegood, and Lund (2005) and Heinrich, Hoddinott, Samson, et al. (2012), and similar to those estimated for the old age grant by Edmonds (2006) despite the fact that the monthly Rand amount of the child support grant is a quarter that of the old age grant.

Economists often cite reduced employment as one of the greatest concerns with unconditional cash transfers. However, I do not find that the parents of adolescents with CSG eligibility reduce their employment and the results are suggestive that fathers' employment may even increase. This is consistent with Ardington, Case, and Hosegood (2009) who show that the old age pension significantly increases the employment of adults in the household. It is likely that the observed increases in enrollment during young adulthood would show up as decreased labour force participation for CSG eligible cohorts over these years, but it may well lead to increased employment in later years. The long term impacts of the child support grant on employment and income have not yet been studied.

This paper provides evidence in support of unconditional cash transfers. Even though the child support grant is neither conditioned on any education outcomes nor 'labeled' for education spending, it has a significant impact on education outcomes during adolescence and young adulthood that are very similar in magnitude to the effects for Mexico's conditional transfer program, Progresa, estimated by Behrman, Parker, and Todd (2011). Further, there is some evidence that the grant is actually channelled toward increased educational expenditures for the intended beneficiary. Since conditional cash transfer programs have a larger administrative and monitoring burden than unconditional cash transfers, the results in this paper suggest that policymakers might do better by implementing unconditional cash transfer programs instead.

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