The impact of South Africa's Child Support Grant on schooling and learning

Carolyn Heinrich
University of Texas, Austin

John Hoddinott
International Food Policy Research Institute

Michael Samson
Economic Policy Research Institute

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Address for correspondence: John Hoddinott, International Food Policy Research Institute, 2033 K Street NW, Washington DC, USA, 20006. Em: J.Hoddinott@cgiar.org

Abstract

This paper examines the impact of South Africa's Child Support Grant (CSG) on the schooling and learning attainments of children. Children who were enrolled in the CSG at birth completed 0.14 more grades of schooling than children who were enrolled at age six. There are marked gender differences. Early CSG enrollment improves girls' grade attainment and their scores on tests of mathematical ability and reading. There are no impacts on boys' schooling or learning. The CSG plays a compensatory role in narrowing the grade attainment gap between children whose mothers have not completed primary school and mothers with at least some secondary education. This occurs because early receipt of CSG reduces the likelihood that children from disadvantaged backgrounds are less likely to enroll late.

1. Introduction

A considerable body of evidence exists on the impact of cash transfers on children's schooling, see Fiszbein and Schady (2009), Behrman and Parker (2010), Barrientos and Niño-Zarazúa, (2010) and DfID (2011) for recent reviews of this literature. These show positive effects on enrollment and attendance with the magnitudes of these impacts typically varying by pre-program enrollment rates. However, much of what is known about cash transfers and children's schooling comes from the analysis of conditional cash transfer programmes in Latin America. There are relatively few studies situated in sub-Saharan Africa. The evidence base on cash transfers' impacts on grade attainment and learning is more limited than that on enrollment and attendance. In developing country contexts, there is little empirical evidence of the impact of providing cash transfers earlier rather than later during childhood on education-related outcomes.[1]

This paper contributes to efforts to fill these knowledge gaps. It examines the impact of South Africa's Child Support Grant (CSG) on the schooling and learning attainments of children.[2] The CSG is an important transfer program to explore. It is large, benefitting more than 10 million children so analysis of its impacts provides evidence of a cash transfer intervention that operates at scale. Unlike general poverty reduction transfer programs, the CSG puts children as its focal point with the result that children's caregivers are – subject to certain conditions – eligible to receive the CSG on behalf of the child from the child's birth onwards. This feature makes it possible to assess the impact of cash transfers given prior to start of schooling on subsequent education related outcomes. We use data from a survey instrument specifically designed to capture the CSG's impact on a variety of schooling and learning outcomes. This includes grade attainment, grade progression, age at enrollment and impacts on reading skills

[1] Cunha, Heckman and Schennach (2010) discuss the theoretical foundations underpinning the issue of investments made at different points in a child's lifecycle. de Janvry and Sadoulet (2006) discuss the calibration of optimal transfers at different ages to maximize the impact of a conditional cash transfer program on enrollment.
[2] There are a small number of studies that look at the impact of the CSG on schooling outcomes. Case, Hosegood and Lund (2005) find that the CSG increases the likelihood of enrollment. Samson *et al* (2004) use a three-stage model to show a correlation between household CSG receipt and increased school attendance, a result confirmed by Williams (2007). However, this finding is disputed by Santana who argues that "The increase of the age eligibility in the Child Support Grant implemented in South Africa has not led to considerable improvements in children's school attendance" (Santana, 2008, p. 35).

and mathematics, allowing us to move beyond the typical schooling outcomes that are assessed, attendance and enrollment.

We begin with short descriptions of the CSG and the data available to us. The program has been widely available for a number of years and some children have been receiving grant benefits since birth while others have only received transfers for several years. In section 4, we describe how using dose-response models with generalized propensity scores addresses the complications that these factors introduce into our efforts to estimate impact. Results are presented in sections 5 and 6 with conclusions found in section 7.

2. The Child Support Grant

The South African Child Support Grant (CSG) was introduced in 1998 following a recommendation by the Lund Committee to replace the poorly functioning State Maintenance Grant (Lund, 1996). A conscious decision was made to ensure that the grant was available to children from birth onwards, a decision influenced by evidence on the importance of supporting early childhood development (Lund, 1996, 2008). It was also decided that grants would not be conditional on school enrollment given that enrollment rates in South Africa were already high (Lund, 2008). Initially, applicants were required to pass a means test which was based on household income. Recipients needed to offer proof that household income was below the threshold level for eligibility and, if the caregiver was not the child's parent, proof that efforts to secure funds from the child's parents were made unsuccessfully. In addition, all recipients were required to participate in “development programmes” and to have their children immunized (Delany, Ismail, Graham, and Ramkisson, 2008). However, these conditions created barriers to receipt for many poor households. Not all caregivers were able to access the development programmes or could afford the costs associated with getting their children immunized. Children living in poor rural areas where the development programmes were not offered and required costly travel to a heath care were often excluded (Samson, 2002, 2004). Not surprisingly, initial take-up of the grant was low.

In 1999 the participation and vaccination conditions were dropped and the application process was modified to make these requirements less onerous (Delany, Ismail, Graham, and

Ramkisson, 2008). The elimination of these conditions increased the take-up rate especially in very poor areas (Samson *et al*., forthcoming). The means test was altered such that grant eligibility was determined based on caregiver's and spouse's income as opposed to household income. However, the means test remained unchanged in nominal terms from 1999 until 2008. In 2008 the grant level was increased and the means test was updated so that the eligibility threshold was equal to ten times the value of the grant (Hall, 2010). Since then, the grant amount, and therefore the means threshold, has increased in a stepwise fashion. As of April 2012, the grant was R280 per month and the threshold set at R33 600 per year for single caregivers and R67 000 per year for married couples.[3]

Modifications to age limit eligibility have also been made. When the grant was first created in 1998, the grant was limited to children younger than seven years old (Lund, 2008). In April 2003, the age limit was increased to include children under the age of nine. This was further extended in 2004 and 2005 to include children up to the age of eleven and fourteen respectively (Delany, Ismail, Graham, and Ramkisson, 2008). Currently, a child is eligible until their eighteenth birthday. In 2012, there were approximately 10.8 million CSG beneficiaries (SASSA, 2012).

3. Data

Our data are drawn from a survey implemented in five South Africa provinces, Western Cape, Eastern Cape, KwaZulu-Natal, Gauteng and Limpopo. The survey was implemented using a two-stage process. Stage one consisted of selecting geographical areas (Primary Sampling Units or PSUs) with a probability of selection proportionate to the size of the CSG beneficiary population. A PSU consisted of a paypoint – the physical location where beneficiaries received their payments. Within these selected PSUs, we were given access to the South African Social Security Administration’s data base – SOCPEN - of beneficiaries. From this, two groups of households with children born in 2000 were chosen. One group consisted of children who were reported to have been enrolled in the CSG at birth or before they had reached 18 months. The second group was children who according to SOCPEN had been enrolled between age five and

[3] In April 2012, one US dollar was worth approximately 8 South African rand.

nine years. Sample size was estimated to be large enough to detect a ten percent increase in children's grade attainment and if the sample was divided equally between males and females, would detect a 15% increase in this outcome score for a sex-disaggregated sub-sample.

The survey was fielded between October 2010 and March 2011. Two questionnaires were filled out by surveyed households and enumerators. One focused on the entire household while the other two focused in depth on the sampled child. Detailed information was collected on household characteristics at the time of the child's birth as well as details about their current living situation and context. These include measures of wealth, household demographic structure, characteristics of the caregiver, location characteristics and access to forms and offices needed to apply for the CSG. It contained a detailed set of questions on when the household enrolled in the CSG and whether access to the CSG was interrupted,[4] and it also collected detailed information on schooling histories, child time allocation and anthropometry. Children completed tests that covered reading and mathematics skills.

Implementing the survey proved to be challenging. While most communities and respondents were welcoming and cooperative, in some areas, there were concerns that the survey was a mechanism through which information on the means test was being verified and the enumerator teams had to exert considerable effort to elicit the trust of respondents. Another issue related to the accuracy the SOCPEN administrative data. Not only were basic information such as addresses and caregiver characteristics incorrect or missing, there were some cases where age at enrollment was listed incorrectly. As a result, we have children enrolled at every age between birth and 10 years of age in our sample (Table 1). In the analysis below, we use data on 1,187 children (575 girls and 612 boys) for whom we know their age of enrollment in the CSG.

Descriptive statistics on outcomes are found in the sections where explore the impact of the CSG on them. Here we note that as part of our exploratory work with these data, we compared sample characteristics to data derived from two nationally representative data sets, the 2008 National Income Dynamics Study (NIDS) and the 2010 General Household Survey (GHS). We limit the comparison to only those in the same five provinces where the CSG study

[4] In this sample, there are relatively few cases (less than 10) where receipt of the grant was interrupted.

sampled and to those households with 10-year-olds who were receiving the Child Support Grant. When making these comparisons, we applied the sampling weights provided in the GHS and NIDS data sets. Table 2 shows that across four characteristics – household size, number of children less than 18 years old, access to electricity and ownership of corrugated iron/zinc roofing sheets - our sample is very similar to those households found in the NIDS and the GHS.

4. Methods

In assessing the impact of the CSG, we must address four challenges: (i) There is no scope (legally or practically) for randomly allocating the CSG; (ii) Children first obtained access to the CSG at different ages. Some children were enrolled at birth while others did not receive their first CSG until they were eight or even older; (iii) By 2011, enrollment of the target population in the CSG was so high that it was infeasible to find a sample of non-enrolled children; and (iv) We have a single cross-sectional survey that provides the data for the evaluation. But the data available to us also have a number of strengths, including detailed information on children and their households at different times in their life course and the timing and length of their receipt of the CSG.

Consider the case where participation in a program is binary. Let Y_i^1 be the outcome of the *i*th household if it is a beneficiary of this program and let Y_i^0 be that household's outcome if it does not receive benefits. The impact of the program is given by $\Delta = Y_i^1 - Y_i^0$. However, we only observe the household, and therefore Y_i, in one of these states; the household either gets or does not get the program (or gets it early vs. late). Let D indicate program participation (the "treatment"): $D = 1$ if the household receives the program; $D = 0$ otherwise. The evaluation problem is to estimate the average impact of the program on those that receive it:

$$\Delta^{ATT} = E\left(\Delta \mid X, D=1\right) = E\left(Y^1 - Y^0 \mid X, D=1\right) = E\left(Y^1 \mid X, D=1\right) - E\left(Y^0 \mid X, D=1\right), \quad (1)$$

where X is a vector of child, maternal and household characteristics that serve as control variables and subscripts have been dropped. This measure of program impact is the "average impact of the treatment on the treated." We observe values for the expression $E(Y^1 \mid X, D = 1)$

in our data. That is, for households who received CSG benefits, we do observe outcomes Y^1 given their characteristics, *X.* The problem we face is that $E(Y^0 \mid X, D = 1)$—conditional on *X,* the outcome values that a CSG child (D = 1) would have received if it had not received program benefits or received them late, (Y^0), is not observed.

Rosenbaum and Rubin (1983) showed that this problem could be addressed by matching members of the treatment group to members of the control group on the basis of the probability (or propensity) to participate in the program, given the set of characteristics *X*. Doing so requires assuming that after controlling for *X*, mean outcomes for nonparticipants are identical to outcomes of participants if they had not received the program, an assumption known as conditional mean independence or unconfoundedness (Imbens and Wooldridge, 2009). Second, we assume that $P(X)$ is well-defined for all values of *X*. Rosenbaum and Rubin show that if outcomes are independent of program participation after conditioning on *X*, then outcomes are independent of program participation after conditioning only on $P(X)$.

Hirano and Imbens (2005) extend this approach to cases where, as with the CSG, treatment is continuous. Define $\mathcal{T}$ as the set of all treatment levels (such as the number of years a child has received the CSG) and *T* as a specific treatment (years) level. Define the treatment interval $[t_0, t_1]$, so that $T \in [t_0, t_1]$.[5] We are interested in calculating the average dose-response function, $\mu(t) = E[Y(t)]$. Hirano and Imbens note that the unconfoundedness assumption in the binary case can be generalized to the case where *T* is continuous. They define the Generalized Propensity Score, *R,* as $R = r(T, X)$. They note that "The GPS has a balancing property similar to that of the standard propensity score. Within strata with the same value of $r(T, X)$ the probability that $T = t$ does not depend on the value of *X*" (Hirano and Imbens 2004, 2). In combination with unconfoundedness, Hirano and Imbens prove that assignment to treatment is unconfounded, given the generalized propensity score.

To implement their approach, we first estimate the values of the GPS. We assume that the treatment variable is normally distributed, conditional on the covariates *X:*

$$g(T) \mid X \sim N\{h(\gamma, X), \sigma^2\}. \qquad (2)$$

[5] In the case of dichotomous treatment, $\mathcal{T} = D$ where $D \in [0, 1]$.

We estimate (2) using maximum likelihood and calculate the GPS as:

$$\check{R}_i = [2\pi\ \sigma^2]^{(-0.5)} \exp[\ (-(2\sigma^2)^{-1})[\ g(T_i) - h(\gamma, X)]]. \qquad (3)$$

Next, as with case of a binary outcome, we test the balancing properties. Following a suggestion found in Kluve *et al* (2007), we divide the sample into four equalizing sized groups based on the distribution of the treatment variable, cutting the sample at its quartiles. We then divide each group into five blocks by the quintiles of the GPS using only the GPS distribution of households in that group. Within each block, we calculate differences in means of each element of *X* for households in a given block compared to households in the same group but in different blocks. As Kluve *et al*. note, this procedure tests whether within each group covariate means of households belonging to the particular treatment-level group are significantly different from those of household with a different treatment level but similar GPS. A weighted average over the five blocks in each treatment-level group is then used to calculate a t-statistic of the differences-in-means between the particular treatment-level group and all other groups. This procedure is repeated for each treatment-level group and each covariate. If adjustment for the GPS properly balances the covariates, differences-in-means should not be statistically different from zero.

With the balancing property satisfied, we estimate the conditional expectation of *Y*, given *T* and *R*. *Ex ante*, we do not know the functional form this takes and so Bia and Mattei (2008) suggest using polynomial approximations of order one, two, and three. Having done so, we can obtain a dose-response function by estimating the average potential outcome at specified levels of treatment (transfers) and use bootstrap methods to calculate the confidence intervals for these.

In the results presented below, we estimate (3) with the following covariates appearing in *X:* characteristics of the child (relationship to the household head, race), household access to other social grants (whether anyone in the household receives the Old Age Grant or whether another child in the household receives the CSG), maternal characteristics (mother's age, mother's age squared, schooling), paternal characteristics (father's age known), wealth of the household at time of birth (dwelling had metal or tile roof, household had electricity), whether the mother was given an application form for the CSG when the child was born, and location of birth (province, urban or rural locality) affect when the child first starts receiving the grant.[6] Note that we do not need to match on age as all children were born in the same year, 2000.

The choice of these covariates reflects two considerations: observations regarding the CSG application process found in qualitative (Devereux et al (2010), Hunter and Adato, 2007) and quantitative (Case, Hosegood, and Lund , 2005) studies, and whether the covariates balance across treatment levels. Prior research identifies a number of factors affecting the ability of caregivers to apply for the CSG. One relates to knowledge about eligibility criteria and the application process. For example, Case, Hosegood and Lund (2005) hypothesized that primary care givers may not believe themselves to be eligible for a grant if they are not the biological mother of an age eligible child and this informs the inclusion of the child's relationship to the household head. Receiving a CSG application at the time of the child's birth and experience with the CSG with another child in the household or with another grant is also likely to improve understanding of the application process and thereby increase the likelihood of earlier enrollment in the CSG.

Maternal age and schooling have two effects on the timing of enrollment. Older and better educated mothers may have greater awareness of the CSG and understanding of how the application process works. They may find it easier to navigate the application process because they are more confident in their interactions with program staff. More educated mothers might find the application forms, which are in English, less daunting to complete. But to the extent that age and education are correlated with higher earnings, older and more educated women face a higher opportunity cost of applying as it is not uncommon to wait for a long time in queues at welfare offices (Hunter and Adato, 2007).

While the application process is free, there are other costs associated with the application process such as the costs of travelling to offices where CSG applications are processed. The household wealth variables are proxies for the ability to absorb these costs. If

[6] Results are available on request.

caregivers perceive that the child is only temporarily residing in the household, they may decide that the limited amount of benefits they will receive does not justify the time and monetary costs of applying for the grant, hence the inclusion of child's relationship to the head as a covariate. Physical access to these offices varies across provinces and between rural and urban centers which is why these are included. There may also have been provincial and rural/urban differences in the dissemination of the grant, the speed with which applications were processed and other factors that cause access to the CSG to vary and these location variables capture these differences too. Lastly, household circumstances change over time and this might also affect the timing of grant receipt. We include the following shocks that affected the child between birth and 10 years of age as covariates to capture this: adult death, serious illness, loss of work, loss of transfers and serious family conflict.

Next, we test the balancing properties of these data. We first divide the sample into four quartiles based on treatment levels. Following Carneiro and Rodrigues (2009), we then test whether the mean for each covariate in each group differs from the mean value of this covariate in the other two groups combined. We then calculate these mean differences adjusting for the GPS as described above. With 25 covariates, we calculate 100 t-statistics and assess whether, at the 90 and 95 confidence levels, we do not reject the null hypothesis that the mean difference in covariates is zero. Before the adjustment, there are many mean differences in covariates where we reject this null hypothesis at either confidence level. After adjusting for the GPS, there are only four instances where the t-statistic exceeds 1.96. This is less than what we would expect in terms of Type I errors given that we are testing across 100 mean differences at a 95 percent confidence level, implying that the GPS successfully balances the covariates.

Lastly we estimate the conditional expectation of *Y*, given *T* and *R,* where here Y is the outcome variable. Initially, we use a linear specification that only includes the treatment (years of participation) level, the GPS, and the interaction (years *x* GPS) of these two terms. We use the results of this estimation to calculate a dose-response function at specified levels of transfers and use bootstrap methods to calculate the confidence intervals for these. As a specification check, we also used a quadratic specification finding that this gives similar estimates.

One issue remains; we have only a single cross-sectional survey to work with and ideally we should be estimating models of changes over time. But many of our outcomes can be interpreted as cumulative changes since birth. For example, when we talk about grade attainment, we can also describe this as the change in the number of grades attained between age zero (birth) and age 10 (when survey was completed). Figure 1 illustrates this idea.

In Figure 1, the star shows grade attainment for children at birth. For all children this is equal to zero. (We put grade attainment of zero at a point above the origin to make simply to make the graph easier to read). Children who enroll at birth attain "A" grades of schooling by age 10 (the triangle) while children enrolled at age six attain "B" grades of schooling by this age. A single difference impact of early (at birth) v late (age six) enrollment equals A – B. But given that all children begin with zero grades of schooling, this is equivalent to the double difference estimate, AC – BC. In this way – for these outcomes – we recover our double-difference estimator.

5. Results: Schooling

Principal caregivers provided their child's schooling history. Specifically, they were asked to complete a timeline, starting with Grade 7 / Standard 5 and working backwards in time.[7] Using these data, for children no longer in school, we calculate the highest grade of schooling attained; for children still in school, the highest grade attained is the current grade they are enrolled in. Mean grade attainment is 4.02 grades. The mean is slightly higher for girls (4.15) than for boys (3.89). Table 3 shows the distribution of grade attainment by sex.

In this sample, the average child starts Grade 1 at age 5.8 years of age. There is no difference in age starting Grade 1 by whether or not a child had attended a crèche. Girls start slightly younger than boys, at age 5.73 years versus 5.86 years. To assess the extent to which children's entry into primary school is delayed, we begin by noting that the South African school

[7] During pilot testing, we experimented with working forwards from Reception and working backwards from Grade 7; respondents found it easier to recall these data if they worked backwards.

year begins in January. Children should enter Grade 1 at age five, turning six by 30 June in the year of admission and that enrollment can be delayed if a child is not considered ready to start school.[8] This implies that a child enrolling on time enters school at age five if she is born between January and June and enters school at age six if she is born between July and December. Table 4 shows the distribution of children entering early, on-time and late by sex. Girls were slightly more likely to start school early and boys slightly more likely to start school late. There is an association between delayed enrollment and grade attainment. On average, children who started early had, by the time of the survey, completed 4.5 grades. Children who had started on time had completed 4.2 grades while children whose entry was delayed had completed 3.7 grades.

Even in these early years of schooling, grade repetition occurs. Across grades one through four, 21.5 percent of children have repeated one grade and another 5.8 percent have repeated two or more grades. There is a pronounced gender difference with 32.6 percent of boys repeating at least one grade compared to 20.9 percent of girls. Mean grade repetition is 0.26 grades for girls and 0.43 grades for boys. Conditional on age at enrollment in Grade one, boys are always most likely to repeat at any grade level.

Table 5 disaggregates these outcomes by the child's age when the caregiver first received CSG payments for that child. There is a correlation between earlier receipt of the CSG and schooling attainments. Children in households where the grant was received when the child was very young appear to start school at a slightly younger age. There is no obvious difference in grade repetition when tabulated against duration of receipt of the CSG. The differences in schooling outcomes are slightly more pronounced when we restrict attention to girls (not shown).

We now turn to estimates of impact. As noted above, we match children across a wide range of characteristics.[9] Figure 2 shows the estimated dose-response function, and Table 6 shows the conditional expectations for grade attainment of the age of receipt of CSG on grade attainment. Standard errors are based on 100 bootstraps. In preliminary work, we

[8] See http://www.southafrica.info/services/education/edufacts.htm.

[9] As there are few observations of children who first received the CSG after age seven, we estimate the dose-response model for children first receiving the CSG at zero to six years of age.

experimented with higher numbers of bootstrap repetitions but even when we increased these to 500, standard errors tended to be only slightly smaller, within five percent of those reported here. Figure 2 shows that children enrolled at birth were predicted to complete 4.12 grades of schooling. Children enrolled at age six, just prior to starting school, were predicted to complete 3.98 grades; thus, children who were enrolled in the CSG at birth completed 0.14 more grades of schooling than children who were enrolled at age six. This impact is statistically significant.

Next, we disaggregate our sample by sex as shown in Figure 3 and Table 7. Enrollment at birth improved girls' grade attainment by one quarter grade compared to enrollment in the CSG at age six. This is a large impact given that most children in the sample had only completed four grades of schooling. By contrast, early CSG enrollment had no impact on boys' grade attainment.

Does the CSG complement or substitute for resources available within the household. We consider this question by assessing whether the impact of the CSG differed by maternal education. Table 8 shows that among children whose birth mothers have less than eight grades of schooling, early enrollment in the CSG raises grade attainment by 0.38 grades, a 10.2 percent increase. By contrast, the CSG has no impact on grade attainment on children whose mothers have eight or more grades of schooling. This indicates that the CSG is playing a compensatory role in narrowing the gap between children whose mothers have not completed primary school and mothers with at least some secondary education. This can also be seen by comparing the predicted outcomes across mothers' education attainments. For children enrolled at age six, the difference between predicted grade attainments is 0.41 (3.70 v 4.11), a difference of nearly half a grade. But for children enrolled at birth, the difference is negligible, 0.06 grades (4.08 v 4.14).

We also considered a number of other disaggregations including whether the current primary care giver was the child's biological mother, whether the child's father was present, the relationship of the child to the household head, and current residence (rural or not rural). For brevity, Table 9 reports only the estimated differences in predicted grade attainment between children enrolled at birth and at age six rather than the full set of dose-response estimates. Earlier first receipt of the CSG has similar impacts on children whose current caregivers are either their mothers or a non-mother and whether (or not) the child's father is present in the household. However, children who are not sons or daughters of the household head (for example, children of teen mothers who continue to live in their parents households) benefit more from early receipt of the CSG than children who are offspring of the household head.

Why does earlier receipt of the CSG affect grade attainment? We suggest three possible mechanisms: (a) by affecting the timing at which the child starts school; (b) by affecting the likelihood of grade repetition once they are in school; and (c) a combination of (a) and (b).[10] Table 10 examines how the likelihood of delayed entry is affected by early receipt of the CSG. Approximately 38 percent of children enrolled in the CSG at birth are predicted to be enrolled in school late as are 41 percent of children enrolled at age six. This difference is not statistically significant. When we disaggregate we find statistically significant impacts for two sub-groups, girls and children whose mothers have less than eight grades of schooling. Early receipt of the CSG reduces the likelihood of delayed entry by 12.5 and 14.8 percentage points respectively. Put another way, early receipt of the CSG reduces delayed school entry of girls by 26.5 percent and by 31.8 percent for children whose mothers have less than eight grades of schooling. By contrast, once children are enrolled in school, there is no additional impact of early CSG receipt on school progression. While Table 11 shows that girls and children whose mothers have little education are less likely to repeat when they were enrolled in the CSG at birth, the differences are not statistically significant.[11]

The survey instrument asked about attendance at pre-schools, nurseries and crèches. The majority of children in this sample, 59.9 percent, attended some sort of pre-school or crèche. Virtually all of these had toys, a toilet and provided a meal. Girls were slightly more likely to attend a crèche than boys, but boys tended to start attendance at a slightly earlier age. The modal age for first attendance is three years of age and few children start after age four.

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[10] Two additional pathways would be through the impact of the CSG on child labor or on study time. At this age, we find few children working for pay outside the household. In preliminary work, we explored the impact of duration of CSG receipt on time spent doing household chores and on studying. We found no statistically significant impact of early enrollment on these outcomes, further there were no statistically significant impacts when we disaggregated by sex or by maternal education.

[11] We also estimated dose-response models for the disaggregated samples listed in Table 9 but could not find evidence of statistically significant impacts of early enrollment in the CSG on the likelihood that the child started school late or the likelihood of grade repetition.

For the full sample, our dose-response models find no evidence that early receipt of the CSG affects attendance at crèches or nursery schools. There is weak evidence that it increases the duration of attendance by girls but this impact is not precisely measured.

6. Results: Learning

As part of the Young Child Questionnaire, children were administered the Early Grade Mathematics Assessment (EGMA), a battery of mathematics related tests developed by Reubens (2009). These include "oral counting" (count as high as you can in 30 seconds), number identification (point to a number and say what it is), number sequences (completes the following sequence, 23, …, 25), word problems (There are 8 children walking to school. Six are boys and the rest are girls. How many girls are walking to school?), addition and subtraction, shape recognition (circles, squares, triangles, rectangles) and pattern recognition. The questions used in our survey were selected to be consistent with what South African children in grades one to four would be expected to learn. The counting and number identification questions would have been most discerning for children in very early grades. We were aware of this when we implemented the EGMA but chose to include these as a means of gently introducing children to the test in a non-threatening manner. Table 12 provides mean scores and their standard deviations for children's responses on arithmetic, shape recognition, word problems and pattern recognition. Girls, on average, score slightly higher than boys and children whose mothers have more education score higher than children whose mothers have not completed primary school.

Table 13 provides some evidence that children who have been enrolled longer in the CSG score higher on the EGMA. The magnitude of this impact is small however, children at birth score 0.77 points or three percent higher than children enrolled at age six. This difference is not statistically significant. When we disaggregate by sex, we find that late enrolled girls score 2.05 points or 8.7 percent lower than early enrolled girls and this impact is statistically significant. There is no impact on boys or on children whose mothers have less than eight grades of schooling. Early enrollment boosts the EGMA scores of children who mothers have completed primary school but the effect is imprecisely measured. Table 13 also provides disaggregate impacts by test item for the largest components of the EGRA battery of tests, arithmetic and shape recognition. This shows that for the full sample and for children whose mothers have eight or more grades of schooling, late CSG enrollment (enrollment at age six) lowers arithmetic scores by 6.0 and 6.2 percent (-0.44 and -0.46 points) respectively compared to children enrolled at birth or in the first year of life. Both are statistically significant at the 10 percent level. There is a large impact on girls' arithmetic scores – early enrollment raises these by 0.72 points or 10.5 percent. This effect is statistically significant at the five percent level. There is no impact of early CSG enrollment on shape recognition.[12]

Children were also administered the Early Grade Reading Assessment (EGRA). EGRA documents early grade reading skills (Gove and Wetterberg, 2011). Like the EGMA, it consists of a battery of tests. Children begin by doing a timed reading of letters and familiar words. They are then asked to read the following passage and answer questions about it.

> Jabu had a dog. The dog was fat and happy. One day Jabu and the dog went out to play. The little dog ran away and got lost. Jabu was sad but after a while the dog came back. Jabu took the dog home. When they got inside the house Jabu gave the dog a bone. The little dog was tired, so he slept. When the dog woke up, Jabu took the dog outside again to play.

As with the EGMA, the first two components (the reading of letters and words) were administered to ensure that children were comfortable with the test. Here we assess whether early enrollment in the CSG affected the likelihood that children could read this passage and answer questions about it. Just under half of the same (47 percent) could read this passage in less than one minute. On average, children provided 2.8 correct answers to the five questions that tested their reading comprehension.

Table 14 shows the impact of early enrollment in the CSG on the predicted likelihood that a child could read this passage in less than one minute. As with many of the outcomes considered in this paper, there is no impact of early enrollment for the full sample. However,

[12] We also estimated dose-response models for the disaggregated samples listed in Table 9. There is some evidence that children who are not sons or daughters of the household head and who were enrolled at birth rather than at age six, do better on the arithmetic component of the EGMA. Their score improves by 0.73 with a standard error of 0.36. No other significant impacts of early versus late CSG enrollment were found for the other disaggregations.

when we disaggregate by sex, we find that enrollment at age six, as opposed to enrollment at birth, reduces the predicted likelihood that the child could finish this passage by 12.3 percentage points. We find no evidence that enrollment at birth improved scores on reading comprehension, though this may reflect the fact that as this test had only four questions, it may not have been sufficiently sensitive to pick up such effects.

7. Conclusion

This paper examines the impact of South Africa's Child Support Grant on a range of schooling outcomes. A unique feature of the CSG is that, as a child-centered grant, it is available to children from birth onwards. However, as take-up of the grant has been gradual, there are variations in grant receipt among children born in 2000. This allows us to estimate a dose-response model to compare the impact of being enrolled "early" in the CSG (at birth or in the first year of life) or "late" (age six, just before starting school). We find that children who were enrolled in the CSG at birth completed 0.14 more grades of schooling than children who were enrolled at age six. This impact is statistically significant.

Gender differences in impact are marked. Early CSG enrollment has positive impacts on many dimensions of girls' schooling and learning. Earlier enrollment in the CSG improved girls' grade attainment by one quarter grade compared to enrollment in the CSG at age six. This is a large impact given that most children in the sample had only completed four grades of schooling. The pathway through which this occurs is the reduction in delayed entry. Early receipt of the CSG reduces delayed school entry of girls by 26.5 percent. Girls who were enrolled early obtain higher marks on tests of mathematical ability and reading. By contrast, there are no impacts on boys' schooling or learning. The CSG plays a compensatory role in narrowing the gap between children whose mothers have not completed primary school and mothers with at least some secondary education. For children whose mothers have less than eight grades of schooling, early enrollment in the CSG raises grade attainment by 0.38 grades, a 10.2 percent increase while having no impact on grade attainment on children whose mothers have eight or more grades of schooling. We observe this effect because early receipt of CSG reduces the likelihood that children from these disadvantaged background are less likely to enroll late. These results provide evidence that unconditional cash transfers can, in an African setting, increase human capital formation and the acquisition of skills. As such, transfers such as the CSG have enormous potential to contribute to current poverty reduction while increasing the economic productivity of future generations.

Our study has weaknesses. Our impact estimates rely on the assumption that we have satisfied the unconfoundedness condition. Our use of a single age cohort means that we cannot assess whether the positive impacts observed here will persist as children grow older. We observe marked differences by sex but our data offer few clues as to why we observe these differences. Set against these weaknesses is the attention our study focuses on the timing within the child's lifecycle of these transfers and on their impacts on learning rather than just enrollment or attendance, issues that we perceive has received too little attention in the literature on social protection in developing countries. Further advances on these topics will require designing and evaluating interventions where these weaknesses can be remedied.

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Table 1: Distribution of children by age at first receipt of CSG

Age at first receipt of CSG	Number of children	Percent	Number of girls	Number of boys
0	281	23.7	138	143
1	362	30.5	180	182
2	108	9.1	52	56
3	63	5.3	24	39
4	44	3.7	19	25
5	97	8.2	51	46
6	82	6.9	34	48
7	76	6.4	39	37
8	43	3.6	20	23
9	26	2.2	15	11
10	5	0.4	3	2
Total	1,187	100.0	575	612

Source: Household survey

Table 2: Comparison of selected sample characteristics to those found in NIDS and GHS

	Our Sample	NIDS	GHS
Household size	6.4	6.2	6.3
Number of children < 18y	3.4	3.4	3.5
Percent with access to electricity	77%	72%	75%
Percent with corrugated iron/zinc roofing sheets	59%	63%	61%

Table 3: Highest grade attained by sex

Highest Grade Attained	Girls	Boys	All children
1	0.2%	0.5%	0.3%
2	2.2	4.5	3.4
3	14.5	23.0	18.9
4	50.5	49.7	50.1
5	30.3	21.1	25.5
6	2.3	0.9	1.6
Not stated	0.0	0.3	0.2

Source: Household survey

Table 4: Distribution of entry into Grade One by sex

Starts Grade One	Girls	Boys	All children
Early	11.8%	8.5%	10.1%
On-time	53.2	53.9	53.5
Late	35.0	37.6	36.4

Source: Household survey

Table 5: Schooling outcomes by child age at first receipt of CSG, all children

	Schooling outcomes			
Child age at first receipt of CSG (years)	Highest grade attained	Age started school (mean)	Number of grades repeated	Proportion of children
<1	4.13	5.75	0.31	0.26
1	4.04	5.77	0.35	0.26
2	3.92	5.80	0.41	0.32
3, 4	4.00	5.80	0.40	0.29
5, 6	3.95	5.84	0.40	0.29
7, 8, 9, 10	3.91	5.94	0.28	0.21
Total	4.02	5.80	0.35	0.27

Source: Household survey

Table 6: Dose response estimates of impact on grade attainment

Age at first receipt of CSG	Predicted Outcome	Standard error
0	4.12	0.04**
1	4.07	0.03**
2	3.99	0.04**
3	3.92	0.06**
4	3.90	0.06**
5	3.92	0.05**
6	3.98	0.05**
Difference between predicted outcomes at receipt at age zero and six	-0.14	0.06**

Sample size is 1091. *, significant at the 10% level; **, significant at the 5% level.

Table 7: Dose response estimates of impact on grade attainment by sex

	Girls		Boys	
Age at first receipt of CSG	Predicted Outcome	Standard error	Predicted Outcome	Standard error
0	4.27	0.05**	3.99	0.06**
1	4.21	0.04**	3.91	0.04**
2	4.15	0.05**	3.83	0.05**
3	4.10	0.07**	3.78	0.07**
4	4.06	0.08**	3.80	0.08**
5	4.04	0.07**	3.87	0.06**
6	4.02	0.08**	3.95	0.07**
Difference between predicted outcomes at receipt at age zero and six	-0.25	0.10**	-0.04	0.09

Predicted outcomes estimated for 529 girls and 562 boys. *, significant at the 10% level; **, significant at the 5% level.

Table 8: Dose response estimates of impact on grade attainment by maternal education

	Mothers have <8 grades schooling		Mothers have 8+ grades, schooling	
Age at first receipt of CSG	Predicted Outcome	Standard error	Predicted Outcome	Standard error
0	4.08	0.08**	4.14	0.06**
1	3.85	0.07**	4.14	0.04**
2	3.67	0.07**	4.10	0.05**
3	3.60	0.09**	4.05	0.07**
4	3.61	0.10**	4.03	0.07**
5	3.66	0.09**	4.05	0.06**
6	3.70	0.10**	4.11	0.06**
Difference between predicted outcomes at receipt at age zero and six	-0.38	0.12**	-0.03	0.07

Predicted outcomes estimated for 286 children whose mothers have <8 grades schooling and 805 children whose mothers have 8 or more grades schooling. *, significant at the 10% level; **, significant at the 5% level.

Table 9: Differences in predicted grade attainment for children receiving the CSG at birth and at age six by selected child and household characteristics

	Difference between predicted outcomes at receipt at age zero and six	Standard error	Number of observations
Primary care giver is child's mother	-0.14	0.08*	841
Primary care giver is not child's other	-0.14	0.17	250
Both biological parents present	-0.10	0.11	290
At least one biological parent is absent	-0.15	0.076**	801
Child is son or daughter of head	-0.01	0.09	617
Child is not son or daughter of head	-0.33	0.09**	474
Household is in rural locality	-0.28	0.12**	379
Household is in non-rural locality	-0.10	0.08	712

*, significant at the 10% level; **, significant at the 5% level.

Table 10: Dose response estimates of impact on probability of delayed enrollment by child sex and maternal education

	All children		Girls		Boys		Mothers <8 grades schooling		Mothers 8+ grades schooling	
Age at first receipt of CSG	Predicted Outcome	Standard error	Predicted Outcome	Standard error	Predicted Outcome	Standard error	Predicted Outcome	Standard error	Predicted Outcome	Standard error
0	0.378	0.029**	0.347	0.041**	0.397	0.033**	0.317	0.05**	0.401	0.03
6	0.413	0.028**	0.472	0.046**	0.351	0.046**	0.465	0.06**	0.386	0.04
Difference between predicted outcomes at receipt at age zero and six	0.036	0.040	0.125	0.057**	-0.046	0.093	0.148	0.08*	-0.015	0.050

*, significant at the 10% level; **, significant at the 5% level.

Table 11: Dose response estimates of impact on probability of grade repetition by child sex and maternal education

	All children		Girls		Boys		Mothers <8 grades schooling		Mothers 8+ grades schooling	
Age at first receipt of CSG	Predicted Outcome	Standard error	Predicted Outcome	Standard error	Predicted Outcome	Standard error	Predicted Outcome	Standard error	Predicted Outcome	Standard error
0	0.275	0.024**	0.202	0.035**	0.325	0.041**	0.327	0.043**	0.256	0.029
6	0.285	0.027**	0.279	0.034**	0.288	0.041**	0.407	0.054**	0.227	0.029
Difference between predicted outcomes at receipt at age zero and six	0.009	0.041	0.077	0.050	-0.036	0.056	0.080	0.073	-0.029	0.047

*, significant at the 10% level; **, significant at the 5% level.

Table 12: Scores on selected components of the EGMA by sex and maternal schooling

				Mother's schooling		
	All	Boys	Girls	< 8 grade	8+ grades	Maximum score
Arithmetic	7.11 (3.07)	6.96 (3.11)	7.27 (3.01)	6.56 (3.22)	7.30 (2.99)	10
Shape recognition	10.85 (4.02)	10.64 (4.09)	11.08 (3.93)	10.42 (4.42)	11.00 (3.87)	14
Word problems	3.05 (1.28)	3.00 (1.31)	3.11 (1.24)	2.94 (1.36)	3.09 (1.25)	4
Pattern recognition	3.74 (1.62)	3.64 (1.65)	3.84 (1.59)	3.48 (1.80)	3.79 (1.60)	5
Total across all tests	24.50 (9.05)	24.03 (9.14)	25.01 (8.93)	23.11 (9.61)	24.95 (8.82)	33

Source: Calculated from household survey. Standard deviations in parentheses

Table 13: Dose response estimates of impact on EGMA score and selected subcomponents by child sex and maternal education

	EGMA									
	All children		Girls		Boys		Mothers <8 grades schooling		Mothers 8+ grades schooling	
Age at first receipt of CSG	Predicted Outcome	Standard error	Predicted Outcome	Standard error	Predicted Outcome	Standard error	Predicted Outcome	Standard error	Predicted Outcome	Standard error
0	25.20	0.47**	25.68	0.57**	24.70	0.53**	24.17	0.98**	25.50	0.49**
6	24.43	0.58**	23.63	0.92**	25.18	0.65**	23.76	0.82**	24.54	0.77**
Difference between predicted outcomes at receipt at age zero and six	-0.77	0.71	-2.05	1.09*	0.48	0.86	-0.40	1.26	-0.96	0.87
	EGMA Arithmetic Score									
0	7.50	0.17**	7.61	0.20**	7.32	0.16**	7.12	0.34**	7.64	0.16**
6	7.06	0.18**	6.88	0.32**	7.20	0.20**	6.71	0.34**	7.18	0.22**
Difference between predicted outcomes at receipt at age zero and six	-0.44	0.25*	-0.72	0.36**	-0.12	0.30	-0.41	0.41	-0.46	0.28*
	EGMA Shape Recognition									
0	10.88	0.21**	11.01	0.30**	10.78	0.30**	10.34	0.49**	11.04	0.19**
6	10.84	0.24**	10.31	0.44**	11.36	0.26**	10.67	0.60**	10.82	0.32**
Difference between predicted outcomes at receipt at age zero and six	-0.04	0.31	-0.70	0.52	0.58	0.41	0.33	0.67	-0.22	0.40

*, significant at the 10% level; **, significant at the 5% level.

Table 14: Dose response estimates of impact on likelihood of complete reading of story and reading comprehension by child sex and maternal education

	Predicted likelihood of reading the story									
	All children		Girls		Boys		Mothers <8 grades schooling		Mothers 8+ grades schooling	
Age at first receipt of CSG	Predicted Outcome	Standard error	Predicted Outcome	Standard error	Predicted Outcome	Standard error	Predicted Outcome	Standard error	Predicted Outcome	Standard error
0	0.490	0.027	0.545	0.036	0.417	0.046	0.479	0.055	0.497	0.038
6	0.463	0.030	0.422	0.055	0.487	0.047	0.412	0.055	0.479	0.035
Difference between predicted outcomes at receipt at age zero and six	-0.026	0.043	-0.123	0.060**	0.070	0.059	-0.067	0.076	-0.017	0.049
	Reading comprehension									
0	2.95	0.09	3.12	0.13	2.78	0.13	2.66	0.22	3.05	0.13
6	2.87	0.12	2.86	0.19	2.90	0.19	2.27	0.21	3.13	0.13
Difference between predicted outcomes at receipt at age zero and six	-0.08	0.14	-0.26	0.22	0.12	0.19	-0.39	0.25	0.08	0.15

*, significant at the 10% level; **, significant at the 5% level; *** significant at the 1% level.

Figure 1: Recovering double difference estimates from a single cross-section

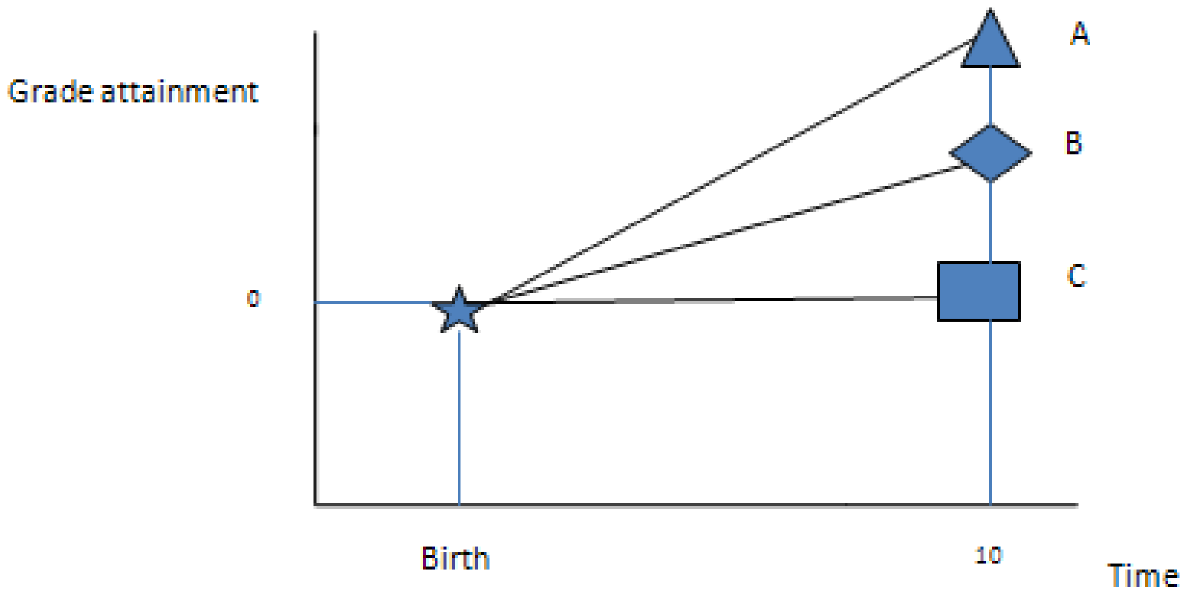


Figure 2: Dose-response graph of impact of age at first receipt of CSG on grade attainment

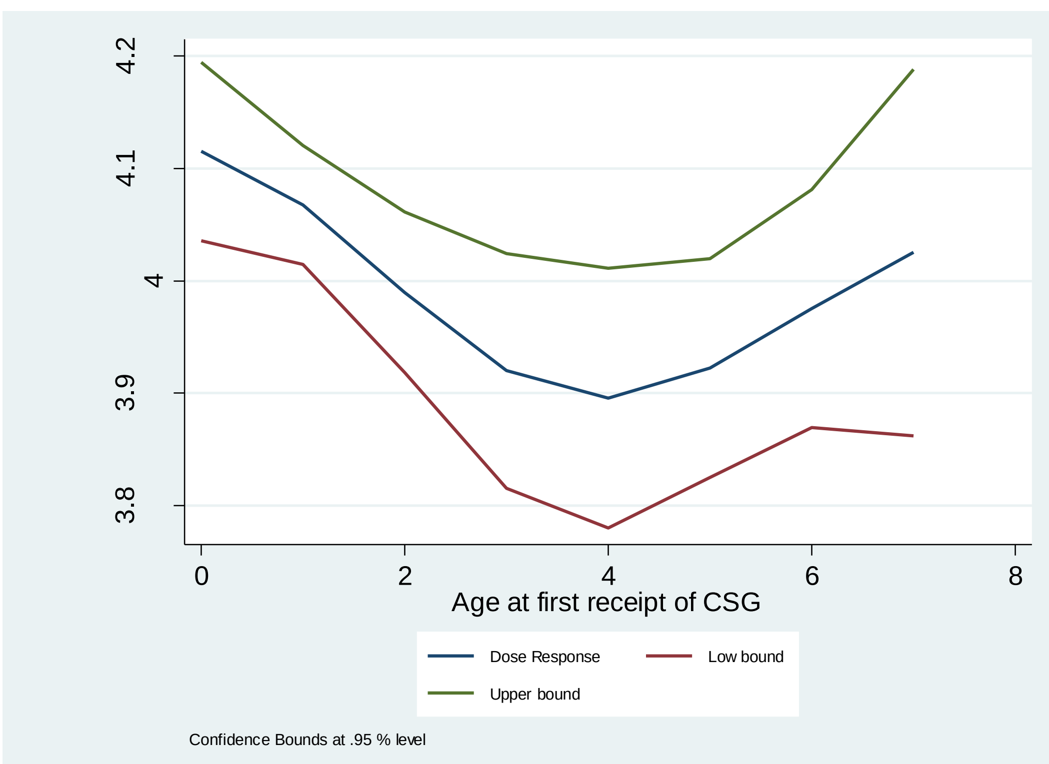


Figure 3a: Dose-response graph of impact of age at first receipt of CSG on grade attainment by sex, girls

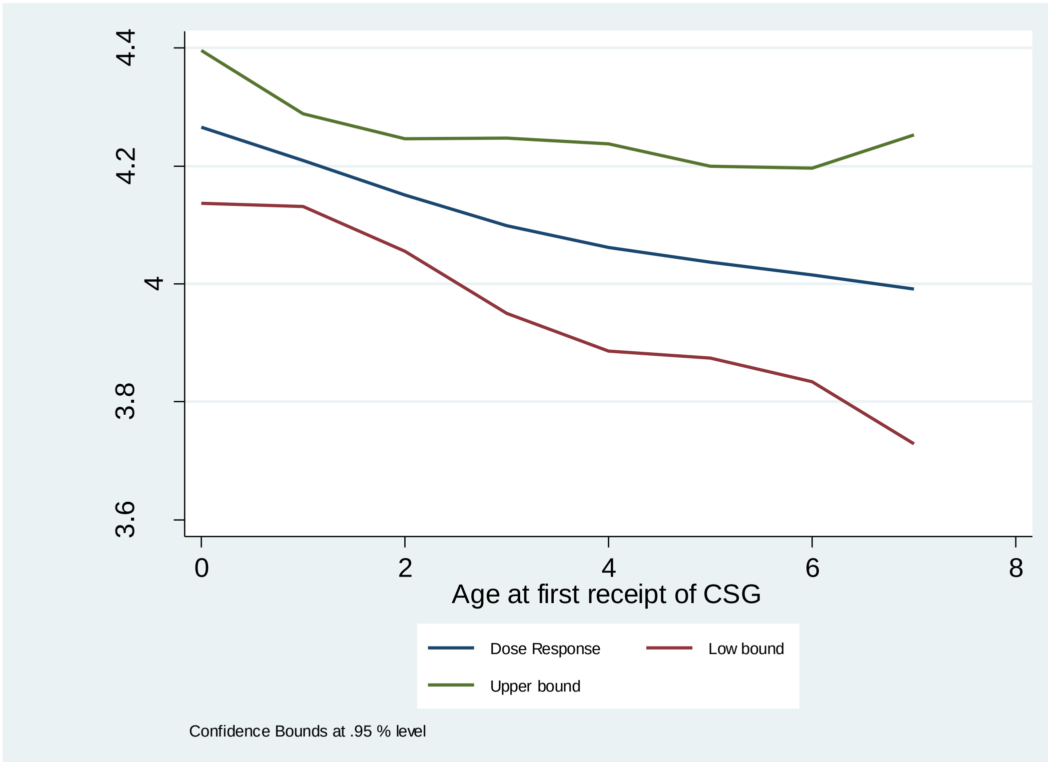
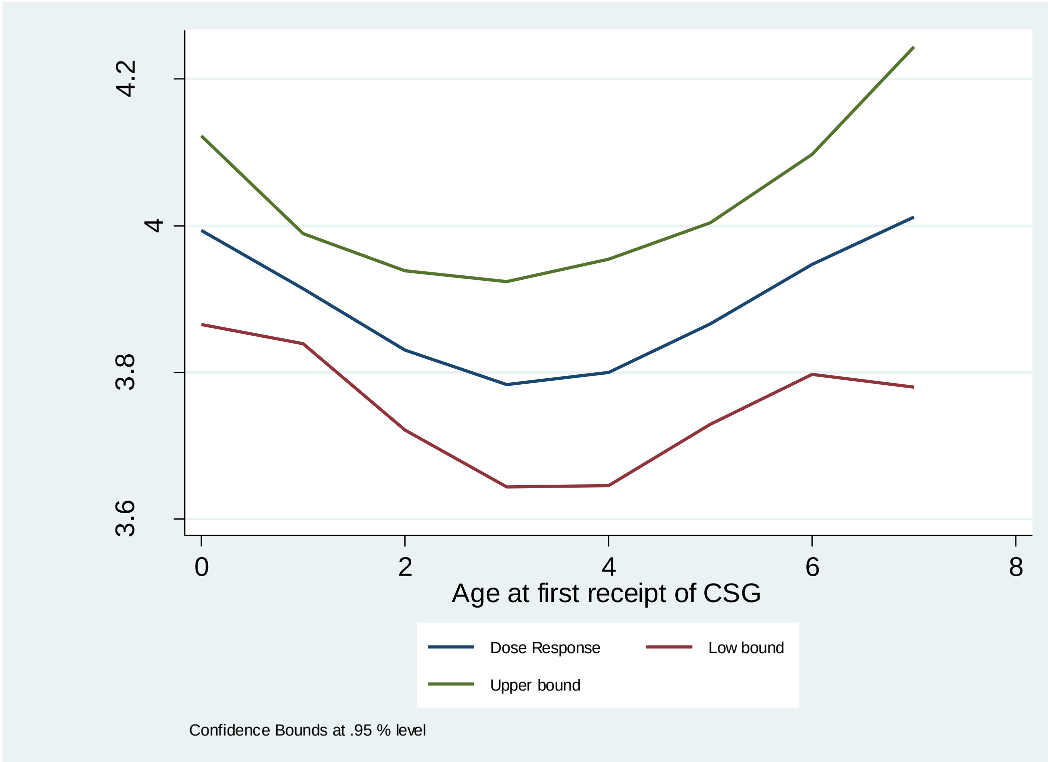


Figure 3b: Dose-response graph of impact of age at first receipt of CSG on grade attainment by sex, boys



Appendix: Correlates of generalized propensity score

	Covariate	Coefficient	Standard Error	Z statistic
Child characteristics	Son or daughter or head	0.205	0.244	0.84
	Grandchild of head	-0.183	0.260	-0.71
	African	-0.538	0.263	-2.05
Maternal characteristics	Age at birth	-0.180	0.049	-3.65
	Age squared	0.002	0.0007	2.90
	Age known	0.266	0.463	0.58
	Grades of schooling	-0.068	0.023	-2.89
	Schooling known	-0.848	0.427	-1.99
Household wealth at time of birth	House had tile or metal roof	0.168	0.302	0.56
	House had electricity	0.0386	0.164	0.24
Access to grants	Mother received CSG application when child born	-0.363	0.239	-1.52
	Other household member receives old age grant	-0.100	0.207	-0.48
	Other child in household receives CSG	0.135	0.142	0.95
Location	Child born in KwaZulu-Natal	-0.247	0.188	-1.31
	Child born in Eastern Cape	-0.467	0.229	-2.03
	Child born in Western Cape	-0.491	0.235	-2.09
	Child born in Limpopo	-0.928	0.261	-3.55
	Travel to time to nearest urban centre when child born	0.047	0.121	0.39
Shocks*	Death of household member	-0.022	0.204	-0.11

	Death of family member or friend who had provided financial support	-0.203	0.323	-0.63
	Illness that prevented household member from working	0.292	0.455	0.64
	Job loss of household member	0.243	0.393	0.62
	Loss of remittances	-0.212	0.633	-0.33
	Serious family conflict	0.218	0.621	0.35
	Abandonment or divorce	0.706	0.449	1.57
	Constant	7.067	0.895	7.89

* Shocks are defined as follows: IN THE LAST TEN YEARS, since [NAME] was born. Has this household been affected by a serious **economic** shock—an event that led to a serious reduction in your asset holdings, caused your household income to fall substantially or resulted in a significant reduction in consumption?