

# The Lasting Labor-Market Effects of Cash Transfers: Evidence from South Africa's *Child Support Grant*

Alessandro Tondini

## Abstract

Can unconditional cash transfers have long-term benefits for women's employment in developing countries? This study exploits discontinuous exposure to the South African Child Support Grant for mothers whose children were born one year apart to identify the short- and long-term effects of a positive income shock of roughly \$400 (\$650 PPP in 2010). In the short term, there is a considerable increase in the probability of being active and looking for a job. Five years after receiving the transfer, mothers who benefited for one year are as likely to be employed as those who never received it; the type of occupation is also similar, other than a small decrease in work in the agricultural sector. Overall, the grant appears to facilitate job search for single mothers in the presence of high search costs, but does not significantly change job prospects.

**JEL classification:** O12, H53, I38, J64, J22, J4

**Keywords:** unconditional cash transfers, job quality, unemployment, South Africa

## 1. Introduction

Improving access to good jobs is a key issue in the labor market of developing countries, where a significant portion of the workforce is often employed in low-quality, subsistence-level jobs. The broad concept of “job quality” is then particularly relevant, as simple measures of employment (and unemployment) may be a poor indication of labor-market performance. In these contexts, a fundamental question is to what extent unemployment and low-quality jobs are the result of frictions and barriers in the labor market, which could be addressed by specific policies. For example, the presence of search frictions, as a result of high search costs, would suggest that an income shock that relaxes liquidity constraints for the unemployed may result in persistent benefits in both the quantity and quality of jobs. This could be achieved by cash transfers, a widespread policy instrument in developing countries. However, empirical evidence on whether these types of programs can lead to lasting improvements in the labor-market outcomes of adult recipients is still very limited.

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In order to shed more light on the topic, this paper analyzes the labor-market effects of a child grant in a dynamic way. This form of cash transfer is very common in developing countries, yet the South African Child Support Grant (CSG) has some unique features that make its analysis particularly informative. This policy provides a pure income shock, without introducing incentives or conditionalities that might complicate the interpretation of the effect, and is therefore informative of the potentially lasting positive effects of small to medium income shocks more generally. At the same time, its labeling, targeting, and the size of its income component are comparable to those of many similar policies across the developing world. Women's employment outcomes are particularly dire in South Africa, with low employment and high unemployment, which speaks to the salience of the analysis of this type of program.

The identification strategy exploits exogenous variation in eligibility for the grant across children's birth cohorts, which results from sudden reforms in the age requirement necessary to access the grant. This estimation recovers the labor-market effects on mothers who received roughly one year of the grant,<sup>1</sup> both during the period of receipt and after the grant has stopped. As this specific shock occurs when the child is already quite old, direct effects on children are likely to be very limited. The results show that recipient mothers are significantly more likely to be active and to look for a job after receiving the transfer, with no evidence of disincentive effects on work. Individuals who respond most are the ones likely to be the most liquidity constrained: single mothers in households without a husband or partner. Five years after the grant has stopped, the employment rate is still the same between mothers who have received the CSG and those who have not; the composition of employment is also similar, other than that single mothers are less likely to be working in elementary occupations in the agricultural sector as a result of the transfer. The effect on overall income is far from statistically significant. Due to data constraints, the minimum detectable effect is well above what could realistically be identified with the data at hand.

The interaction of social programs and job search is at the heart of labor and public economics. Many studies have examined the impact of severance pay or unemployment benefits on unemployment duration, and the quality of the subsequent job (Card, Chetty, and Weber 2007; Nekoei and Weber 2017). In the context of developed countries, while there is virtually unanimous empirical evidence that more generous assistance during job search usually increases unemployment duration, the evidence on the effect on job quality is more diverse.<sup>2</sup> Moreover, this literature has largely ignored the context of developing countries, where the salience of this question is amplified by the important share of subsistence-level jobs within the labor market. This may be because the programs usually analyzed for this type of evaluation, such as unemployment benefits or severance pay, are often less widespread in the context of the developing world.<sup>3</sup> Nonetheless, whether an exogenous cash transfer can raise future job quality is a key question with important policy implications. As Card, Chetty, and Weber (2007) say, "Testing this prediction sheds light on whether improvements in future job outcomes provide a rationale for temporary income support programs."<sup>4</sup> The paper tests this prediction within the context of unconditional cash transfers targeted at mothers, who, suddenly and unexpectedly, become eligible for one year of grant.

Despite the extensive literature on cash transfers, the labor-market effects on adult recipients are still poorly understood, in particular for child grants targeted at women. Overall, contrary to what a canonical model would suggest, the literature shows little evidence of a leisure effect of cash transfers, meaning that disincentive effects on work appear to be minor and concentrated on specific populations (see Banerjee et al. (2017), Baird, McKenzie, and Özler (2018) for a summary). However, while the long-term effects of

1 This is roughly equivalent to \$400 in 2010, or \$650 in PPP terms.

2 See Nekoei and Weber (2017) for a detailed summary of the empirical evidence.

3 One notable exception is Gerard and Gonzaga (2021), who study how informality in Brazil impacts the efficiency cost of unemployment insurance, but do not look at subsequent job quality.

4 Improvements in labor-market outcomes of recipient mothers is certainly not the main policy justification behind child support grants. Yet this is an important aspect that cannot be overlooked, in particular for cash transfers that are targeted at children only in name, without any actual conditionalities attached.

cash transfers on children have received significant attention (see Millán et al. (2019) for a review), similar evidence for adults is rare (Baird, McKenzie, and Özler 2018).<sup>5</sup> The main contribution of this paper is to show that an unconditional cash transfer can facilitate job search for those most vulnerable portions of the population, but also that, on its own, this translates into relatively minor lasting effects on their labor-market outcomes.

From a policy standpoint, understanding whether medium-size income shocks can facilitate job search and lead to persistent job quality gains is key, both as an additional rationale for cash-transfer policies and, more generally, to know whether relaxing liquidity constraints can be a way to better labor-market outcomes for recipients. This is not obvious. If there are barriers other than financial constraints (such as low human capital, for example) that prevent access to employment and “good” jobs, then the labor-market effects of a positive income shock, at least through the job search channel, should be null or small. This is indeed what our results seem to show: despite a strong, positive, short-term effect on the probability of being active and looking for a job, the persistent effect on the probability of being employed is null, and, as an effect on job quality, we observe only a relatively small drop in agricultural employment. This seems to indicate that other barriers might be binding when it comes to unemployment or low-quality employment, which is a finding consistent with recent research in other African countries as well as South Africa.

The paper proceeds as follows: the second section introduces the institutional context and various reforms. The data used and descriptive statistics are found in the third section. The fourth section presents the empirical strategy and the results. Mechanisms and external validity are discussed in the fifth section. The sixth section concludes.

## 2. The South African Child Support Grant

The CSG is the largest social program in South Africa in terms of participants, with a number that reached around 10 million children in 2010 (roughly 20 percent of the population); it is also the second largest program in terms of government spending (Gomersall 2013), second to the old age public pension. First implemented in April 1998 in post-apartheid South Africa with the aim of reducing poverty and inequality, it is generally considered to be the main anti-poverty policy of the South African government. The other two main social grants are the Disability Grant and the Old Age Pension, which cover either individuals who cannot work or who have reached pension age without a private pension.<sup>6</sup>

The CSG is an unconditional, means-tested, cash-transfer program, where the only eligibility requirements are (a) having a child below a given age and (b) an income below a certain threshold. At the end of the month in which the child surpasses the age threshold, the grant is no longer paid. The CSG is paid per child, with no limitation on the number of grants a person can receive. Obtaining access to the grant requires very few documents: an identity card, the child’s birth certificate, and proof of earnings (this last requirement is flexible, as discussed in the next paragraphs). The grant is paid to the “primary caregiver” of the child; hence, it is not exclusive to the parents. In practice, the CSG is paid out almost exclusively to women, and the biological mother of the child is the direct recipient a large majority of the time (Eyal and Woolard 2011). The Black and Coloured<sup>7</sup> population is disproportionately represented among CSG recipients, while less than 1 percent of recipients are White.

5 Bianchi and Bobba (2013) find no long-term effects on the total population of a cash-transfer program in Mexico on the probability of self-employment. In a recent extension to Haushofer and Shapiro (2016), the authors have shown that an unconditional cash transfer can have a persistent positive effect on assets up to two years later.

6 Coverage of these other grants at the household level is presented in fig. S1.1 in the supplementary online appendix, and does not vary significantly over time.

7 Throughout the paper, we always use Black and Coloured with capital letters to indicate the statistical classification present in all of South African data.

**Table 1.** Evolution of the Child Support Grant

Reform dates	Age limit	Amount (R)	Amount (*10 R)	Means test (R)
Q2 1998	7	100	185	1,100 rural, 800 urban
Q2 2003	9	160	218	1,100 rural, 800 urban
Q2 2004	11	170	234	1,100 rural, 800 urban
Q2 2005	14	180	242	1,100 rural, 800 urban
Q3 2008	14	230	257	2,300
Q1 2009	15	240	250	2,400
Q1 2010	16	250	250	2,500
Q1 2011	17	260	248	2,600
Q1 2012	18	280	252	2,800

Source: Gomersall (2013) and Eyal and Woolard (2013).

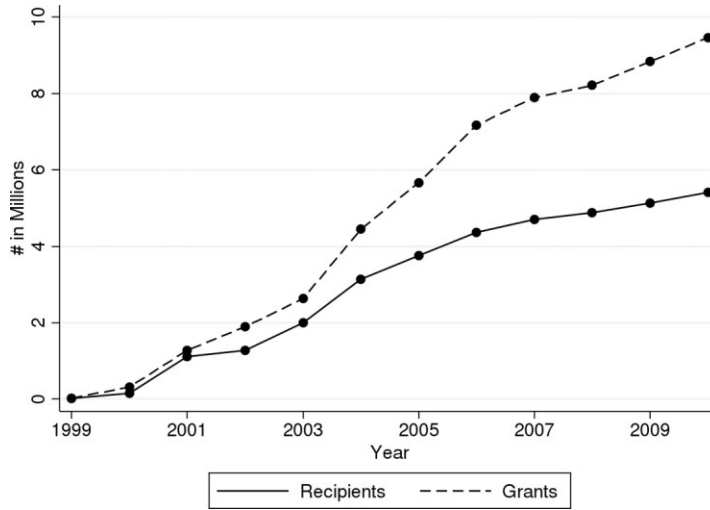
Note: The grant was introduced in April 1998. Column (4) gives the value of the grant in 2010 rand, adjusting for inflation measured as Consumer Price Index (Source: OECD.stat). The means test was fixed until 2008, when it was then set at 10 times the grant for individuals and 20 times the grant for married couples.

Table 1 shows the date of introduction of the CSG, the amount of the grant in nominal and real terms, the level of the means test, and the reforms in age eligibility. The amount of the grant is generally considered to be small (Lund 2007), especially when compared to the less extended but more generous disability and pension grants. However, this does not seem to be true by either national or international standards. The size of the CSG is significant when compared to median earnings, as discussed later. The average amount per child over the period is around \$50, 2010 PPP per month, and its 1998 amount is comparable to that of Progresá in Mexico for the same year (Bianchi and Bobba 2013). The means test of the grant was initially set at R 1,100 in rural areas and R 800 in urban areas, and was kept fixed for the entire period from 1998 to 2008. Finally, in 2008, the means test was harmonized—to 10 times the grant for individual incomes, and to 20 times the grant for the pooled income of the caregiver and their spouse—and it has not changed since.

While the age threshold is strictly applied through the birth certificate requirement, the means test has not always been. Lund (2007), head of the committee behind the creation of this program, states clearly that since its inception the means test was put in place to discourage applications from richer individuals, rather than as a strict threshold. In line with this, there is very little evidence of its application, in particular before 2008. Qualitative research has found that “the various elements of the means test are not generally enforced, understood or relevant” and that “an affidavit stating that the ‘primary care-giver’ and his or her spouse are not earning an income above the means test threshold will generally suffice” (Goldblatt, Rosa, and Hall 2008), which make this constraint de facto non-binding.<sup>8</sup>

Figure 1 plots the number of grants and recipients between 1999 and 2010. Despite being officially introduced in April 1998, the CSG took some time to be fully implemented, a result of administrative difficulties and overall confusion, but also of a lack of political will to truly implement the grant as it was intended (Lund 2007). The number of grants paid out was close to zero before 2001, when the number of grants and recipients began increasing dramatically. The steepest increase in coverage occurs from 2003, when the age-eligibility threshold doubled in only two years, from 7 to 14. These fast-paced reforms rendered more children eligible for the grant. The age threshold was stable from 2005 to 2009, and then gradually increased from 14 to 18 on 1 January of every year. Contrary to the previous increase,

8 Further evidence of the less than strict application of the means test is that, prior to 2008, it was significantly more binding in urban areas and for married couples, as, in theory, the pooled income of both spouses enters the computation. After 2008, when the means test was equalized across urban/rural regions and doubled for married couples, we might have expected to observe very different evolutions for these subgroups, but that is not the case (fig. S1.2). The four groups who experienced different changes in the nominal means test are urban married (+475 percent), urban non-married (+190 percent), rural married (+320 percent), and rural non-married (+110 percent).

**Figure 1.** Number of CSG Recipients, South Africa, 1999–2010

Source: This series is from [Gomersall \(2013\)](#), who uses administrative data provided by the Social Security Agency.

Note: This graph plots the number of Child Support Grant recipients, i.e., how many people receive at least one grant, and the total number of grants, both in millions. Mothers can receive one grant for each child who is eligible.

these reforms only increased the duration for cohorts that were already receiving the grant. The massive means-test reform that occurred at the end of 2008 does not seem to have led to a discontinuous jump in take-up, which increased smoothly from 2006 and then stabilized around 2010. This is consistent with the reports that the means test was not strictly applied, and that age-eligibility was de facto the only binding criterion.

There is a lack of convincing evidence on the lasting impacts of the CSG, mostly due to difficulties in setting up a robust empirical strategy to capture its effects. Several attempts have been made to look at how the CSG affects children's education and health: [Coetzee \(2013\)](#), [Eyal and Woolard \(2013\)](#) find positive but limited effects on children's outcomes. The absence of significant effects on children might be the result of the lack of conditionalities attached to the grant, but further research is needed. A recent paper looks at the impact on food security at the household level ([d'Agostino, Scarlato, and Napolitano 2018](#)). A few papers have begun to investigate the effects of this program on the labor-market outcomes of the parents: [Eyal and Woolard \(2011\)](#), [OECD \(2011\)](#) find some evidence that the CSG might increase employment by comparing mothers of children of a given age, before and after the reforms. On the contrary, [Bengtsson \(2012\)](#) compares the marginal effect on earnings of having a child in the household before and after the CSG was implemented. He finds that CSG receipt lowers the marginal propensity to earn (through lower labor supply) and increases consumption and expenditure. [Berg \(2013\)](#) looks at how households respond in terms of expenditure when the grant lapses, finding no decrease in expenditure when the child reaches the age eligibility threshold. More attention has been given to the labor-market effects of other cash-transfer programs in South Africa, such as the Old Age Pension ([Ardington, Case, and Hosegood 2009](#); [Abel 2019](#)), but evidence on the labor-supply effects of working-age adults remains mixed.<sup>9</sup> Overall, these studies have focused on the immediate, short-term responses and have not been

9 The Old Age Pension is a public pension system in South Africa that provides an unconditional cash transfer, paid to individuals of a certain age regardless of previous pension contributions. [Ardington, Case, and Hosegood \(2009\)](#) find that, contrary to what previous cross-sectional analyses had suggested, the positive income shock that occurs when an older member of the household reaches pension age leads to a significant increase in employment for working-age

able to estimate whether there are effects potentially lasting beyond the immediate receipt of the transfer, which is what this paper tries to address.

### 3. Data and Descriptive Statistics

#### 3.1. Data

This paper combines several data sources to study the labor-market impact of the CSG in detail. The main part of the analysis is conducted on census data (2001 and 2011) and the Community Survey (2007). For simplicity, we refer to both as census data, as they are, by design, highly comparable and have large sample sizes.<sup>10</sup> Both also contain questions on the date of birth of the youngest child, which gives us information on CSG exposure regardless of whether or not the child is observed in the household. This is particularly important following research by [Klasen and Woolard \(2009\)](#), among others, who show how household composition in South Africa is potentially endogenous to the receipt of social grants. Labor-market outcomes in the census are limited to employment status, and detailed information on occupation and sector of work. Primary outcomes such as activity, employment, and unemployment are roughly comparable across the three data points: the reference periods for employment (last seven days) and unemployment (last four weeks) are the same in all years, while the wording of the relevant questions changes slightly.<sup>11</sup> The level of detail of the occupation/industry information varies slightly across the three sources: this will affect the analysis by job type, as not all occupational categories are available in all the years, as we will see later. Wage information is lacking, and income is recorded as a categorical variable. Despite its limitations, census data are the only source with information on fertility, as it collects the date of the latest birth, which is key to (a) comparing the same population over time and (b) to clearly identifying which mothers are treated. It also has a large enough sample to realistically capture the causal impact of the grant.

Census data have no information on CSG receipt. To document the first stage, i.e., how some mothers are exposed to the grant and some are not, another source of data, the General Household Survey (GHS), is exploited. This is an annual survey, conducted in July, which collects information for a large sample of South African households. It contains information on the various social grants, generic questions on employment, and many other socio-demographics. Relative to census data, it lacks the sample size and information on both the date of birth and fertility to make it suited for the estimation of the labor-market effects of the grant.

##### 3.1.1. Cohort of Birth

A key piece of information for this analysis is to know when a child is born, as this is what determines treatment. Census data have very precise information on the date of birth of the youngest child (year, month, and day). However, the GHS, which is essential to estimate actual take-up of the grant, does not contain precise information on when a child is born; the only information available is the age of the child in discrete years. With this information, one can pinpoint when a child is born by measuring cohort = year – age. Taking into account that the GHS always takes place in July, this subtraction will

individuals in the household. However, more recent work by [Abel \(2019\)](#), using nationally representative panel data, does not confirm these results, and finds again a negative result on employment of working-age adults.

- 10 The stated purpose of the Community Survey is to collect information on a large sample of the population between two census waves, which are 10 years apart. This makes the questionnaires very similar and comparable. The publicly available census waves are a 10 percent subsample of the overall census in the years 2001 and 2011; the Community Survey is a large survey of roughly 2 percent of the population in 2007.
- 11 The Community Survey in 2007 and the 2011 census have a very similar set of questions to measure employment, while these are slightly different in 2001, as respondents are asked directly whether they have worked for wage, profit, or in-kind/family gain in the past week.

give a cohort  $t$  that includes those born between July of year  $t - 1$  and June of year  $t$ . For example, a child that is age 7 in July 2002 will be in cohort 1995, hence of those born between July 1994 and June 1995. To compare cohorts in the two sources of data (census and GHS), the precise birth date in the census data are used to exactly match the cohorts in the GHS, i.e., from the middle of the previous year to the middle of the next. Throughout the analysis, with this adjustment, cohorts are always measured in the same way between the census and the GHS.

### 3.2. Descriptive Statistics

In the decade that we study (2001–2011), the employment rate of women increased from 34 percent to 40 percent, while unemployment fell from 29 percent to around 19 percent, but still remained at persistently high levels. The picture looks grimmer when focusing on the sample of interest, Black and Coloured mothers, the women who receive the CSG. For this subgroup, the unemployment rate has always been above 20 percent and peaked at more than 30 percent in some years. Self-employment is generally very rare in South Africa, and private households remain one of the main employers of Black and Coloured women, mostly as domestic workers. Work in the agricultural sector and subsistence agriculture is also not very frequent. In terms of the composition of female employment, wage work accounts for roughly 60 percent, 10 percent are self-employed, 20 percent are domestic workers, i.e., cleaners in private households, and less than 10 percent are in agricultural work. This mostly takes the form of elementary occupations in the agricultural sector, rather than subsistence agriculture in private households, which is only a small share. In South Africa, agricultural work, domestic work and self-employment are all generally considered subsistence-level occupations relative to wage employment: in the census data, people in wage employment report significantly higher income than the other three categories (which are roughly comparable in terms of income): on average, women in wage jobs report income twice as large as people in those other occupations.

In proportion to monthly wages, one CSG grant is comparable to 15–20 percent of the median wage for the population of Black and Coloured women, which is around R 1,500 in 2010. This shows how the amount of the CSG cannot be considered small, in particular relative to returns to employment for mothers. However, it also points out that one CSG grant is not sufficiently large to substitute for labor income at the extensive margin: a very small portion of workers earns less than one CSG grant (less than 7 percent of employed women in 2010). These orders of magnitude are key to interpreting the results presented in the next section, and to explain why, for example, we are unlikely to observe a negative employment effect at the extensive margin.<sup>12</sup>

## 4. Empirical Analysis

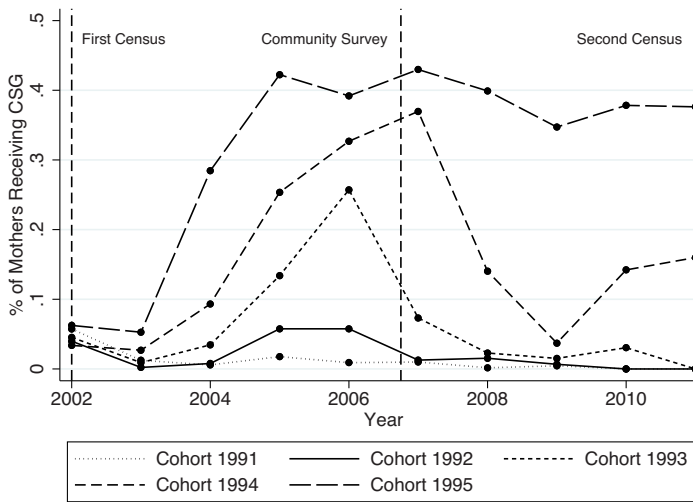
### 4.1. First Stage

Despite its many appealing features, putting in place a sound identification strategy for the CSG is not an easy task, as shown by the relatively little attention this massive program has received in the literature. However, the age eligibility criteria and its reforms provide a valuable source of variation to evaluate the effects of the CSG. In particular, this is the case for the reforms between 2003 and 2005, when the threshold was doubled from 7 to 14 in only two years (table 1). These reforms are key for the analysis as they make some cohorts eligible for the grant that had never been eligible before. The effect of these changes in the age threshold is clearly visible in fig. 2, where the share of mothers receiving the grant suddenly spikes for some cohorts but not for others.<sup>13</sup> This is the result of changes in age eligibility for

12 This pertains to the specific one-year exposure to the grant we focus on. It does not mean that several CSG grants, which can be accumulated for mothers with multiple children, do not have disincentive effects on work.

13 As mentioned before, cohorts are measured as in the GHS, hence from the middle of one year to the next. Here, cohort 1993 means mothers whose child was born between July 1992 and June 1993.

Figure 2. CSG Take-Up by Cohort of Birth, 2002–2011



Source: Author’s calculations on the General Household Survey (GHS).

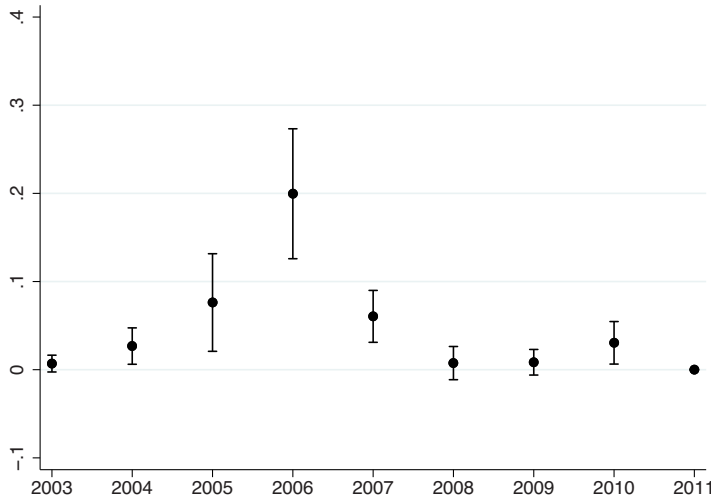
Note: This graph gives the share of mothers receiving the Child Support Grant (CSG) by cohort of birth of the youngest child for the period 2002–2011. Cohorts include children born between July of the previous year and June of a given year; see text for explanation. Overall, take-up of the CSG was virtually zero before 2002 (see fig. 1). The data point in 2002 is different from the rest of the series, because in that year CSG receipt was asked at the household level and not individually.

the grant: as can be seen from fig. S2.1 in the supplementary online appendix, these spikes in take-up occur for those cohorts young enough to be below the threshold. For example, we observe that cohorts 1990 and 1991 are never eligible, the children are too old when the threshold is reformed, and as a consequence those mothers never receive the grant. Cohort 1992 (July 1991 to June 1992) is eligible for a few months in 2005, but in practice the share of mothers receiving the CSG is always close to zero. Instead, cohort 1993 (July 1992 to June 1993) is eligible for part of 2005 and 2006. For this cohort, the take-up of the grant spikes significantly, and then drops as the child loses eligibility the year after. Mothers whose youngest child was born later are eligible for even longer periods, and their take-up is significantly higher.

As shown in fig. 2, exposure to the grant is largely determined by cohort of the child, and varies drastically even for children born only one year apart. This is the variation underlying the identification strategy. More specifically, what matters in this setting is that the large variation across adjacent cohorts is the result of exogenous changes in policy. Figure 3 plots the difference in the share of mothers receiving the grant between those whose youngest child was born in 1992, versus in 1993, the last unexposed cohort and the first exposed cohort respectively, which gives the first stage of the estimation. Overall, the differential is around 20 percentage points in 2006, and around 5 percentage points the year before and the year after. The graph indicates that around 20 percent more mothers in cohort 1993 are treated for one year, where one year of eligibility implies a total transfer of around \$400 (\$650 PPP in 2010). It is key to understand here that these differences in CSG exposure occur, on average, for adjacent cohorts, not adjacent months: individuals born one year apart can have large differences in the eligibility and take-up of the CSG, but not individuals born one month before or after. To the best of my knowledge, no data exist to reconstruct CSG take-up by month of birth. Therefore, the focus is always on average differences across individuals born in different, discrete cohorts.

There are two further important aspects to clarify about the specific shocks used for identification (differential exposure of cohort 1993 versus 1992). First, these cohorts were not exposed to the grant before this period: as can be seen from fig. 1, which comes from administrative data, virtually no grant was paid out before 2001. For that reason, children who had turned seven before 2001 had certainly never



**Figure 3.** Difference in Share of Mothers Receiving the CSG between Cohorts 1993 and 1992, 2003–2011

Source: Author's calculations on GHS.

Note: This graph plots the difference in the share of mothers receiving the CSG whose youngest child was born in cohort 1993 (July 1992–June 1993) relative to mothers whose youngest child was born in cohort 1992 (July 1991–June 1992). 95% confidence intervals around the difference.

received the grant. This is important for the interpretation of the results: this additional year of eligibility is de facto the only year in which the grant is received for the cohort at the threshold. Figure S2.2 in the supplementary online appendix plots total years of eligibility over the period 2002–2011 by cohort: we see that cohort 1993 is eligible only for 1.5 years over the period, while younger cohorts are eligible for significantly longer periods.<sup>14</sup> Finally, the other important aspect for the interpretation of the effect is the age of the youngest child when he/she is eligible for the grant for the specific cohorts relevant in the identification. This is shown in fig. S2.3, where eligibility and take-up of the grant are plotted over the age profile of the child, for cohorts 1992 and 1993: we see that cohort 1993 is eligible for eight months when the child is 12, for the full year when the child is 13, and then loses eligibility. Cohort 1992 is only eligible for eight months when the child is 13, and never before or after.

#### 4.1.1. Heterogeneity by Marital Status

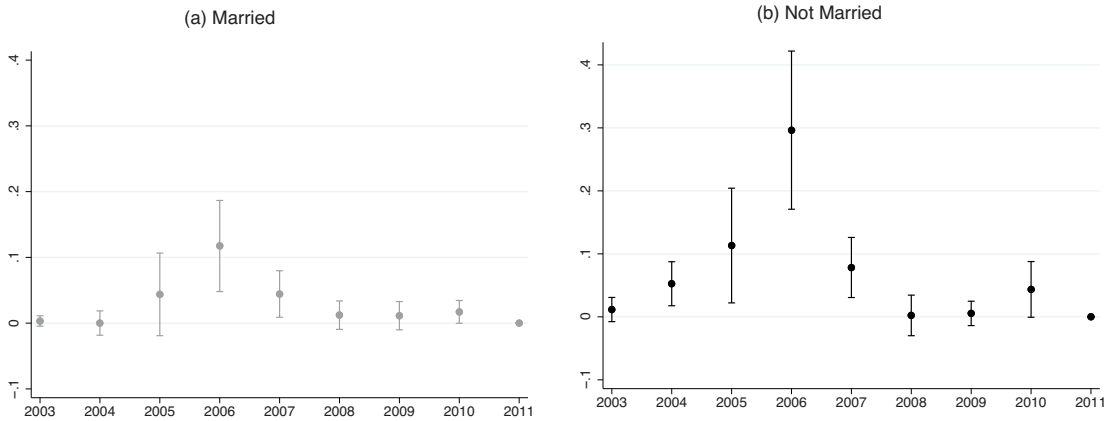
One important heterogeneity, which pertains to the entire analysis, is that of marital status.<sup>15</sup> There are two reasons to expect unmarried women to have a higher take-up of the grant. The first is that access to the grant is easier for them, as the means test (before 2008) specified that the income of both spouses should enter the means test. As mentioned before, the means test was never really enforced, but this more stringent criterion could still have discouraged applications for the grant by those with a partner earning an income. The second reason is that those women not living with a spouse or partner are likely to be the most liquidity constrained, and therefore more prone to apply and take up the grant. On average, unmarried women are in larger households, but with more young people, fewer people employed, and fewer adult males (table S1.1). Moreover, these households have significantly lower total income (and even lower per capita income).

Indeed, in fig. 4, we observe that take-up is three times higher for women who are not married or living with a partner. In 2006, around 30 percent of them receive the transfer, while the share is only 10 percent

14 Cohort 1995 (July 1994–June 1995) is eligible for almost the full period 2002–2011.

15 This category is based on official marriage or cohabiting status, i.e., women reporting living like married are included in the *married* group.

**Figure 4.** Difference in Share of Mothers Receiving the Child Support Grant (CSG) between Cohort 1993 and 1992 by Marital Status, 2003–2011



Source: Author’s calculations on the General Household Survey (GHS).

Note: These graphs plot the difference in the average take-up of the Child Support Grant (CSG) for mothers whose youngest child was born in cohort 1993 (July 1992–June 1993) relative to mothers whose youngest child was born in cohort 1992 (July 1991–June 1992). The left-hand panel plots the difference for mothers who are married or living like married, and the right-hand panel for those who are not. 95 percent confidence intervals are plotted around the difference.

among married women. Both explanations are possibly at play here (discouragement over the means test and self-selection), but what matters to the analysis is that marital status is a strong predictor of who is treated, to the point where almost all of the take-up is concentrated among unmarried mothers. Given this large differential, throughout the analysis results are always presented separately for these two subgroups (which are of roughly the same size).

#### 4.2. Identification Strategy

Given this setting, a first logical option would be to perform a difference-in-differences estimation, comparing the outcomes of mothers of exposed and unexposed cohorts before and after CSG receipt. However, a diff-in-diff with cohorts can be problematic. This estimation would rely on a “common trend” assumption, which is unlikely to hold. By definition, for a given period of time, we cannot observe the same age evolution for different cohorts. If there are effects of the age of the child on the mother’s labor-market outcomes, which is most likely the case, and if these age effects are non-linear, then identification fails by construction. See supplementary online appendix S3 for a simple proof outlining this issue.

To solve this problem, we run a different estimation, which exploits the large, discontinuous spike in take-up observed in [fig. 2](#) of cohort 1993 relative to 1992. In the spirit of a regression discontinuity (RD) design, we estimate a regression where the forcing variable is the discrete cohort of birth of the child and the threshold is set at 1993. The advantage of this approach, compared to a difference-in-differences, is that any age effect should be captured away by the functional form on both sides of the threshold; hence, the “common trend” assumption among treated and non-treated cohorts is not required. In this setting, identification relies on the fact that there is a last untreated cohort, 1992, and a first treated cohort, 1993, which, for at least one year, receives the grant to a significant extent. This estimation is made possible by the large take-up differential between cohorts 1992 and 1993, of the order of 0.2 in total (0.3 for unmarried) in 2006.

With this estimation, identification relies on the assumption that, had the CSG not been implemented, we should not observe any discontinuity at the threshold with respect to the labor-market outcomes of the mother. This can be easily checked by using the three cross-sections available: one in late 2001, before any CSG; one in early 2007, just after the large spike; and one in 2011, well after the spike. By estimating

the effect separately in 2007 and 2011, it is possible to disentangle the short-term effect of receiving the CSG from its long-term effect five years after. Formally, we estimate the following equation, separately for 2001, 2007, and 2011:

$$Y_i = f(c_i - 1993) + \mathbb{1}\{c_i \geq 1993\} \times f(c_i - 1993) + \beta_1 \mathbb{1}\{c_i \geq 1993\} + \gamma X' + \varepsilon_i, \quad (1)$$

where  $Y_i$  is the outcome of interest for the mother of a child born in a given cohort (July to June), and  $f$  is a function of the cohort of the youngest child centered at the cut-off point. We then introduce a binary variable for individuals whose youngest child is born after cohort 1993, and interact it with the cohort of birth of the youngest child born. Here,  $\beta_1$  should capture in 2007 the short-term effect of the positive CSG shock, and in 2011 the persistent effect of having received the grant five years earlier. We denote by  $X'$  a vector of covariates including a control for household size and dummies for age, education, population group, and province. Standard errors are clustered at the household level.<sup>16</sup>

All estimations are performed on the sample of Black and Coloured mothers (more than 90 percent of all mothers). The forcing variable is always the cohort of the youngest child born to a given mother, regardless of whether this child is observed in the household or not. One advantage of this approach, which is possible using fertility information in the census, is that it is not necessary to observe a child in a household to know whether or not the mother was treated. In this way, treatment status is defined much more accurately than if only observing children in households, which would be particularly problematic for the long-term results when the children at the threshold are already quite old, and therefore likely to live away. It also takes away the concern that the grant might lead to changes in household composition (Ardington, Case, and Hosegood 2009; Hamoudi and Thomas 2014), which could introduce selection bias. A disadvantage is that fertility information is only asked of women under 50 years of age, so there is no information for women older than this threshold.<sup>17</sup> Therefore, all estimations are run on the subsample of women born between 1960 and 1985, for whom both fertility and labor-market information is available in the three census waves. In this way, the underlying population over time is roughly the same, except for those mothers who have other children between 2001 and 2011, who drop out of the sample.<sup>18</sup> Given the discreteness of the forcing variable, and the few support points near the threshold, an Akaike information criterion (AIC) that maximizes the window for a linear fit is used to select the preferred bandwidth, and also to avoid the use of higher-order polynomials, as suggested in the RD literature (Gelman and Imbens 2019).

### 4.3. Results

The results of equation (1) on “job quantity” outcomes (activity = employment + unemployment) are presented in table 2, by year and marital status, because, as shown in fig. 4, unmarried women are three times more likely to be treated than married women. First and foremost, we observe that in 2001, before the implementation to scale of the CSG, mothers were comparable in terms of labor-market outcomes on either side of the threshold. Mothers who will receive the CSG a few years later are statistically indistinguishable from those who will not. This reassures us of the validity of the estimation; it shows that there is nothing taking place before that could confound the effect of the CSG.

- 16 Lee and Card (2008) initially suggested that in an RD design with a discrete forcing variable, clustering of the standard errors should occur over the discrete values of the running variable, while in a more recent development, Kolesár and Rothe (2018) strongly advise against this practice, in particular when the number of clusters is small. In the estimations, the standard errors always drop significantly when clustering by the running variable.
- 17 This makes the data censored at age 50. We consider this not to be an issue as long as the probability of being over 50 is not discontinuous at the threshold. Further discussion of this issue can be found later in the robustness checks.
- 18 Further discussion of how this could impact the estimation or the dynamic interpretation of the estimates is found in the robustness checks.

**Table 2.** CSG Effects on Activity, Unemployment, Employment: Mothers, 2001, 2007, and 2011

	Year 2001—"before"					
	Married			Unmarried		
	(1) Active	(2) Unemployed	(3) Employed	(4) Active	(5) Unemployed	(6) Employed
CSG	0.0017 (0.0064)	-0.0017 (0.0066)	0.0034 (0.0067)	0.0008 (0.0055)	0.0088 (0.0065)	-0.0080 (0.0063)
Observations	113,332	113,332	113,332	125,030	125,030	125,030
$\bar{Y}$ at threshold	0.703	0.332	0.371	0.783	0.398	0.386
Pop. at threshold	82,840	82,840	82,840	92,580	92,580	92,580
	Year 2007—"during"					
CSG	-0.0001 (0.0157)	-0.0146 (0.0150)	0.0146 (0.0176)	0.0314** (0.0141)	0.0379*** (0.0145)	-0.0065 (0.0167)
Observations	16,176	16,176	16,176	17,010	17,010	17,010
$\bar{Y}$ at threshold	0.751	0.235	0.515	0.773	0.221	0.552
Pop. at threshold	58,448	58,448	58,448	62,604	62,604	62,604
	Year 2011—"after"					
CSG	0.0061 (0.0091)	0.0003 (0.0071)	0.0058 (0.0094)	-0.0023 (0.0085)	-0.0031 (0.0071)	0.0008 (0.0091)
Observations	50,218	50,218	50,218	50,926	50,926	50,926
$\bar{Y}$ at threshold	0.628	0.162	0.466	0.688	0.170	0.517
Pop. at threshold	43,146	43,146	43,146	44,450	44,450	44,450

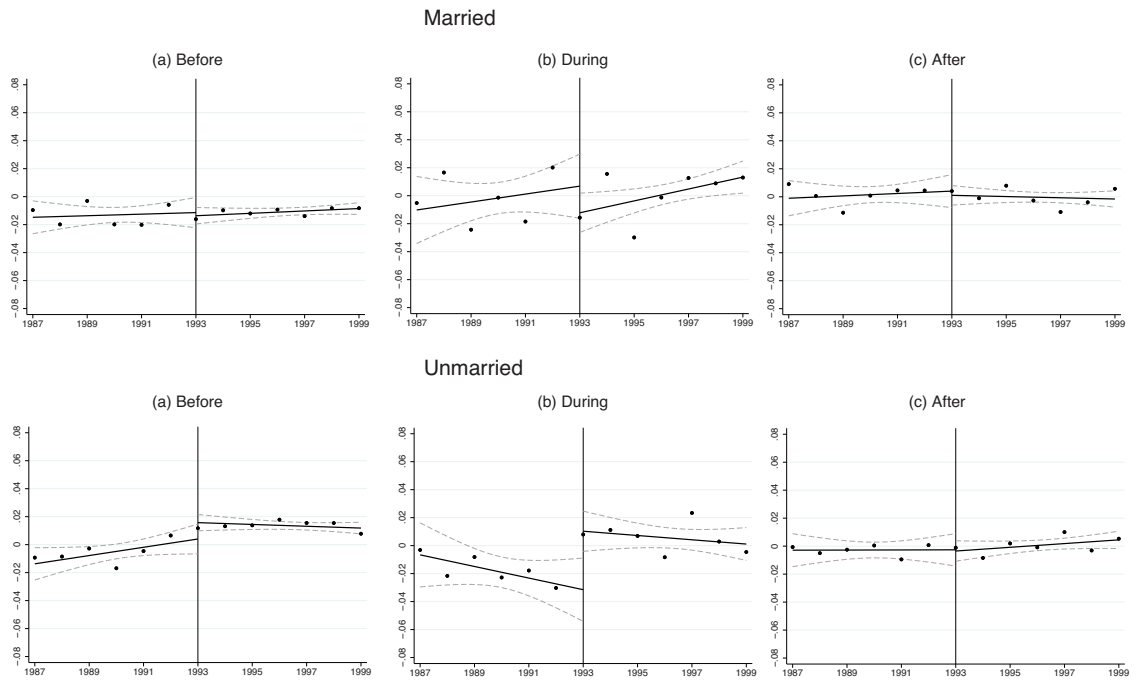
Source: Author's calculations on Census 2001 & 2011 and Community Survey 2007.

Note: This table gives the Ordinary Least Squares (OLS) estimates of equation (1) in 2001, 2007, and 2011, for married and unmarried mothers respectively. Only Black and Coloured mothers born between 1960 and 1985 are included. The forcing variable is the cohort of birth of the youngest child ever born to a given mother. The functional form is linear for the window of cohorts born between 1987 and 1998. Child Support Grant (CSG) is a binary variable for the child being born in or after cohort 1993, which indicates being part of a cohort that had access to the Child Support Grant, and  $\bar{Y}$  gives the mean of outcome  $Y$  for the last untreated cohort in the sample. "Pop. at threshold" indicates the weighted population for the last untreated cohort in the sample. All estimations include controls for age (quadratic), education, population group, province, migration status, household size, and month of birth of the child. Standard errors clustered at the household level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

In 2007, after the year in which the CSG was received, there is a significant increase in the probability of treated single mothers being active, and a symmetric and equal size increase in the probability of actively looking for a job. Married women do not respond across any of the outcomes: activity, unemployment, or employment. These effects are clearly visible in [fig. 5](#). Before and after, the unemployment rate of married and unmarried women is the same at the threshold. In 2007, there is a large and significant spike, but for single mothers only. This increase in unemployment comes entirely from higher activity levels ([fig. S2.4](#)). In the short term, the probability of being employed is always the same between treated and non-treated mothers. Consistent with the surveys of the literature by [Banerjee et al. \(2008\)](#) and [Baird, McKenzie, and Özler \(2018\)](#), here also we do not find any evidence that cash transfers actually discourage work.

In 2011, five years after the one-year exposure to the grant for cohort 1993, mothers who received at least one year of grant are not more likely to be employed. There seem to be no lasting effects on the probability of having a job. This could be taken as an indication that liquidity constraints do not prevent entry into employment at the extensive margin, such that, although the grant promotes activity and search, the long-term employment prospects are the same.

Instead of changing the probability of employment, the impact of the grant could manifest in the quality of employment. In the context of a developing country, subsistence-level jobs might be easily accessible, such as agricultural, domestic work, or informal self-employment, while higher-quality-wage jobs might

**Figure 5.** CSG Effect on Unemployment Rate (Residuals)

Source: Author's calculations on Census 2001 & 2011 and Community Survey 2007.

Note: These graphs report the estimations of equation (1), for both married (upper panel) and unmarried mothers (lower panel), of the population of Black and Coloured mothers born between 1960 and 1985. The residuals are plotted after controlling for age (quadratic), education, population group, province, migration status, household size, and month of birth of the child. The forcing variable is the cohort of birth of the youngest child ever born to a given mother. The functional form is linear for the window of cohorts born from 1987 to 1998. The window is chosen according to an Akaike information criterion (AIC) that maximizes the window for a linear function.

be harder to get and require a longer search. Table 3 shows how one year of exposure to the CSG has impacted the types of jobs that mothers hold five years after having received the grant. Employment is divided into four categories that match those mentioned before and are standard in the literature: employment in the agricultural sector, domestic cleaners (which is the most frequent job of Black and Coloured women), self-employment, and wage employment (outside agricultural and domestic work).<sup>19</sup> In South Africa, wage employment is generally considered of better quality than the other categories and with higher earnings. For the categories that can be tested in 2001 and 2007, we observe balance at the threshold in those years. Consistent with the lack of contemporaneous effects in 2007, nothing shows up for married women in 2011 either: the employment composition is the same between those who have and have not been eligible for the CSG. On the contrary, as unmarried women significantly increased their activity and search as a result of the CSG, we could expect this extra search induced by the grant to translate into better quality jobs. However, for single mothers also, we do not estimate substantial effects in terms of reallocation across type of job. We only observe a drop in the probability of working elementary occupations in the agricultural sector, which is large as a percentage of agricultural employment (−23 percent), but a relatively small effect overall (−0.7 percentage points). This is also clearly visible in fig. 6: the rate of employment in the agricultural sector drops at the threshold, again

19 To note that, as mentioned before, the same categories as in 2011 cannot be replicated for 2007 and 2011. In 2007, only the distinction between wage and self-employment is possible. In 2001, a specific category for domestic workers is not available.

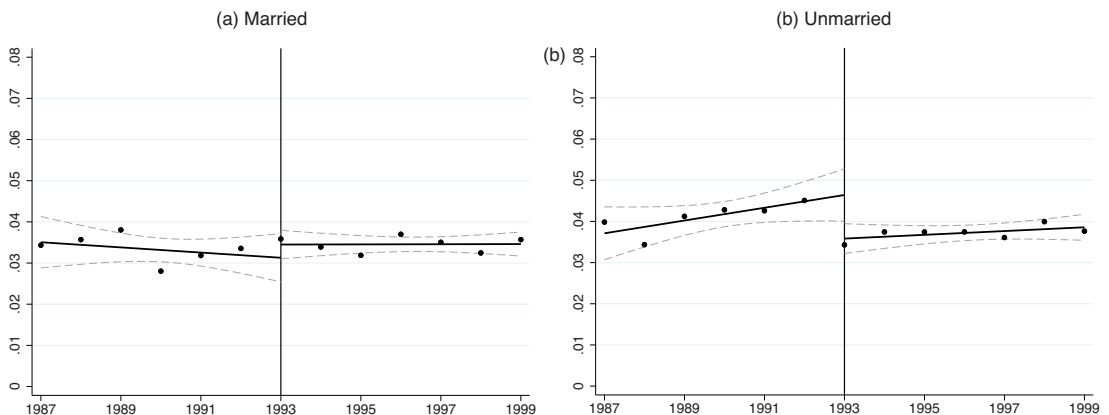
**Table 3.** Child Support Grant (CSG) Effects on Job Type: Mothers, 2001, 2007, and 2011

	Year 2001—"before"							
	Married				Unmarried			
	(1) Agriculture	(2) Domestic	(3) Self	(4) Wage	(5) Agriculture	(6) Domestic	(7) Self	(8) Wage
CSG	-0.0011 (0.0028)	—	-0.0007 (0.0018)	0.0052 (0.0064)	-0.0016 (0.0026)	—	0.0003 (0.0016)	-0.0067 (0.0061)
Observations	113,332	—	113,332	113,332	125,030	—	125,030	125,030
$\bar{Y}$ at threshold	0.034	—	0.018	0.320	0.032	—	0.015	0.338
Pop. at threshold	82,840	—	82,840	82,840	92,580	—	92,580	92,580
	Year 2007—"during"							
CSG	—	—	0.0062 (0.0086)	0.0084 (0.0178)	—	—	-0.0032 (0.0082)	-0.0033 (0.0169)
Observations	—	—	16,176	16,176	—	—	17,010	17,010
$\bar{Y}$ at threshold	—	—	0.051	0.458	—	—	0.059	0.490
Pop. at threshold	—	—	58,448	58,448	—	—	62,604	62,604
	Year 2011—"after"							
CSG	0.0042 (0.0033)	0.0009 (0.0056)	0.0048 (0.0045)	-0.0042 (0.0085)	-0.0075** (0.0035)	0.0020 (0.0060)	0.0022 (0.0043)	0.0040 (0.0084)
Observations	50,218	50,218	50,218	50,218	50,926	50,926	50,926	50,926
$\bar{Y}$ at threshold	0.026	0.091	0.055	0.294	0.037	0.110	0.056	0.314
Pop. at threshold	43,146	43,146	43,146	43,146	44,450	44,450	44,450	44,450

Source: Author's calculations on Census 2001 & 2011 and Community Survey 2007.

Note: This table gives the Ordinary Least Squares estimates of equation (1) on mothers' types of occupation in 2011. Only Black and Coloured mothers born between 1960 and 1985 are included. The forcing variable is the cohort of birth of the youngest child ever born to a given mother. The functional form is linear for the window of cohorts born between 1987 and 1998. CSG is a binary variable for the child being born in or after cohort 1993, which indicates being part of a cohort that had access to the Child Support Grant, and  $\bar{Y}$  gives the mean of outcome  $Y$  for the last untreated cohort in the sample. The coefficients of columns (1)–(4) and (5)–(8) add up to the coefficient on employment of table 2. Pop. at threshold indicates the weighted population for the last untreated cohort in the sample. "Agriculture" includes any work in the agricultural sector, including subsistence agriculture. "Domestic" indicates those employed as cleaners in private households. "Self" indicates self-employed and "Wage" those with a wage job. The cell is empty when a given category is not available in that year. All estimations include controls for age (quadratic), education, population group, province, migration status, and household size. Standard errors are clustered at the household level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Figure 6.** Rate of Agricultural Employment by Birth Cohort of Youngest Child, 2011



Source: Author's calculations on Census 2011.

Note: These graphs give the probability of being employed in the agricultural sector in 2011, for married and unmarried women, by cohort of birth of the youngest child ever born to a mother. A linear function is fitted on each side of the threshold with 95 percent confidence intervals. Mothers of children born in cohort 1993 have been eligible for the Child Support Grant for at least one year, and unmarried women are three times as likely to be treated; see text for explanation.

for unmarried women only. The effect on wage employment is positive but not significant, as is the joint coefficient of all non-agricultural employment.<sup>20</sup>

The magnitude of the contemporaneous effects is not trivial, and has to be read relative to the first stage that is shown in [fig. 4](#): at most, 30 percent of unmarried women are treated for one year. The 2007 activity/unemployment effect is very large when measured on the total population (+4 percent activity, +13 percent unemployment), which indicates an even larger effect on the treated. However, this does not imply that the total effect would be larger if the entire population of mothers were to be treated and not only 30 percent. Indeed, we may expect that those who self-select into take-up are also the ones most in need of cash to facilitate job search.

Overall, two main takeaways from the labor-market effects of the grant seem important and in part new to the literature. First, the CSG clearly did not have a persistent positive (or negative) impact on the employment rate. This is true both immediately after the grant was received, and several years later. This finding could be an indication that the grant is not large enough, or that liquidity constraints are not so severe as to prevent entry into employment at the extensive margin, or that, most likely, other barriers are binding that are not necessarily monetary. As overall employment for Black and Coloured women is very low, one could argue that increasing participation at the extensive margin is the first-order concern, and, on that, no evidence emerges that cash transfers to mothers, at least in the South African case, are enough to achieve that goal. Second, we nonetheless find clear evidence that the grant appears to facilitate job search, in particular for single mothers, although this does seem to translate into relatively small job quality improvements. This is key as it points to the presence of binding liquidity constraints that may prevent women from searching without the welfare transfer. In the absence of constraints, we would not expect this spike in activity and search levels when they have more cash on hand. Instead, such a spike is consistent with what we know about high search costs in South Africa (and African labor markets in general).

A key question is whether the grant can have lasting positive effects on income-generating activities, including, but not limited to, wages. The census only collects interval-coded data for individual and household income, in large brackets, and has been shown to significantly overestimate the number of zero incomes ([Ardington et al. 2006](#)). The estimates of equation (1) on income are presented in table S2.1 in the supplementary online appendix, using a tobit model to account for the interval-coded nature of the data. The coefficients are positive for both married and unmarried women as from 2007, but they are not statistically distinguishable from zero. Given the data constraints, the minimum detectable effect on monthly income would be of the order of R 200 in 2011 on the whole population. Considering the first stage of around 0.3 to be statistically identifiable, this would require any contemporaneous or persistent income gains on the treated to be significantly larger than the monthly amount of the grant (R 250), which seems unrealistic. With this in mind, the lack of a significant effect on income should not be surprising; estimating whether this type of grant can have lasting effects on income would require much more precise data (or a less demanding specification). Yet, a zero effect is consistent with the absence of significant changes in both the probability and the composition of employment that we observe for treated mothers: as employment prospects do not change significantly, and other sources of income are rare, we would not expect there to be lasting positive effects on income.

#### 4.4. Robustness Checks

Several robustness checks are performed for these estimations, presented in the supplementary online appendix. Results are robust to changing the bandwidth size, by gradually reducing the number of cohorts included in the estimation. These checks are presented in [fig. S2.5](#). The positive short-term effects on activity levels and unemployment are not sensitive to the bandwidth size. The coefficients are always of

20 Not shown in the table:  $\beta$ : 0.0084,  $p$ -value: 0.364.

similar size between the two estimations—implying that unemployment increases as a result of people coming out of inactivity—and are stable regardless of the number of cohorts included. In 2011 we also see that the null effect on employment is very stable across the bandwidth size, and so is the small negative effect on the probability of working in the agricultural sector.

One concern is that fertility decisions might be impacted by the grant. Even though the rollout of the grant occurs after the youngest child is born for the cohorts in this analysis, exposed mothers might be more or less likely to have an additional child because of the grant, which would lead to selection. This would show up in the density of cohorts around the threshold in the three years and the share of mothers remaining in the estimation window relative to the initial population in 2001 (i.e., those who do not die or have new children). In the spirit of [McCrary \(2008\)](#), fig. S2.6 shows that no discontinuity exists at the threshold, either before, during, or after the rollout of the grant. The share of mothers remaining in the sample is also smooth at the threshold.<sup>21</sup> Fertility data in the census are censored at age 50, meaning that older women are not asked about their birth history. This should not lead to selection as long as the probability of being 50 or older is not discontinuous at the cutoff point. Again, the smoothness of the density around the threshold suggests that this is not the case. Another concern is that marital status—which is used as a heterogeneity dimension throughout the analysis—might be endogenous to the treatment. Figure S2.7 in the supplementary online appendix shows that the probability of being married does not change significantly across the three data points over the cohort of birth of the youngest child, and does not jump or drop at the threshold. More generally, we test the continuity at the threshold of several household covariates in the next section, and find no evidence of discontinuities.

Moreover, a set of placebo estimates are obtained by varying both the bandwidth and the position of the threshold around cohort 1993. This should give an idea of the size of the jumps estimated at the “correct” threshold relative to other, “misplaced” thresholds, in the spirit of a randomization inference test. These distributions are presented in fig. S2.8 in the supplementary online appendix for the three relevant outcomes: unemployment and activity levels in 2007, and agricultural employment in 2011. For all three outcomes, the distributions of coefficients with placebo thresholds are centered around zero, and the coefficients estimated with the “correct” threshold are above (respectively below) the 95th (respectively 5th) percentile of these distributions.<sup>22</sup> This supports both the internal validity of the estimation, and the statistical significance of the results: the estimated coefficients with the correct threshold in the correct year are generally at the extremes of the distributions relative to all other years  $\times$  threshold possible combinations.

## 5. Discussion

### 5.1. Mechanisms

As underlined earlier, one of the attractive features of studying this specific program is the lack of conditionalities, coupled with the de facto non-application of the means test, which significantly simplifies the interpretation of its labor-market effects: among the possible mechanisms, in interpreting the results of the CSG, we can exclude those that go through the conditionality attached to the grant, as there are none.<sup>23</sup>

- 21 While a large share of mothers have new children over the period (roughly 50 percent), this does not occur differentially at the threshold. Moreover, most of the drop occurs between 2001 and 2007, while what is important for the dynamic interpretation of the coefficients is the share of the population remaining at the threshold between 2007 and 2011, when the decrease is much smaller (as these mothers are older and less likely to have new children).
- 22 It should be noted that, in this setting, estimations with placebo threshold still contain the “correct” threshold, which could potentially lead to misspecification and coefficients that are further away from zero.
- 23 [Baird, McKenzie, and Özler \(2018\)](#) separate those from “conditioning on work or not working” and “conditioning on time-consuming activities”.



It should be noted, however, that the program is labeled as “child support” grant, and labels have been shown to matter (Benhassine et al. 2015). As a cash transfer cannot be completely without a label, this remains informative about an income shock that occurs within the most common type of cash-transfer label, given that child grant programs are virtually universal across developing countries.

To frame the effects showed in the previous sections, it is useful to think in terms of the framework put together by Baird, McKenzie, and Özler (2018), who surveyed the literature for the various arguments about how cash transfers could impact labor-market outcomes, in the short and long terms. In the short term, as already mentioned, the literature shows little evidence of a significant labor–leisure trade-off effect of cash transfers, despite what a canonical leisure-consumption model would suggest, i.e., that cash transfers should decrease work at the extensive and intensive margins (Banerjee et al. 2017; Baird, McKenzie, and Özler 2018).<sup>24</sup> This is consistent with the evidence presented in the paper: employment does not decrease, and activity levels increase through higher search. Among the relevant potential mechanisms cited by Baird, McKenzie, and Özler (2018) that could last in the longer term, we can operate a distinction between those that imply either a change in behavior of the individual, or, alternatively, a change in the characteristics of the individual herself as a result of the transfer. In this last cluster of potential mechanisms, the common denominator is that the cash-transfer recipient either becomes more productive, because of better health or higher human capital, or less productive, because the cash-transfer-induced decrease in labor supply leads to a depreciation of her human capital. A second class of potential mechanisms deals instead with changes in behaviors as a result of the positive income effect. Baird, McKenzie, and Özler (2018) cite three mechanisms: (a) a “self-employment liquidity” effect, (b) an “insurance” effect, and (c) an “investment-in-labor-search” effect. The first channel argues that, in the presence of liquidity constraints, a cash transfer could allow aspiring entrepreneurs to put up the capital to start a business, an argument that has been at the core of the micro-finance literature. Instead, the insurance effect suggests that a cash transfer could increase a person’s willingness to bear risk, and hence push them towards more risky behaviors (e.g., migration), or more risky occupations (e.g., self-employment). Lastly, the investment-in-job-search effect stems from the intuition that looking for a job can be very costly, in terms of both time and resources. This is particularly true in developing countries, and even more so in South Africa, where transport costs are exceptionally high (Kerr 2017).<sup>25</sup>

The evidence here points directly to the “investment-in-labor-search” effect as the main channel. After receiving the grant, the probability that single mothers are active and looking for a job increases. Contrary to Bianchi and Bobba (2013), no evidence emerges of a liquidity or insurance effect towards self-employment: treated mothers are not more likely to end up running a small business either in the short term or several years after receiving the transfer. This is also true at the household level: there is no increase in self-employment for other household members who are not the direct recipients of the grant (Tondini 2019).<sup>26</sup> More generally, there is no support for an “insurance” effect at play here, meaning evidence of cash transfers pushing towards more risky behaviors/occupations: the probability of migrating (internally or abroad) is not affected by the grant (fig. S2.9), contrary to what Ardington, Case, and Hosegood (2009) find in the case of the Old Age Pension, and the CSG does not seem to significantly change the occupational composition other than the small shift away from agricultural employment.

24 This evidence applies to working-age recipients. For specific populations who receive large transfers, such as the elderly, disincentive effects on work can be large. See for example the case of the Old Age Pension in South Africa in Tondini, Ardington, and Woolard (2017).

25 As a result of the designs of the apartheid period, Black and Coloured individuals were coercively relocated far away from the city center where most jobs usually are, and this has, to a large extent, persisted to this day.

26 One possible interpretation for these different findings from Bianchi and Bobba (2013) in Mexico is the diverse nature of informal employment, as shown by the drastically different importance of self-employment in the two economies. The lack of self-employment in South Africa is possibly a long-term consequence of its troubled political and economic history. The apartheid regime strongly repressed all types of self-employment activities among the native African population.

It is harder to tackle the set of “competitiveness” arguments, i.e., whether the grant has changed the recipients or their environment in a way that could explain these results. To the extent that it can be observed in the data, this does not seem to be the case. Figure S2.10 in the supplementary online appendix plots predicted mothers’ activity and unemployment rates across birth cohort of the youngest child based on all of the mothers’ and household members’ (child and other adults’) characteristics (such as school attendance or employment), and household assets, selected through a LASSO procedure.<sup>27</sup> They are smooth around the threshold, suggesting that treated and non-treated mothers should be comparable in terms of their labor-market outcomes given their, and their households’, relevant observable characteristics. In other words, there is nothing observable that is different for treated mothers or their environment that would explain this increase in activity and unemployment other than the extra money from the grant.

An advantage of the specific CSG shock used, the one resulting from the initial age-threshold reform, is that it is received when the child is quite old. As early childhood interventions might have more lasting effects on children’s development (Millán et al. 2019), this makes it less likely that the grant has a direct effect on the child that could be the driver of the mother’s spike in search and could confound the interpretation of the effect. Lastly, it has been shown that cash transfers can have positive effects on women’s empowerment and bargaining power with the spouse (Ambler 2016; Almas et al. 2018). However, as the effect is concentrated on single mothers, i.e., those who live without a husband/partner, this is unlikely to be the main mechanism at play. For married women, the lack of effect could hide a response from the spouse/partner living in the household. This is presented in table S2.2 in the supplementary online appendix. Although estimates are more imprecise than for women, there is no evidence of a significant response from the spouse/partner in the household.

## 5.2. External Validity

The evidence in favor of the job search mechanism is also in line with recent studies in the context of other African countries. Abebe et al. (2021) evaluate two interventions in Ethiopia aimed at reducing both search frictions—through transport subsidies for the unemployed—and information frictions. Transport subsidies, similarly to cash transfers, can be seen as a way to alleviate liquidity constraints, although they are more strongly tied to job search, as they also incentivize search and do not only lift constraints. The authors find that transport subsidies significantly increase search levels, but any gain in job quality/quantity dissipates over the medium term, and only the information frictions intervention has lasting effects. In the context of South Africa, Carranza et al. (2021) also show that reducing information frictions has lasting positive effects on employment outcomes. Similarly, Abel, Burger, and Piraino (2020) estimate large, positive effects of incentivizing the use of reference letters, again aimed at reducing information frictions, on women’s employment. This paper can be seen as a complement to the results of these studies, as it shows—similarly to Abebe et al. (2021)—that relaxing liquidity constraints seems to encourage and facilitate search, yet it is not enough on its own to increase employment prospects in the long term. Taken together, these insights suggest that it may be more effective to combine cash transfers with interventions that lift other type of barriers, not only financial.

Finally, it should be noted that these results are likely to be context specific across two important dimensions: the type and size of income shock, and the treated population (mothers) in a particular labor market (South Africa) for a developing country. Regarding the importance of the size of the transfer, the evidence would suggest that the same conclusions, for this subpopulation and in this labor market, would hold for larger transfers. Looking at the graphical evidence presented before, we note that treated

27 These include (a) individual: age, population group, years of schooling, away from birth province, current district, and number of children; (b) household level: number of people in the household, number of employed people, number of people attending school, average years of schooling in the household, number of adult males, number of children in the household, and average age; (c) household assets: fridge, radio, computer, television, telephone, cell phone, tenure status, electricity, water, and toilet.

cohorts who received significantly larger amounts because of longer eligibility (1994 onwards, fig. S2.1) have labor-market outcomes that are not qualitatively different.<sup>28</sup> With this descriptive evidence, one could speculate that, while larger transfers might have marginal effects we cannot capture here, they would not change the overall picture. The other caveat to keep in mind is the target population of this cash transfer and its labor market: women's labor-market outcomes are particularly dire in South Africa, which already has a labor market far underperforming developing countries with similar GDP per capita level and with significantly less self-employment. It is very possible that in countries where other barriers to (good-quality) employment are less binding, the overall results would be different. For example, the positive effect on entrepreneurship that [Bianchi and Bobba \(2013\)](#) find for Progresia in Mexico is less likely to appear in a country that has less than 1/3 of the self-employment of Mexico ([Tondini 2019](#)). It should also be noted that large cash transfers such as this one can have important general equilibrium effects, which can then affect labor-market outcomes in a variety of different ways ([Egger et al. \(forthcoming\)](#)), and that what is captured here is the effect of receiving the grant relative to not receiving it in an economy where many receive it.

## 6. Conclusion

This paper has studied how an unconditional cash transfer, the South African CSG, impacts the labor-market outcomes of recipients, in both the short and long terms. By exploiting eligibility across birth cohort of children, it shows that mothers who experience this positive income shock are more likely to be active and look for a job just after receiving the grant. This positive effect on search is entirely concentrated on single mothers, and there is no evidence of a disincentive effect on work. Five years after the grant was received, the employment rate is the same and the employment composition is similar between treated and non-treated mothers, but single mothers who have received the grant are marginally less likely to work in agricultural employment.

The characteristics of the transfer, its complete lack of conditionalities and eligibility conditions other than the age of the child, allow a clean interpretation as an income effect, avoiding potentially confounding factors on behavior. A transfer as universal as the CSG is rare in developing countries, as reflected by its large coverage. For these reasons, these results on the short- and long-term effects of an unconditional grant are more generally informative for the debate around transfers “no strings attached”, and whether cash transfers can be a way to achieve better labor-market outcomes, in particular for more disadvantaged groups. By showing that an unconditional cash transfer leads to higher levels of activity and search, this paper contributes to the growing empirical literature on search frictions in developing countries. The results are indeed consistent with key insights from the job search literature, and with recent empirical evidence on the effects of transport subsidies. The lack of a positive long-term effect on the employment rate suggests that a cash transfer of this magnitude does not help to overcome barriers to entry into employment at the extensive margin, or that these barriers are not related to liquidity constraints and search frictions.

Lastly, an important question that remains unanswered is to what extent these results would carry through at the economy-wide level, or to different target groups or labor markets. What we observe in South Africa is that, despite dramatic growth in CSG coverage in the early 2000s, labor-market outcomes for women have not improved significantly. This would also seem to confirm that cash transfers are not sufficient, on their own, to increase women's employment rate and job quality in the aggregate. Yet labor-market prospects for women in South Africa were particularly grim over the period of the study, much

28 For example, in fig. S2.4 we note that the activity rate of mothers whose children have been eligible for many more years than those at the threshold is roughly similar to never-treated cohorts, or the cohort treated for only one year. This would suggest, speculatively, that even larger amounts have not drastically changed employment prospects.

more so than in other countries at similar levels of development, which might indicate similar policies could be more successful in different contexts. Overall, the results of this paper, combined with other recent evidence, suggest that cash transfers could be an important policy in lifting liquidity constraints and help to decrease barriers to quality employment for disadvantaged groups.

## Data Availability

The data underlying this article are available at the DataFirst repository of the University of Cape Town, and can be accessed at <https://www.datafirst.uct.ac.za/>.

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