Labor Market Effects of Cash "No Strings Attached": Evidence from South Africa's *Child Support Grant*

Alessandro Tondini*
Paris School of Economics

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Abstract

Does money without conditions have lasting effects on employment outcomes in a segmented labor market? I study the impact of an unconditional cash transfer in South Africa paid to Black and Coloured women, a group with both low employment and high informality. I use discontinuous exposure to the *Child Support Grant* for mothers whose child was born one cohort apart to identify the labor market effects of roughly one year of grant ($\approx$ 400 \$ 2010). As a job search model would predict, these mothers are more likely to be unemployed, and less likely to be working, while receiving the transfer. Five years after the grant has stopped, the employment rate is back to the same level as ineligible mothers, but those who had received the grant are more likely to work in the formal sector. More money does not seem to translate into greater job quality gains. Overall, these results indicate that unconditional cash transfers can have persistent, positive effects on job quality, but do not seem to increase employment at the extensive margin.

JEL Codes: J46, J42, O17, I38
Keywords: job quality; unconditional cash transfers; informal sector; South Africa;

1 Introduction

Labor market segmentation is a salient issue in developing countries, where the informal sector often employs a significant portion of the workforce. In these contexts, the broad concept of "job quality" is particularly relevant, as simple measures of employment (and unemployment) may be a poor indication of labor market performance. A key question is whether segmentation is the result of frictions and barriers in the labor market, or whether workers voluntarily and freely allocate across sectors. The answer to this question

*Contact: alessandro.tondini@psemail.eu
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has important repercussions on what policies should be put in place to improve access to better jobs in the context of a segmented labor market, specifically for disadvantaged groups. A negative view of segmentation, for example as a result of high search costs, would suggest that social assistance programs could potentially result in job quality gains for recipients. Cash transfers are one of the most common policy options in the context of developing countries. However, empirical evidence on whether this type of policy can lead to lasting improvements in labor market outcomes is still limited.

In order to shed more light on the topic, this paper analyzes the labor market effects of a child grant. 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 unconditional grant makes it possibile to test if and how cash with "no strings attached" impacts employment outcomes, in terms of both quantity and quality, in a segmented labor market. Because of its peculiar characteristics, I argue that this policy provides a pure income shock, without introducing incentives or conditionalities that might complicate the interpretation of the effect. At the same time, its labeling and the size of the income component are comparable to that of many similar policies across the developing world. For identification, I exploit the large variation in access to the grant across birth years. This allows me to pin down the labor market effects on mothers of roughly one year of grant[1] both during the period of receipt, and after the grant has stopped. I find that recipient mothers are more likely to be unemployed, and less likely to take up formal sector jobs, when receiving the transfer. Five years after the grant has stopped the employment rate is the same between mothers who have received the CSG and those who haven't. However, the employment composition is not: "treated" mothers are significantly more likely to be working in the formal sector, and less likely to be working informally, which indicates a persistent positive effect of the grant on job quality. These mothers also work in occupations and industries with on average higher job stability and that pay higher wages. Significantly longer periods of eligibility to the grant do not seem to cause further job quality gains, which suggests a strongly non-linear effect.

Labor and development scholars have gone back and forth on the nature of segmented labor markets. The debate can be traced back to Todaro (1969), immediately challenged by Hart (1970, 1973), presumably the first to define and openly ask the question about the nature of the informal sector.[2] This dichotomy of views, also referred to as the "segmentation" and the "comparative advantage" hypotheses (Günther and Launov (2012)), has shaped the literature on the informal sector and informal work. While South Africa's level of informality is relatively low (Kingdon and Knight (2004), Banerjee et al. (2008)), the informal sector remains one of the main employers of Black and Coloured[3] women - mostly as domestic wage workers. Recent papers have attempted to identify the impact of specific policies on workers' allocation in a segmented labor market (Dinkelman and

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

[2] Hart (1973):" The question to be answered is this: Does the 'reserve army of urban unemployed and underemployed' really constitute a passive, exploited majority...or do their informal economic activities possess some autonomous capacity for generating growth in the incomes of the urban (and rural) poor?"

[3] This terminology reflects the classification of population groups in all the official statistics in South Africa: Black Africans, Coloured, Indian/Asians, Whites.

Ranchhod (2012), Azuara and Marinescu (2013), Bergolo and Cruces (2014), Garganta and Gasparini (2015), Gerard and Gonzaga (2016)).[4] The main conceptual difference from the *Child Support Grant* in South Africa is that these are all policies that change relative payoffs across sectors, while an unconditional cash transfer does not. In other words, while these policies provide both an income and a substitution effect, I argue that with the CSG one is able to capture the effects of a pure income shock on both the quantity and the quality of employment. This is partly in line with the work by Bianchi and Bobba (2013) on *Progresa* in Mexico, who show that recipients of a conditional cash transfer are more likely to become self-employed. One possible interpretation for these different findings is the diverse nature of informal employment, as shown by the drastically different importance of self-employment in the two economies.[5]

The interaction of social insurance policies and job search is at the heart of labor and public economics. Many studies have examined the impact of "cash-on-hand" or unemployment benefits on unemployment duration, and the quality of the subsequent job (Card et al. (2007), Nekoei and Weber (2017)). In the context of developed countries, this has led to a variety of different findings.[6] 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. Moreover, this literature has mostly focused on developed countries. There is, to my knowledge, little evidence of how cash-on-hand impacts job search in the context of a segmented labor market, and, more importantly, how this impacts the quality of the subsequent job. Probably because the programs usually analyzed for this type of evaluation, such as unemployment benefits or severance pay, are often less relevant in the context of developing countries.[7] However, whether cash-on-hand can raise future job quality is a key question, and possibly even more so in countries where "good" and very "bad" jobs coexist in the same labor market. As Card et al. (2007) say, "*..testing this prediction sheds light on whether improvements in future job outcomes provide a rationale for temporary income support programs.*" Therefore, knowing whether cash transfer recipients end up in better quality jobs (possibly paid

[4] Azuara and Marinescu (2013) study the labor market repercussions of the introduction of *Seguro Popular*, a non-contributory health insurance program, in Mexico. While such a policy should make formal employment relatively less attractive, their results show that it had "no effect on informality in the overall population." In a similar fashion, Bergolo and Cruces (2014) looks at the extension of health insurance to children of formal workers in Uruguay. Their results show that there is an increase formal employment, "mainly due to an increase in labor force participation rather than to movement from unregistered to registered employment." Garganta and Gasparini (2015) look at an "Universal Child Allowance" in Argentina, a cash transfer available only to those outside formal employment. They find that this transfer decreases incentives to become formal, but does not reallocate workers from the formal to the informal sector. Gerard and Gonzaga (2016) examine the efficiency cost to unemployment insurance in an economy with high informality, where potentially there is the perverse incentive to draw unemployment benefits and hold an informal job at the same time. However, they estimate this cost to be relatively small. Last but not least, Dinkelman and Ranchhod (2012) study the impact of a the introduction of a sectoral minimum wage for domestic workers in South Africa, a sector that is largely informal and the main employer of Black, South African women. They show a strong positive impact on wages and formality rates within the sector, yet this sector remains largely informal even after the introduction of the minimum wage.

[5] 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. Presumably, this repression still has consequences today.

[6] Nekoei and Weber (2017) for a detailed summary of the empirical evidence.

[7] One notable exception is Gerard and Gonzaga (2016) who study how informality in Brazil impacts the efficiency cost of unemployment insurance.

higher wages) would provide a clear rationale for this policy.[8] This literature also provides a very useful framework to think about the mechanisms behind the possible effects of a cash grant. As I will discuss later, the predictions of a standard job-search model match well the results of this paper.

Despite the large literature on cash transfers overall, the labor market effects on adult recipients are still poorly understood. Overall, contrary to what a canonical model would suggest, the literature shows little evidence of a leisure effect, meaning that disincentive effects on work appear to be minor and concentrated on specific populations (Banerjee et al. (2017), Baird et al. (2018)). However, while the long term effects of cash transfers on children have received significant attention (see Molina Millán et al. (2018) for review), evidence on the medium/long term employment effects on adults is rare (Baird et al. (2018)).[9] The main contribution of this paper is to show that an unconditional cash transfer can have lasting effects on the labor market outcomes of adults, and that these effects can be sizable. This finding also helps to better understand the short term responses in a dynamic way. The decrease in employment and longer search as a result of the cash grant leads recipients to better quality jobs up to five years after. This indicates that the negative, short term employment effect might result from more productive job search rather than leisure. Indeed, I show direct evidence that the grant does modify the job search behavior of recipients, who search for longer and spend more money in the process.

The paper proceeds as follows: Section 2 introduces the institutional context and various reforms. Section 3 presents the conceptual framework, and predictions of the possible effects of the grant according to an occupational choice and a job search model. The data used and descriptive statistics are found in Section 4. In Section 5, I present the empirical strategy and the results. I discuss the relevance of the channels underlying these results in Section 6. Section 7 concludes.

2 The South African *Child Support Grant*

The *Child Support Grant* is the largest social program in South Africa in terms of number of participants, reaching around 10 million children in 2010 (roughly 20% of the population), and second largest in terms of government spending (Gomersall (2013)).[10] It is generally considered to be the main anti-poverty policy of the South African government. It was first implemented in April 1998 in post-Apartheid South Africa with the aim of reducing poverty and inequality. 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

[8] Improvements in labor market outcomes of recipient mothers is certainly not the main policy justification behind child support grants. However, this is an important aspect, in particular for cash transfers that are targeted to children only in name, without any actual conditionalities attached.

[9] 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 their original paper, Haushofer and Shapiro (2016) have shown that an unconditional cash transfer can have a persistent positive effect on assets.

[10] The *Old Age Pension*, i.e. the public pension system, accounts for slightly more than the CSG in terms of yearly government spending.

pension age without a private pension.[11]

The CSG was proposed by the Lund committee as replacement for the support system existing at the time, the *State Maintenance Grant* (SMG). The SMG was subject to very strict requirements, such that "one parent had to be deceased or maintenance had to be petitioned for in court" (McEwen et al. (2009)). Moreover, having been designed during Apartheid South Africa, this system had a significant racial bias. African children *de facto* did not have access to the grant, which was attributed almost exclusively to Coloured and Indian children (and White to a lesser extent).[12] For both these reasons, overall coverage of the SMG was lower than 1% in the early 1990s.

Table 1: Evolution of the CSG

Reform dates	Age limit	Amount	Amount ('10 R)	Means test
Q2 1998	7	100 R	185 R	1100 R rural, 800 R urban
Q2 2003	9	160 R	218 R	1100 R rural, 800 R urban
Q2 2004	11	170 R	234 R	1100 R rural, 800 R urban
Q2 2005	14	180 R	242 R	1100 R rural, 800 R urban
Q3 2008	14	230 R	257 R	2300 R
Q1 2009	15	240 R	250 R	2400 R
Q1 2010	16	250 R	250 R	2500 R
Q1 2011	17	260 R	248 R	2600 R
Q1 2012	18	280 R	252 R	2800 R

Note: The grant was introduced in April 1998. Column 4 gives the value of the grant in 2010 Rand, adjusting for inflation measured as CPI (*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.
Source: Gomersall (2013) and Eyal and Woolard (2013)

The CSG is an unconditional, means-tested, cash transfer program, where the only eligibility requirements are having a) children of a certain age and b) income lower than a certain threshold. Hence, to be eligible, a grant recipient has to have low enough income and a child who is not older than a given threshold. At the end of the month when 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.[13] Very few documents are required to have access to the grant: an identity card, a birth certificate, and proof of earnings, but this last requirement is flexible as I will discuss in the next paragraph. The grant is paid to the "primary caregiver" of the child, hence it is not exclusive to the parents (contrary to the SMG system in place before). This allows members of the households other than the parents to access the grant, given that they can provide an official document showing they are taking care of the child.[14] In practice, the CSG is paid out almost exclusively to

[11] Coverage of these grants at the household level is presented in Figure A4 in the Appendix, and does not vary significantly over time.

[12] "Kruger (1998) states 0.2% of African children, 1.5% of White children, 4% of Indian children and 4.8% of Coloured children received the state maintenance grant in 1990" (McEwen et al. (2009))

[13] This is true for biological children. For non-biological children, only up to 6 grants can be paid.

[14] The South African government lists the following documents: "If you are not the child's parent, you must provide proof that you are the child's primary caregiver through an affidavit from a police official, a

women,[15] and the biological mother of the child is the direct recipient a large majority of the time. Africans are disproportionately represented among CSG recipients, while close to 0% of recipients are white.[16] This underlines a complete reversal from the SMG system in place during Apartheid.

While the age threshold is strictly applied through the birth certificate requirement, the means-test is not. 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. Consistently, there is very little evidence that it is actually applied. Qualitative research has found "that the various elements of the means test are not generally enforced, understood or relevant." Moreover, "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 et al. (2008)), which make this constraint *de facto* non-binding.[17]

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, especially in the informal sector, as I will discuss later. The average amount per child over the period is around 50 $ '10 PPP, and its 1998 amount is comparable to that of *Progresa* in Mexico for the same year (Bianchi and Bobba (2013)). The CSG was also constantly increased in order to keep up with inflation. In real terms, the amount of the transfer has increased by slightly less than 40% since its implementation.[18] The opposite is true for the means-test of the grant, which was initially set at 1100 R in rural areas and 800 R in urban areas, and has not been changed for the entire period from 1998 to 2008.[19] Finally, in 2008 the means-test was harmonized to 10 times the grant for individual incomes, and at 20 times the grant for the pooled income of the caregiver and his/her spouse, and has not changed since.

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. Lund (2007) links this slow start of the CSG with administrative difficulties and overall confusion, but also to a lack of political will to truly implement the grant as it was intended. The coverage of the CSG did not really take off before the year 2000, when take-up for

social worker's report, an affidavit from the biological parent or a letter from the principal of the school attended by the child." *Source*: South African Social Security Agency (http://www.gov.za/services/child-care-social-benefits/child-support-grant)

[15] In only 2% of the cases a man reports receiving the grant.

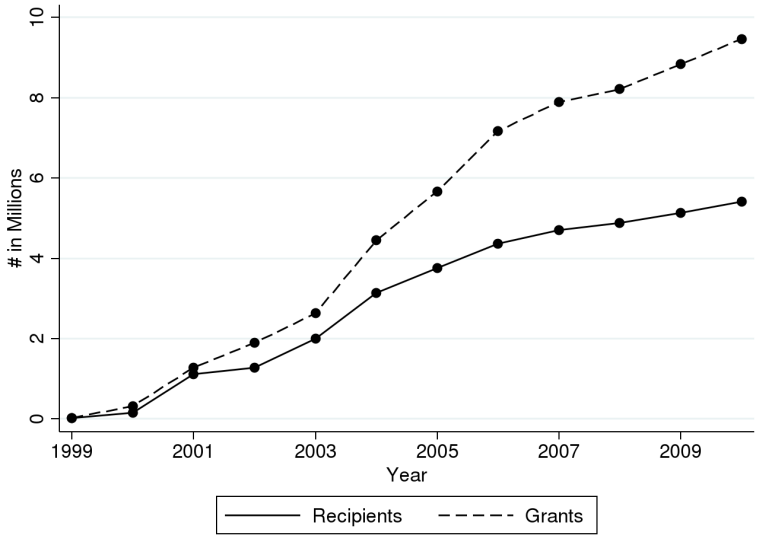
[16] All these descriptive statistics are calculated on the *National Income Dynamics Survey* (NIDS)

[17] The non-bindingness of the means-test in practice does not imply that individuals' perceptions, and therefore behavior, might be affected by its presence. I discuss this more in detail in the Appendix.

[18] The amount of the grant in real terms is obtained by adjusting for inflation, measured by CPI at the national level. CPI data is obtained from OECD.stat.

[19] Surprisingly, the means test threshold was set 30% higher in rural than urban areas, apparently in order to compensate for a lack of access to health and education services in those areas (Lund (2007)).

Figure 1: Number of CSG Recipients, South Africa 1999-2010



Note: This graph plots the number of CSG recipients, i.e. how many people receive at least one grant, and the total number of grants. Mothers can receive one grant for each child who is eligible.
Source: This series is from Gomersall (2013), who uses administrative data provided by the Social Security Agency

eligible children began increasing dramatically. The steepest increase in coverage occurs as from 2003, when the age-eligibility threshold was doubled in only three years from 7 to 14. This dramatic rise has made many more cohorts eligible to the grant. The age threshold was stable from 2005 to 2009, and then gradually increased from 14 to 18 January 1st of every year. Contrary to the previous increase, this raise did not make new cohorts eligible, but rather increased duration for cohorts that were already receiving the grant. The means-test reform that occurred at the end of 2008 does not seem to have led to a discontinuous jump in take-up, which was increasing smoothly as from 2006 and then stabilizes around 2010. This is consistent with the view that the means-test is not strictly applied, and that age-eligibility is often times the only binding criteria.[20]

There is a lack of quantitative evidence on the impacts of *Child Support Grant*, 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), and for a full review see Eyal and Woolard (2013)), finding positive but limited effects on children's outcomes. The absence of significant effects on children might be due to 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 et al. (2016)). Only a few papers have begun to investigate the effects of this program on the labor market outcomes of the parents.[21] More attention has been given to the labor

[20]Further evidence of the non-strict appliance of the means-test is that, prior to 2008, the means-test was significantly more binding in urban areas and for married couple, as in theory the pooled income of both spouses enters the computation. After 2008, when the means-test is equalized across urban/rural regions and doubled for married couple, we would have expected to observe very different evolutions for these subgroups. The four groups who experienced different changes in the nominal means-test are: urban married (+475%), urban non-married (+190%), rural married (+320%) and rural non-married (+110%).

[21]Eyal and Woolard (2011), OECD (2011) find some evidence that the CSG might increase employment.

market effects of other cash transfer programs in South Africa, such as the *Old Age Pension* (OAP). Ardington et al. (2009) find a positive employment effect of the pension through migration from rural areas, but the evidence is mixed.[22]

3 Conceptual Framework

The job search literature provides a useful framework to better understand the results of this paper. With respect to the impact on unemployment and job quality, this literature has mostly focused on unemployment benefits, rather than cash transfers. While unemployment benefits only increase the value of unemployment, an unconditional cash transfer raises both the value of employment and unemployment. Card et al. (2007) show that in a job search model that incorporates assets and saving decisions, a cash grant would result in less search effort and longer unemployment, even with exogenous wages. This will be the case if individuals cannot perfectly smooth consumption between states, while we should observe no reaction otherwise. I re-propose this framework in the Appendix as a reference. This simple model offers a first interesting prediction, which is useful for the analysis presented in this paper: a cash grant should result in longer unemployment for recipients. The other important aspect is the possible effect on subsequent job quality. While this is not the case when wages (or job quality) are taken as exogenous, a similar model but with reservation wages (à la McCall) would possibly predict a higher job quality in the subsequent job. This offers another interesting prediction: a cash transfer should increase reservation wages (or reservation job quality), and potentially increase the quality of the subsequent job.

However, the literature on the impact of unemployment benefits on job quality has found often null, and sometime negative, impacts on job quality. Nekoei and Weber (2017) show that these results can be reconciled with a *directed* search model, which opposes two forces driving the possible impact of longer unemployment on job quality. On the one hand, by making people more selective, higher benefits result in a higher target wage (positive effect on job quality). On the other hand, duration dependence makes people less likely to find good work as unemployment lengthens. Therefore, while the effect of the CSG on unemployment should be zero or positive, the sign of the effect on subsequent job quality is ambiguous. As mentioned before, testing in the data whether cash transfers can lead to persistent job quality gains is a key policy question, both as a rationale for this type of policy, and, more generally, to know whether these grants can be an effective (and cost-effective) way of improving labor market outcomes for disadvantaged groups. This

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 household respond in terms of expenditure when the grant lapses, finding no decrease in expenditure when the child reaches the age eligibility threshold.

[22]The *Old Age Pension* system in South Africa, which provides an unconditional cash transfer, paid to individuals of a certain age regardless of previous pension contributions. Ardington et al. (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 individuals in the household. Moreover, more recent work by Abel (2013), using nationally representative panel data, does not confirm these results, and finds again a negative results on employment.

is not obvious. If there are barriers other than financial constraints (such as low human capital, for example) that are preventing this group from accessing "good" jobs, then the labor market effects of such a policy, at least through the job search channel, should be null or small.

Alternatively, one could also think in terms of an occupational choice model (Bianchi and Bobba (2013), Falco (2014)). The difference from a standard job search model is that individuals only look for jobs in one sector or the other, which in some cases may reflect better the nature of segmented labor markets. This type of model also allows to formalize the possible impact of the grant at the extensive margin of employment. If there are fixed costs to entry into employment, and agents are liquidity constrained, then a cash transfer could potentially increase access to jobs.[23] I develop a simple model, presented in the Appendix, where individuals can search either in the formal or informal sectors, with the former having higher search costs. With this setup, it can be shown easily that a cash grant might have a positive impact on employment, by allowing mothers to cover search costs that they might not otherwise. Moreover, the grant could also push people towards formal sector search, either by allowing people in informal jobs to look for formal sector jobs, or by allowing better/more effective formal sector search and reducing informality as a "fall-back" option.

4 Data and Descriptive Statistics

4.1 Data

This paper combines several data sources to study in detail the labor market impact of the CSG. The main part of the analysis will be conducted on Census data (2001 & 2011) and the Community Survey (2007). For simplicity, I refer to both these sources as Census data, as they are highly comparable.[24] The advantage of this data is its large sample size,[25] and that it contains questions on the date of birth of the youngest child, which allows to have information on CSG exposure regardless of whether the child is observed in the household or not. This is particularly important following research by Hamoudi and Thomas (2014) among others, who show how household composition in South Africa is endogenous to the receipt of social grants. The drawback of using Census data is that information on labor market outcomes only includes the bare essentials. In the Appendix, I outline how the informal sector is measured in the Census, and how it compares to other surveys. To expand on the outcomes, and investigate the mechanisms, I will conduct a similar analysis on the *National Income Dynamics Survey* (NIDS), which is the other data source in South Africa with fertility and labor market information. The advantage of this panel dataset is that it has a very detailed labor market section, which allows to expand the scope of

[23] For example, Ardington et al. (2009) make this argument in the case of the *Old Age Pension* in South Africa, showing that working age adults migrate more when receiving the transfer and are more likely to be employed.

[24] The stated purpose of the Community Survey is to be a smaller census, in order to have some information in-between two census waves, which are 10 years apart, without having to carry out a full census. The questionnaires are very similar.

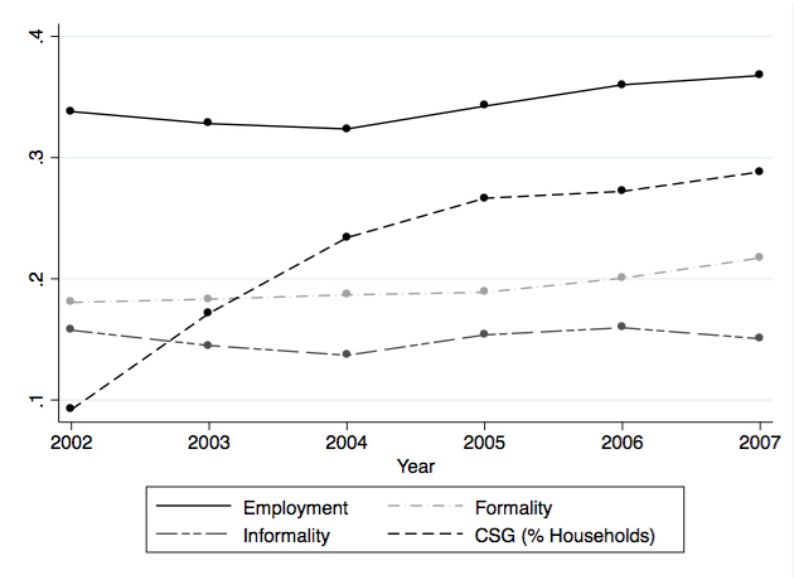
[25] This is a 10% subsample of the overall Census, available for the years 2001 and 2011; the Community Survey is a large survey of roughly 2% of the population.

the analysis, but covers a later time period (2008-2014) and at the cost of a significantly smaller sample size.

For detailed information on wages and CSG receipt over time, I will make use of the South African *Labour Force Survey* (LFS) from 2002 to 2007,[26] and the *General Household Survey* (GHS) over the same period. The LFS and GHS are nationally representative household surveys, with large sample sizes. In the LFS, there is no information on the Child Support Grant, while the GHS does not have detailed questions on informality status. Combined together, they allow to construct time-series both with respect to employment and informality (LFS) and the Child Support Grant (GHS), by sub-population.

4.2 Descriptive Statistics

Figure 2: Female Employment Rates in the Formal and Informal Sectors, South Africa 2002-2007



Note: This graph plots average employment, informality and formality rate and the share of households receiving the CSG in South Africa over the period 2002 to 2007. The informality and formality rate add up to total employment. Informality/formality status is based on business registration.
Source: Author's calculations on GHS and LFS

In South Africa, employment in the informal sector accounts for around 30 per cent of total employment, slightly less than 15 per cent of working age population.[27] Black South Africans are a great majority of the population, and they are overly represented in informal jobs, inactivity and unemployment, and underrepresented in formal jobs (Table A2). On the contrary, Whites are greatly overrepresented in formal jobs and make up very little of the informal and unemployed workforce. With respect to gender, women are as equally present as men amongst the informal and the unemployed, while they make up a great majority of the inactive population and only around 40% of formal employment. Young

[26] I exclude the two initials years of the LFS, 2000-2001, because of the problems of comparability in measurement of informal employment as pointed out in Kerr and Wittenberg (2015).

[27] Average for the 2002-2007 period calculated on the LFS. Instead, the estimates for informal employment are around 35% of total employment, or 17% of working age population.

Table 2: Median Wages by Sector in South Africa, 2010 Rand

Median Monthly (Hourly) Wages	**Formal Sector**	**Informal Sector**	**Gap**
All	2660 R (13,3 R/h)	875 R (5,1 R/h)	1785 R (8,2 R/h)
Employees	2608 R (13,1 R/h)	875 R (5,0 R/h)	1733 R (8,1 R/h)
Self-Employed	6821 R (32,1 R/h)	893 R (5,2 R/h)	5928 R (26,9 R/h)
Employers	7823 R (38,8 R/h)	1412 R (7,9 R/h)	6411 R (30,9 R/h)
Own-Account	3547 R (19,6 R/h)	782 R (4,6 R/h)	2765 R (15,0 R/h)
Men	2728 R (13,4 R/h)	1166 R (6,1 R/h)	1526 R (7,3 R/h)
Women	2457 R (13,2 R/h)	722 R (4,5 R/h)	1735 R (8,7 R/h)

Note: Informal sector refers to individuals employed in non-registered businesses. Median wages are in 2010 Rand. Employers are self-employed individuals with at least one employee, while own-account workers have no employees. Wages are averaged over the reference period 2002 to 2007 for individuals aged 18 to 60. One CSG grant is R 250.
Source: Author's calculations on LFS (2002-2007)

people are particularly concentrated in inactivity and unemployment, and underrepresented in formal jobs. People with no educational attainment are significantly concentrated in informality and inactivity, less in unemployment.

Only around 4% of the working age population is employed in agriculture. These jobs are mostly informal yet they account for a small portion of the stock of informal jobs. Self-employment is predominantly informal, but does not constitute the majority of employment in the informal sector. The public sector accounts for less than 15% of the total stock of formal jobs. On the other hand, more than 30% of formal sector employment is unionized, reflecting the importance of labor unions in South Africa.[28] Moreover, while part-time work is found exclusively in informal jobs, more than 80% of informal workers still work full time. Among those who are formally employed, around 20% have a temporary contract, while the large majority has a permanent contract or one with an undefined duration.

Table 5 presents the median monthly earnings and hourly wages in the formal and informal sectors. Overall, wages are around three times higher for formal than informal employees. Interestingly, decomposing the gender gap in wages between formal and informal employment reveals how women are paid almost the same as men in the formal sector, while they are, on average, paid significantly less in the informal sector. This gap is not explained away by a higher prevalence of part-time amongst women, as hourly earnings are also lower. Median wages for employees and self-employed in the informal sector are

[28]See Magruder (2012) for an interesting analysis of the employment effects of labor unions in South Africa.

very similar, while self-employed in the formal sector report by far the highest wages.

In proportion to wages, the CSG is comparable to around 30% of the median informal wage, and only to 10% of the median wage in the formal sector. This shows again how the amount of the CSG cannot be considered small, in particular relative to returns to informal employment for women. However, it is important to point out that one CSG grant is not sufficiently large to substitute for labor income at the extensive margin, neither formal nor informal: virtually no formal worker earns less than the CSG, and less than 5% of workers in the informal sector do. The same proportions hold even when focusing on Black and Coloured women only. These orders of magnitude are key to interpret the results that I will present in the next section.

5 Empirical Analysis

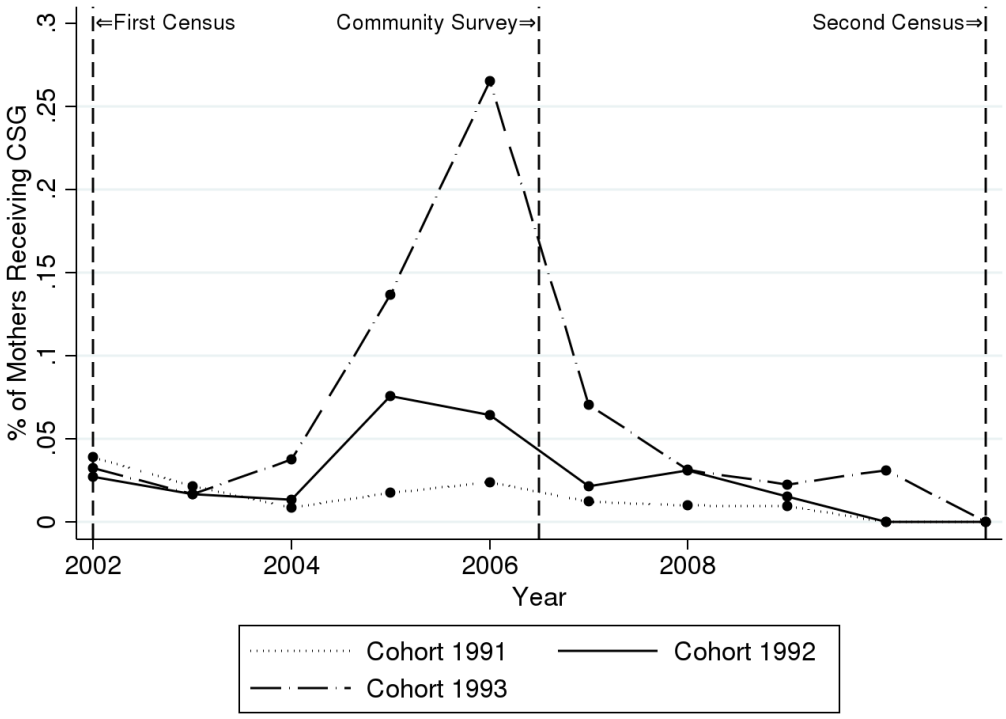
5.1 Identification Strategy

Despite its many appealing features, putting in place a sound identification strategy for the *Child Support Grant* is not an easy task, as shown by the relatively little attention this policy 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. The basic idea is that some cohorts are always older than the age threshold and can never receive the grant. Instead, there are other cohorts who were not initially eligible, but, because of the reforms, become eligible for some periods. As shown in Figure 3, exposure to the grant is largely determined by the cohort of birth of the child, and varies drastically even for children born only one year apart. As I show in Figure A5, this large variation in take-up is clearly the result of changes in age eligibility. The large spike that we observe for cohort 1993 in 2006 matches the peak in eligibility. We also observe that cohort 1991 and 1992 are eligible for some months, 5 and 14 respectively, but in practice never receive the CSG to a significant extent.[29] This occurs probably because these short periods of eligibility are not long enough to induce any actual take-up for the grant. In this setting, what matters is that the large variation across adjacent cohorts of Figure 3 is the result of exogenous changes in policy, which I argue is the case.

It is key to understand here that these differences in CSG exposure occur, on average, for cohorts in adjacent years, not adjacent months: individuals born one year apart can have large differences in the eligibility and take-up of the CSG, but probably not individuals born one month before or after. In the same way, individuals born in the same year, but in months very far apart (January vs. December, for example), may also experience drastically different eligibility and presumably take-up too. Unfortunately, to the best of my knowledge, there is no data that allows to reconstruct CSG take-up by month of birth. Therefore, I will always focus on average differences across individuals born in different, discrete years.

[29] Instead, cohort 1993 is eligible for 30 months overall.

Figure 3: CSG Take-Up by Cohort of Birth, 2002-2011



Note: This graph gives the average take-up of mothers whose youngest child was born in 1991, 1992 or 1993 for the period 2002-2011, in the month of July, when the GHS takes place. Take-up before 2002 was virtually zero for these cohorts. Children born in 1992 are eligible on average for 14 months. On the contrary, cohorts born in 1993 are eligible for 30 months, of which the full year in 2006, and gradually lose eligibility in 2007. Children born after 1993 are eligible for longer periods.
Source: Author's calculations on GHS

Given this framework, the first instinct 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. I show a simple proof of this in the Appendix.

To solve this problem, I exploit the large, discontinuous spike in take-up observed in Figure 1 of cohort 1993 relative to 1992. In the spirit of a Regression Discontinuity (RD) design, I estimate a regression where the forcing variable is the discrete cohort of the child and the threshold is set at cohort 1993. The advantage of this approach, compared to a diff-in-diff, is that any age effect should be captured away by the functional form on both side 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, i.e. those children born in 1992, and a first treated cohort, i.e. 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 cohort 1992 and 1993, in the order

of 0.2-0.3 in 2006.[30] Those children born after 1993 are eligible for increasingly longer periods.

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 labor market outcomes of the mother. This can be easily checked by using the three cross-sections at my disposal: one in late 2001, before any CSG, one in early 2007, during the large spike, and one in 2011, after. Overall, this estimation should be understood as capturing two effects: in 2007, it should capture the effect of receiving the CSG as opposed to not receiving it. In 2011, it should capture the effect of having received the CSG for one year as opposed to never, i.e. the long term effect. Formally, I estimate the following equation, separately for 2001, 2007 and 2011:

$$Y_i = f(c_i - 1993) + \mathbb{1}\{c_i \geqslant 1993\} \times f(c_i - 1993) + \beta_1 \mathbb{1}\{c_i \geqslant 1993\} + X' + \epsilon_i \quad (1)$$

where Y_i is the outcome of interest for the mother of a child born in a given year; f is a function of the cohort of the youngest child centered at the cut-off point. I then introduce a binary variable for individuals whose youngest child is born after 1993, and interact it with the cohort of birth of the youngest child born. β_1 should capture the marginal effect of receiving the CSG in 2007, and of having had some access to the CSG as opposed to none in 2011. X' is a vector of covariates including a control for household size and dummies for age, education, race, marital status and municipality. Standard errors are clustered at the household level.[31]

All estimations are performed on the sample of Black and Coloured mothers. As White mothers do not receive the CSG, I will use this sub-population to perform a placebo test. Unless otherwise indicated, the forcing variable is always the youngest child born to a given mother, regardless of whether of this child is observed in the household or not. The advantage of this approach, which is possible using fertility information in the Census, is that I do not need to observe a child in the household to know whether the mother was treated or not. This takes away the concern that the grant might lead to changes in household composition (Ardington et al. (2009), Hamoudi and Thomas (2014)), which could introduce selection bias. The disadvantage is that fertility information is only asked to women under 50 years of age, so there is no information for women older than this threshold.[32] Therefore, all estimations are run on the sub-sample of women born between 1960 and 1985, for whom I have both fertility and labor market information in the three

[30] One could, in theory, exclude some cohorts in order to increase this differential. For example, excluding cohort 1992 or excluding cohort 1993. However, while this estimation would certainly give a larger first stage, it would also compare mothers whose child's cohorts are further apart, and hence less likely to be similar. However, taking out cohort 1992 does not qualitatively change the results.

[31] Lee and Card (2008) initially suggested that 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 my estimations, the standard errors always drop significantly when clustering by the running variable. For this reason, I only cluster at the household level and not at the child cohort level.

[32] This makes the data censored at age 50. I 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 later in the Section.

Census waves.[33]

5.2 Effects of the CSG on Employment and Sectoral Allocation

The results are presented in Table 3. Encouragingly, we observe that in 2001, before the full roll-out of the CSG, there is no discontinuity at the threshold neither in the employment level nor its composition. This suggests that mothers of cohorts at the threshold were comparable in terms of labor market outcomes before the grant was received. It also works as a placebo test, as it suggests that the proposed specification does not pick up effects where there should be none. In the next subsections, I comment the effects in 2007, therefore when the CSG was still received by cohort 1993, and in 2011, five years after.

5.2.1 Effects of Receiving the CSG

The "during" results are presented in the middle panel of Table 3. We observe a significant and large jump in unemployment at the threshold, matched by a symmetric drop in formal sector employment. None of the other coefficients are significant, but the drop in overall employment is around 2pp. Graphical evidence of these effects is presented in Figures A6 to A9. The coefficient on informality is positive, but insignificant and closer to zero when using other windows. These effects are entirely concentrated on mothers, as shown in Table A5. Other household members, on average, do not respond to the CSG in terms of labor market outcomes. With respect to employment and unemployment, it would seem that only the direct recipients, i.e. mothers, are affected. This might be surprising, as it contrasts with a unitary household model where the grant should be pooled into household income.

These results indicate that the receiving the CSG has an unambiguously positive effect on unemployment. Mothers who receive the grant search more. This comes at the expense of formal sector employment, implying that those mothers who search more would have been formally employed in the absence of the grant. This higher unemployment is presumably the result of a longer job search.[34] This result might appear counter-intuitive at first, as I have shown in the previous section that formal jobs pay significantly more than informal jobs. If there were to be disincentive effects on employment, one would expect these to be concentrated on the lower paid, informal jobs. It is possible that the higher flexibility of informal jobs does not require a decrease at the extensive margin, meaning that people can search while also being informally employed. As I do not observe working hours or on-the-job search, I cannot test this hypothesis directly. Alternatively, this heterogeneity across informality might just reflect that workers in formal and informal jobs are inherently different across other dimensions. Nonetheless, these results match overall the predictions of a job search model, as discussed before. In the presence of liquidity constraints, or reservation wages in a more complete model, an exogenous cash grant should raise unemployment duration, which is indeed what I observe.

[33] Labor market information is only collected in the Census for individuals older than 15 years old.

[34] A higher unemployment rate can be the result of a lower flow out of unemployment, i.e. longer unemployment spell, or a higher flow into unemployment, i.e. a higher separation rate. Unfortunately, I have no way to test this using this data, but the first explanation, given the size of the CSG with respect to formal sector wages, seems more plausible.

Table 3: Labor Market Effects of the CSG on Mothers, 2001, 2007 & 2011

	Year 2001 - "Before"				
	(1) Active	(2) Unemployed	(3) Employed	(4) Informal	(5) Formal
CSG	-0.0018	-0.0029	0.0011	0.0016	0.0002
	(0.0046)	(0.0051)	(0.0051)	(0.0040)	(0.0042)
Mean Y at Threshold	0.7397	0.3561	0.3835	0.1548	0.2286
Weighted n at Threshold	194,868	194,868	194,868	194,868	194,868
Observations	477,466	477,466	477,466	477,466	477,466
R-squared	0.1495	0.0656	0.1786	0.0840	0.2085
	Year 2007 - "During"				
	(1) Active	(2) Unemployed	(3) Employed	(4) Informal	(5) Formal
CSG	0.0078	0.0267**	-0.0190	0.0122	-0.0312***
	(0.0114)	(0.0115)	(0.0133)	(0.0110)	(0.0117)
Mean Y at Threshold	0.7600	0.2231	0.5369	0.2101	0.3268
Weighted n at Threshold	133,611	133,611	133,611	133,611	133,611
Observations	90,084	90,084	90,084	90,084	90,084
R-squared	0.1160	0.0857	0.1520	0.0619	0.2050
	Year 2011 - "After"				
	(1) Active	(2) Unemployed	(3) Employed	(4) Informal	(5) Formal
CSG	-0.0026	-0.0053	0.0027	-0.0114**	0.0141**
	(0.0069)	(0.0056)	(0.0071)	(0.0057)	(0.0063)
Mean Y at Threshold	0.6517	0.1660	0.4857	0.1821	0.3036
Weighted n at Threshold	92,079	92,079	92,079	92,079	92,079
Observations	247,032	247,032	247,032	247,032	247,032
R-squared	0.1288	0.0421	0.1499	0.0374	0.1838

Note: *** p<0.01, ** p<0.05, * p<0.1. This table gives the OLS estimates of Equation 1 on mothers' labor market outcomes in 2001, 2007 and 2011 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 quadratic for the window of cohorts born between 1981 to 2004. CSG is a binary variable for the child being born in or after 1993, which indicates being part of a cohort that had access to the CSG. Mean Y at Threshold gives the mean of the outcome for the cohort 1992 (last unexposed cohort). Weighted n at Threshold gives the size of the underlying weighted population for cohort 1992. All estimations include controls for: age (cubic), education, race, marital status, municipality and household size. Standard errors are clustered at the household level.
Source: Author's calculations on Census 2001 & 2011, and Community Survey (2007)

5.2.2 Persistent Effects of the CSG

Figure 4: Share of Informal Employment by Birth Cohort of Youngest Child, 2001 & 2011

(a) Year 2001 - "Before" (b) Year 2011 - "After"

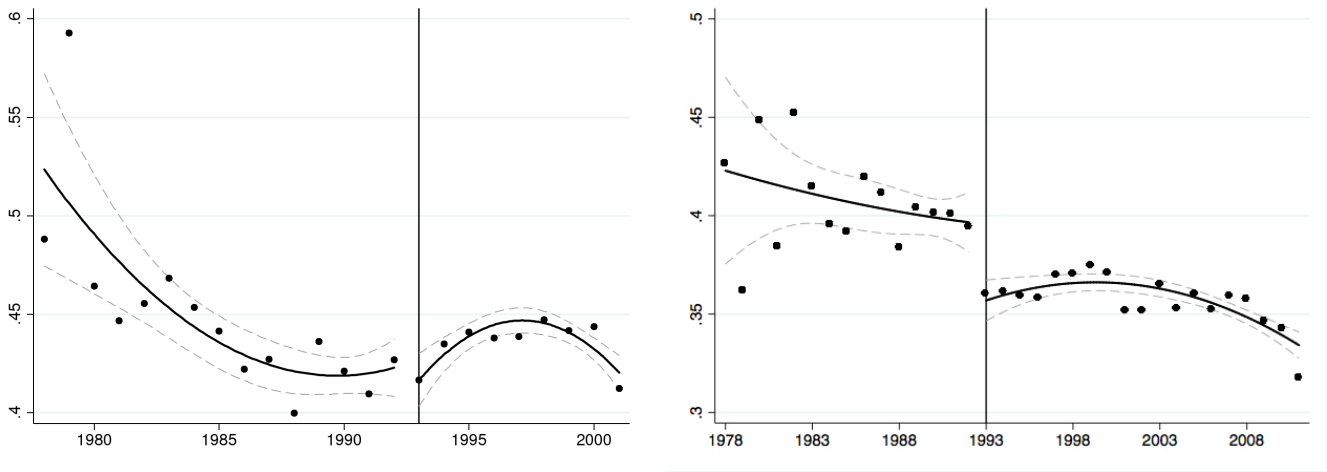
Note: This graph gives the probability of being employed in the informal sector, conditional on being employed, for mothers by cohort of birth of the youngest child ever born, in 2001 and 2011 respectively. Therefore, before and after the CSG was fully rolled out. A quadratic function is fitted on each side of the threshold with 95% confidence intervals. As fertility information in Census data is censored at age 50, I only include mothers aged 39 or younger in the 2001 sample to compare the same population over time.
Source: Census (2001 & 2011)

I now explore to what extent this higher unemployment translates into job quality gains for recipients. The "after" results are presented in the bottom panel of Table 3. In 2011, mothers whose child was born in 1993 are as likely to be employed as mothers who did not have any access to the CSG. Any disencentive effect on employment did not outlast the grant. However, the composition of employment between mothers of exposed and non-exposed cohorts is not the same. Among the employed, those who had access to the CSG are around 1.1 percentage point less likely to be working informally, and, symmetrically, around 1.4 pp. more likely to be working in the formal sector. Again, Figures A6 to A9 replicate the results of the table in graphical form. However, the clearer indication of the effect on the composition of employment appears already when looking at the share of informal employment, which I present in Figure 4. While in 2001, we observe no significant difference in the composition of employment, in 2011 mothers of exposed cohorts are around 5-6% less likely to be holding an informal job rather than a formal one. This decrease in the share of informal employment is significantly more marked for single mothers, as can be seen in Figure 5. For this subgroup, the decrease in informality is almost 10% at the threshold. This heterogeneity may not be entirely surprising as single mothers are likely to be the most credit constrained or unable to stay unemployed and look for a job, given that they cannot count on any income support from a spouse. Again, I do not find any evidence of an effect for people who are not the direct recipient. Consistently with the previous results, these persistent effects are also entirely concentrated on mothers.

The first take-away is that the CSG did not have a persistent positive (or negative) impact on the employment rate. This finding could be an indication that the grant might

Figure 5: Share of Informal Employment by Birth Cohort, Single Mothers Only, 2001 & 2011

(a) Year 2001 - "Before" (b) Year 2011 - "After"



Note: This graph gives the probability of being employed in the informal sector, conditional on being employed, for single mothers in 2001 and 2011 by cohort of birth of the youngest child ever born. A quadratic function is fitted on each side of the threshold with 95% confidence intervals.
Source: Census (2001 & 2011)

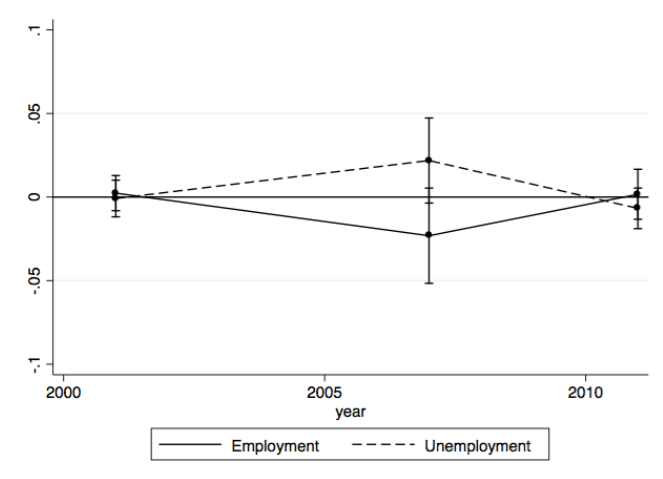
not be sufficient to overcome fixed cost to access employment, or these frictions do not prevent entry into employment at the extensive margin. On the contrary, the result on job quality, proxied by informality, suggests that the grant leads to a different, and presumably better, sectoral allocation. The fact that we observe these differences up to five years after the CSG has stopped for the cohort 1993 should be emphasized. This suggests that the positive effects on job quality are potentially long-lasting, as they do not dissipate after a significant amount of time.

In Table A4 I decompose this effect by occupational status and sector. The long-term increase in formal wage employment comes in equal parts from informal wage- and self-employment.[35] Contrary to Bianchi and Bobba (2013), I do not find any evidence that access to a cash transfer increases self-employment in South Africa. Abel (2013) finds similar results in the case of the *Old Age Pension*, where self-employment does not increase when an household member gains access to the pension. This is an indication that credit/liquidity constraints might not the main reason behind the stunningly low rate of self-employment in South Africa.

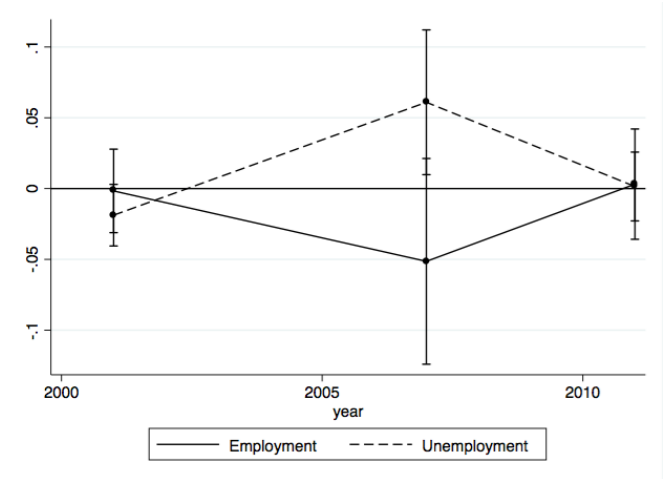
[35]One could also wonder to what extent this is a "real" increase, meaning whether it implies an actual change in the type of jobs mothers hold. In Table A3, I decompose the effect by industry and occupation. The table shows that, of the total decrease of 2.5 pp. in the share of informal employment, more than half is a change in industry, as mothers are less likely to be employed by private households. The remainder of the effect occurs instead within occupation, rather than across occupations. Hence, the persisent effect can be decomposed in two: the CSG makes mothers less likely to be employed by private households and to hold a formal job in other industries. Also, for a given occupation outside private households, the grant makes it more likely to be in a formal sector job rather than in an informal one.

Figure 6: CSG Effect on Labor Market Outcomes: Before, During, After

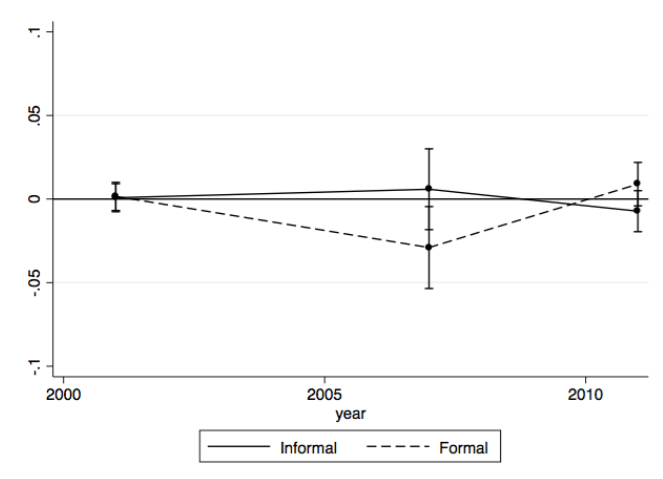
a) Black Mothers



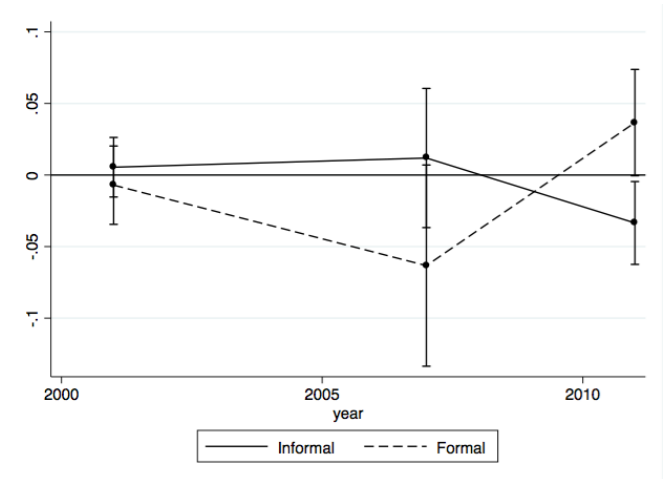
b) Coloured Mothers



a) Black Mothers



b) Coloured Mothers



Note: These graphs plot the coefficients of Equation 1 for the Black and Coloured population separately, and by year. The outcomes are employment & unemployment (upper panel), formal & informal employment (lower panel). The vertical bars indicate 95% confidence intervals.

5.2.3 Heterogeneous Effects by Population Group

In Figure 6, I explore heterogeneous effects by population groups, dividing between the populations of Black and Coloured mothers. The advantage of focusing on this heterogeneity is that this categorization in population groups is not time-changing, as opposed to other variables, such as, for example, education, marital status etc. Using this dimension allows me to be sure that the underlying population is the same in the three years, and explore whether there is a match between the size of the "during" and "after" effect by sub-population. These graphs plot the coefficients of Equation 1 for each year and by population group. Overall, they confirm the effect dynamics presented before: for both groups, we observe a drop in formal employment in 2007, matched by a large increase in unemployment. In 2011, employment and unemployment are back to the same level of untreated cohorts, but formal employment is higher, and informal employment is lower.

Both "during" and "after", the effect of the grant is particularly strong on the Coloured population. This is a small subgroup of the population, around 5% overall, with significantly better labor market outcomes on average. Encouragingly, this implies that the subgroup for whom the "during" effect is stronger is also the one for whom the persistent effect is larger. This hints that we are not observing two separate responses, in 2007 and 2011, but rather a dynamic, sequential effect, where higher formal employment after the grant comes from higher unemployment when the grant is received. Indeed, we observe a very large drop in employment (and a very large increase in unemployment) for Coloured in 2007. Consistently, for this population group the long term effect is three times larger than for the Black population. For those familiar with the South African labor market, this is perhaps not surprising, as the Coloured population overlaps well with that marginal population with access to formal jobs, yet liquidity constrained enough that a small cash transfer could make the difference.

5.2.4 Heterogeneous Effects by Years of Eligibility

Having established the main results, I now relax the identification strategy to exploit the large variation across treated cohorts in terms of exposure to the CSG. As shown in the Appendix, a difference-in-differences estimation likely suffers from a bias due to age effects when comparing different cohorts over time. With some caution, I still recur to this estimation both as a robustness check and to extend the analysis to those cohorts that have benefited from longer eligibility. This allows to explore an important dimension of heterogeneity, whether receiving money for a longer period results in higher job quality gains. Formally, I now estimate the following equation:

$$Y_i = \mathbb{1}\{c_i \geqslant 1993\} + \sum_{t=1}^{2} \mathbb{1}\{year = t\} + \mathbb{1}\{c_i \geqslant 1993\} \times \sum_{t=1}^{2} \beta_t \mathbb{1}\{year = t\} + X' + \epsilon_i \quad (2)$$

The results of this estimation, for different cohort windows, are presented in Table A6. For the smallest possible window, comparing the evolution of mothers with a youngest child born in 1992 vs. 1993, the results replicate, for the most part, those presented before. Mothers receiving the grant are more likely to be unemployed. The main difference concerns the persistent results. With this estimation, the increase in formal employment appears as an increase in net employment, rather than as a decrease in informal employment. This tells a slightly different story, where the CSG has a positive effect on the employment rate. However, for the reasons mentioned before, an RDD-like estimator is more trustworthy in this setting.[36]

Relaxing the constraints of the RDD estimation allows to explore further how mothers respond to longer exposure to the grant. First, I compare the relative evolution of cohort 1993 vs. cohort 1994. These cohorts also experience dramatically different exposures, as shown in Figure A5. While those mothers whose child is born in 1993 can receive the grant

[36] The lack of a negative coefficient on informal employment (or, symmetrically, the presence of a positive and significant effect on employment) is likely the result of age bias in the estimator, as can be seen in the Figure A6 the child cohort function is much more "steep" in 2001 than 2011, because children are younger, and age effects stronger.

for 30 months in total on average, those whose child is born just a year after experience, on average, 60 months of eligibility. Despite this variation, which reflects in large differences in take-up, I do not observe a statistically different evolution in terms of labor market outcomes over the period. Similar evidence is presented in Figure A10, extending this analysis to cohorts further away from the threshold. These results suggest that the effect might be strongly non-linear, and that longer exposure to the grant does not lead to greater job quality gains. Overall, from the results presented, it would seem that what matters in terms of job quality gains is getting at least one year of grant.

5.2.5 Predicted Effect on Job Stability

The South African labor market is characterized by large worker flows and by high churning, i.e. workers flows that are not justified by job reallocation (Banerjee et al. (2008)). This stylized fact is true even when focusing only on formal jobs, as Kerr (2018) confirms using administrative data on the universe of formal jobs in South Africa. Worker flows and churning are particularly high, which contrasts with the popular view that the South African labor market is not flexible. I can easily confirm this stylized fact by using the NIDS: for the relevant subgroup, over a two year period between 2008 and 2014, the probability of transitioning out of a formal job is around 30%. The probability of "falling" into informality from a formal job, 15%, is as high as flows into inactivity and unemployment combined.

Therefore, one argument is that the CSG could allow mothers to look for more stable formal jobs. This would explain why mothers are more willing to search for longer in 2007, and why the share of formal jobs is higher in 2011 for those who have received the grant. In order to investigate this further, I exploit the longitudinal dimension of the NIDS. I use this panel dataset to calculate the "stability" of formal jobs by occupation and industry, two variables that are also collected in the census. As a proxy for stability, I use retention rates over a two-year period (the time span between two waves in the NIDS).[37] I replicate the main results but separating between more and less stable formal job.[38] These results are presented in Table 4. The drop in formal jobs in 2007 comes from the most part from formal jobs in the bottom half of retention rates, while the increase in 2011 benefits occupations and industries where formal jobs tend to be more stable.

These results suggest that the grant allows recipient mothers to look longer, instead of taking (or targeting) less stable formal jobs. This could the result of mothers being able to look for a longer period without hitting a cash constraint, as the increase in the unemployment rate would suggests. Overall, this appears to reduce the probability of falling into informality in the years after the grant has stopped, which would justify why I observe a higher stock of formal sector jobs and a lower stock of informal sector jobs five years after the grant has lapsed.

[37] These retention rates are calculated for the relevant sub-population of Black and Coloured mothers.

[38] I define a stable formal job as one in an occupation and industry category where retention rates are above the median, and the opposite for an unstable formal job, in a way that splits the formal workforce in two, roughly equal parts.

Table 4: CSG & Job Stability, 2007 & 2011

	Year 2007 - "During"					
	(1)	(2)	(3)	(4)	(5)	(6)
	Active	Unemployed	Employed	Informal	Formal Stable	Formal Unstable
CSG	0.0109	0.0244*	-0.0135	0.0137	-0.0067	-0.0206*
	(0.0123)	(0.0124)	(0.0138)	(0.0112)	(0.0090)	(0.0105)
Mean Y at Threshold	0.7342	0.2469	0.4873	0.1891	0.1329	0.1651
Observations	82,392	82,392	82,392	82,392	82,392	82,392
R-squared	0.1239	0.0892	0.1617	0.0604	0.1899	0.0592
	Year 2011 - "After"					
	(1)	(2)	(3)	(4)	(5)	(6)
	Active	Unemployed	Employed	Informal	Formal Stable	Formal Unstable
CSG	-0.0027	-0.0054	0.0027	-0.0115**	0.0112**	0.0030
	(0.0069)	(0.0056)	(0.0071)	(0.0057)	(0.0048)	(0.0054)
Mean Y at Threshold	0.6508	0.1661	0.4847	0.1818	0.1397	0.1631
Observations	246,461	246,461	246,461	246,461	246,461	246,461
R-squared	0.1285	0.0421	0.1495	0.0375	0.1553	0.0464

Note: *** p<0.01, ** p<0.05, * p<0.1. This table gives the OLS estimates of Equation 1 on mothers' labor market outcomes in 2007 and 2011 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 quadratic for the window of cohorts from 1981 to 2004. CSG is a binary variable for the child being born in or after 1993, which indicates being part of a cohort that had access to the CSG. Mean Y at Threshold gives the mean of the outcome for the cohort 1992 (last unexposed cohort). **Stable** formal jobs are those with the highest retention rates as calculated on the NIDS (above median), while **unstable** are those below the median. Observations for whom occupation and industry information are missing are dropped from the sample. All estimations include controls for: age (cubic), education, race, marital status, municipality and household size. Standard errors are clustered at the household level.
Source: Author's calculations on Census 2011 and Community Survey (2007)

5.2.6 Predicted Effect on Wages

I also explore to what extent this positive effect on job quality, as proxied by an increase in formal sector employment, reflects into higher wages. Unfortunately, census data does not have a direct measure of wages.[39] Therefore, I impute wages by using occupation and industry information, and calculating median wages for all industry×occupation cells in the NIDS, on the relevant population subgroup. I then perform the same estimation as in Equation 1, presented in Table 5. This should be interpreted as the predicted increase in wage, not as the actual increase, which could be larger or smaller. In Column 6, I estimate a positive, predicted increase in the order of magnitude of around R 60 per

[39]Census data measures a categorical, ordinal variable for income category both at the individual and household level. However, this measure is very imprecise, meaning that the income brackets are very large, hence not refined enough for this analysis.

month. This result is mechanical and should not be surprising, formal sector jobs pay on average significantly more. The increase in predicted wages follows the increase in formal sector employment for treated mothers. Therefore, this suggests, but does not necessarily imply, that actual wages have increased. Nonetheless, this estimation still provides an interesting number, as it allows to calculate whether the increase in wages is supposedly larger than the cost of one year of grant.

This number can be used for some back-of-the-envelope calculations about the cost-effectiveness of one year of grant, although this require several assumptions. I take as a measure of the take-up differential between the first and last unexposed cohort (1993 vs. 1992), 0.3, which is approximately what we observe in Figure 3. More importantly, this calculation requires knowing how long this positive wage effect has been present, as we only observe the 2011 cross-section. If we assume that this effect has been persistent for 5 years, this would imply that the wage gains for recipients are three times the cost of one year of grant.[40] Pushing this argument further, if what matters for job quality gains is receiving the grant for at least one year and not more, as the previous diff-in-diff estimation suggests, this would imply that wage benefits would outweigh the cost up to four years of grant, and become negative afterwards. However, these calculations are biased upwards, as they ignore foregone earnings due to longer unemployment, which would decrease the net benefits from the grant.[41]

Table 5: CSG & Wages, 2011

	Year 2011 - "After"					
	(1)	(2)	(3)	(4)	(5)	(6)
	Active	Unemployed	Employed	Informal	Formal	Predicted Wage
CSG	-0.0038	-0.0046	0.0008	-0.0115*	0.0123**	58.3179*
	(0.0071)	(0.0058)	(0.0072)	(0.0059)	(0.0063)	(33.0430)
Mean Y at Threshold	0.6513	0.1658	0.4855	0.1818	0.1395	1385.432
Observations	237,533	237,533	237,533	237,533	237,533	236,287
R-squared	0.1291	0.0447	0.1542	0.0388	0.1952	0.1972

Note: *** p<0.01, ** p<0.05, * p<0.1. This table gives the OLS estimates of Equation 1 on mothers' labor market outcomes 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 quadratic for the window of cohorts from 1981 to 2004. CSG is a binary variable for the child being born in or after 1993, which indicates being part of a cohort that had access to the CSG. Mean Y at Threshold gives the mean of the outcome for the cohort 1992 (last unexposed cohort). **Predicted Wage** is the median monthly wage by occupation and industry as calculated in the NIDS. All estimations include controls for: age (cubic), education, race, marital status, municipality and household size. Standard errors are clustered at the household level.
Source: Author's calculations on Census 2011

[40] The cost of the grant per person is R 3000 over one year for one recipient (R 250×12), the net benefit for recipients is R 12 000 ($\approx 60/0.3 \times 5 \times 12 = 12000$).

[41] Taking median wages in the formal sector as a reference, the predicted wage benefit over 5 years is equivalent to 5-6 months of foregone earnings due to longer unemployment.

5.3 Robustness Checks

I perform several robustness checks for these estimations, presented in the Appendix. One concern is that fertility decisions might be impacted by the the grant. Even though the roll-out 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. To alleviate this concern, I check that the log density of cohorts around the threshold, in the spirit of McCrary (2008). Graphical evidence, presented in Figure A14, shows that the there is no discontinuity at the threshold neither before, nor during, nor after the roll-out of the grant. Fertility data in the Census is 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 cut-off point. Again, the smoothness of the density around the threshold suggests that this is not the case.

I also look at the distribution of pre-determined observables around the threshold. This is a standard check in an RDD-like analysis and should serve as an additional confirmation that individuals are comparable on each side of the discontinuity. Figures A11 to A13 in the Appendix shows that the observables are well-balanced, and there is no jump in relevant covariates around the threshold: the share of mothers who are Black, married, have migrated, and their age, education and household size evolve continuously around the cut-off point, which suggests that we should not expect any discontinuity in labor market outcomes if not for the CSG. These graphs also show that in 2011, the "after" period, the composition of those employed has not changed: observables characteristics are comparable at the threshold for the sub-population who is employed. This indicates that the persistent effect is not a result of a change in the composition of the workforce, where informal workers would drop out of the labor force and formal workers "drop-in".

I also check the robustness of the results to the bandwidth size and functional specification, by gradually reducing the number of cohorts included in the estimation with a quadratic and linear fit respectively. These results are presented in Figures A15 and A16. Overall, this sensitivity analysis confirms the results presented before. Moreover, I perform a placebo test exploiting the fact that CSG take-up for whites is virtually zero.[42] I estimate Equation 1 on White mothers only and check that there is no discontinuity at the threshold with respect to labor market outcomes (Figure A17). Consistently, I find no significant jump at the threshold for this subgroup.

One concern could be that there are discontinuities in mothers' employment (formal and/or informal), that are due to the age of the child. Given that, at a given point in time, cohort and age are perfectly collinear, I cannot control for age of the child while estimating Equation 1. This concern can be easily dealt with by performing placebo estimations at similar age values in different years. For example, in Figure A23, I show the share of

[42] It is difficult to know from survey data whether this occurs because they do not apply or because there is an implicit rule that they should not access the CSG. Regardless, this group provides a good placebo check.

informal employment around a threshold set for cohort 1983 in 2001 (hence exactly the same age as cohort 1993 in 2011)[43]. Clearly, in 2001 we do not observe the drop in informal employment that we observe in 2011. More generally, I run a set of placebo estimations by varying both the bandwidth and the position of the threshold around cohort 1993. This should give an idea of how large are the jumps estimated at the "correct" threshold relative to other, "misplaced" thresholds. I present these in Figure A22 in the Appendix for the three relevant outcomes: unemployment, informal employment and formal employment. For all three outcomes the distributions of coefficients with placebo threshold are centered around zero, and the coefficients estimated with the "correct" threshold are, in most cases, above (below) the 95th (5th) percentile of the distribution.[44]

6 Discussion

This paper provides evidence that an unconditional cash transfer program can have lasting, positive effects on job quality for a disadvantaged group in a segmented labor market. We also observe that these improvements come from higher unemployment while receiving the transfer. This suggests that mothers respond to this extra cash-on-hand by becoming more selective, as a standard, or *directed*, job search model would predict. Another prediction of a search model à la McCall is that the grant should raise reservation wages. I do not observe this is the case; self-reported reservation wages, which are available in NIDS data, do not appear to be correlated with eligibility and take-up of the *Child Support Grant*. As shown in Figure A24 for the unemployed subpopulation, reservation wages are flat with respect to the year of birth of the youngest child, while CSG receipt is a strongly positive function. Generally, there are concerns about the validity of self-reported measures of reservation wages, so the lack of correlation might be the result of measurement error. An alternative explanation is that mothers might value aspects of job quality other than wage (formality, job stability etc.), and therefore that a measure of "reservation job quality" would be more appropriate.

Instead, I do observe that take-up of the grant is strongly correlated with transport expenses when looking for a job. As can be observed in Figure A25, mothers whose child is eligible for the grant tend to spend significantly more on transport when unemployed. This is not a correlation that we observe for men, which would explain why the effects are entirely concentrated on women, the direct recipients of the grant. Together with the increase in unemployment that we observe in the "during" period, this suggests that mothers are looking for different kind of jobs when receiving the transfer. Overall, this is more in line with what a *directed* search model would predict: because of the transfers, mothers are becoming more selective and target "better" jobs, which leads them to spend more on transport overall. Moreover, the results of this paper are qualitatively similar to those of transport subsidies in other African labor markets (Franklin (2016), Abebe et al. (2017)), which find persistent positive effects on job quality for this type of intervention. Contrary to Ardington et al. (2009), I do not find that these results are driven by labor

[43] Census 2001 and 2011 take place in the same month, October.

[44] 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.

migration. Mothers who have received the transfer are as likely to have migrated as those who have never been eligible to the CSG. This should not be surprising given the size of the income effect provided by the grant, which is probably not large enough to induce migration responses.

This evidence does not exclude that the effect might go through channels other than job search. In the limited set of observables that I can test, I do not find that the grant changes the characteristics of the mother, the child or the household in a way that would justify these improvements in labor market outcomes. Yet, it is certainly possible the grant has an impact on other intermediary outcomes. However, the evidence seems to suggest that job search is the main mechanism at play behind the labor market effects. For example, for subpopulation of unemployed mothers, the correlation between the cohort of the child and transport expense implies an elasticity between the grant and transport expenses around 0.7, meaning that most of the money seems to be spent on transport expenses looking for a job.[45] A correlation close to 1 would imply that all the money goes to transport expenses. This relationship is robust to the introduction of several controls, suggesting that the increase in expense might indeed be the result of money coming from the grant. Again, the grant might surely impact other outcomes beyond the labor market, but there is direct evidence that it strongly impacts job search, suggesting that this may be the main channel behind its labor market effects.

Testing with precision the role of each mechanism is difficult for two reasons. On the one hand, the data at my disposal, while appropriate for this analysis, is fairly limited in the number of outcomes it captures. On the other hand, an unconditional cash transfer is potentially related to an (almost) infinite number of outcomes. No amount of data would cover to the full extent all possible outcomes. This is the drawback of looking at inherently "general" intervention. Nonetheless, the advantage of this program is that it allows to answer two interesting empirical questions. First, whether an exogenous cash transfer changes job search behavior, and potentially leads to higher job quality, as many studies have tested in the context of developed countries. I provide clear evidence this is the case, and that it does so in a very similar way as standard job search models would predict. The second question is whether individuals respond to positive income shock in terms of their allocation between the formal and informal sectors. If individuals sort exclusively according to their comparative advantage, this should not be the case. Instead, we clearly observe that cash transfer recipients are more likely to end up in formal sector jobs, at least in the medium term, which provides support for a negative view of segmented labor markets.

7 Conclusion

This paper has studied how an unconditional cash transfer, the South African *Child Support Grant*, impacts labor market outcomes for women, with a focus on the allocation between

[45] This elasticity is estimated only by using mothers with one child, in order to avoid the issue of multiple grants.

the formal and informal sector. By exploiting cohort discontinuities in access to the grant, I find that mothers of exposed cohorts are more likely to look for a job when receiving the grant, and less likely to work. Five years after the grant was received, the employment rate is the same, but those mothers who have received the grant are more likely to be employed in the formal sector. This persistent effect on job quality is particularly large among single mothers, who seems to benefit the most in terms of gaining access to better employment.

This paper contributes to the growing empirical literature on search frictions in developing countries, while showing that unconditional cash transfers can be a way to overcome them. The main contribution of the paper is to show these effects within the context of a non-targeted, unconditional transfer policy. These results are theoretically consistent with the main predictions of job search model, and with recent empirical evidence on the effects of transport subsidies. A cash transfers increases unemployment, and the quality of the subsequent jobs. The mechanism I put forward, one among many possible, is that mothers are able to search longer and better, this increases their chances of finding a more stable formal jobs, and reduces those of "falling into informality". On the contrary, I do not find that the grant increases the long term employment rate, which suggests 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. The grant seems to have a direct impact on job search, as shown, for example, by higher transport expenditures to look for a job when unemployed.

The results of this paper also connect to the debate on the nature of informal employment. By showing that cash transfer recipients are less likely to hold an informal job after the transfer has stopped, these results indirectly support the "segmentation" hypothesis, at least within the context of the South African labor market, where informal work mostly takes the form of wage jobs and self-employment is rare. Those who are slightly richer because of the transfer are more likely to end up in the formal sector, which intuitively reflects a preference for these type of jobs. The unique feature of this study is the peculiarity of the transfer, its complete lack of conditionalities, and, because the means-test is not applied, conditions. 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 informative for the debate around transfers "no strings attached".

Lastly, an important question that remains unanswered is to what extent these results would carry through at the economy-wide level. Simple descriptive analysis shows that labor market outcomes for women have not improved significantly despite dramatic growth in CSG coverage in the early 2000s. Naively, this would suggest that reducing search frictions through a cash transfer is not sufficient, on its own, to increase formal sector employment in the aggregate. Intuitively, there are many possible explanations why that would be the case, such as a fixed stock of formal jobs. This would suggest combining policies that ease job search with some that increase the pool of formal jobs available, but further research is needed to draw clearer policy implications.

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A Appendix

A.1 Conceptual Framework

A.1.1 A Job Search Model

To present the possible effects of the CSG on the job search process, I use the simple framework developed in Card et al. (2007), which is already as simplification of Lentz and Tranaes (2005). This framework is particularly useful as it simplifies to the bone the standard job search model, and already models the effect of an exogenous cash grant. In the case of Card et al. (2007), this was intended for severance pay, but conceptually this is identical to an unconditional cash transfer.[46] The setup is that of a standard job search model, but assuming that wages are fixed, therefore eliminating reservation wage decisions. Card et al. (2007) justify this choice as their empirical results do not show any job quality gains to severance pay or more generous benefits. Instead, I do find that an exogenous cash grant leads to significant job quality gains for recipients. However, I also do not find that the grant leads to higher reservation wages, which is inconsistent with what a standard job search model would predict. Later, I outline alternative explanations to reconcile the persistent job quality effect that we observe in the data.

Card et al. (2007) write the value function of finding a job at the beginning of period t as:

$$V_t(A_t) = \max_{A_{t+1} \geq L} u(A_t - A_{t+1}/(1+r) + w_t) + \frac{1}{1+\delta} V_{t+1} A_{t+1} \tag{3}$$

where A_t are assets, r is the interest rate, w are wages and δ is the time discount factor. L is the level under which assets cannot fall, which "may or may not be binding." Instead, the value of being unemployed at the beginning of period t is the following:

$$U_t(A_t) = \max_{A_{t+1} \geq L} u(A_t - A_{t+1}/(1+r)) + \frac{1}{1+\delta} J_{t+1} A_{t+1} \tag{4}$$

Contrary to Card et al. (2007), I do not introduce unemployment benefits into the unemployment value function, as only a small fraction of workers actually benefits from this and they are not relevant for the purpose of our analysis. J_t is the "expected value of entering period t without a job":

$$J_t(A_t) = \max_{s_t} s_t V_t(A_t) + (1 - s_t) U_t(A_t) - \psi(s_t) \tag{5}$$

where s_t is the search effort, also normalized to be the probability of finding a job, $\psi(s_t)$ is the disuitility from search. As in any standard job search model, the optimal level of search effort is then given by the following condition:

$$\psi'(s_t^*) = V_t(A_t) - U_t(A_t) \tag{6}$$

[46] This is true for those who are unemployed, in that case severance pay is formally equal to a cash transfer. The difference is that a cash transfer, such as the *Child Support Grant*, is received also by people who are employed, inactive etc.

which implies that the optimal level of search is the one where the marginal cost of finding a job equals to the marginal benefit of getting one. This leads to two predictions that are useful for our framework. First, a cash grant should have the following effect on search effort:

$$\partial s_t^*/\partial A_t = \{u'(c_t^e) - u'(c_t^u)\}/\psi''(s_t^*) \leq 0 \tag{7}$$

This indicates that the a cash grant should have a negative impact on job search. As Card et al. (2007) suggest, this provides a "test of whether agents can smooth consumption perfectly." As this equation is a function of the distance between the marginal utilities in the employed and unemployed state, if agents perfectly smooth consumption, then the utilities will be identical (or close), and the effect of a cash transfer on search intensity will be zero (or small). Hence, observing a reaction in terms of job search, which in our data will be indicated by higher unemployment, provides a test for liquidity constraints. Therefore, a first prediction is that, if mothers do not perfectly smooth consumption between employment and unemployment, we should observe an increase in unemployment when receiving the transfer.

The second prediction as to do with the quality of the subsequent job. Card et al. (2007) do not formalize this, as wages are taken as exogenous, and simply say that in a general model with reservation wages, a cash grant could "potentially lead to a rise in the reservation wage and an increase in the average quality of the next job." In a *directed* search model à la Nekoei and Weber (2017) the job quality effect will depend on two countering forces. On the one hand, a cash grant will make people more selective, by increasing their target wage. Agents will target jobs that tend to pay higher wages. On the other hand, longer unemployment lowers the target wage. Therefore, the effect of a cash grant on job quality in this type of model is ambiguous.

A.1.2 An Occupational Choice Model

In order to understand the potential impacts of the CSG, it is also useful to think terms of a simple two-period model of occupational choice, which is standard in the literature on informality (Bianchi and Bobba (2013), Falco (2014)). With respect to a job search model, an advantage of this approach is that it also allows us to model entry into employment at the extensive margin. It is convenient to think of two sectors, formal and informal, from both a conceptual standpoint but also practical, as this categorization matches the data at my disposal. The individual searches for a job in period t, looking for either a formal or informal job (or set up an informal business). Finding both a formal or informal sector job requires covering some search cost, c. Finding an informal job requires a fixed investment, c_{min}, which can be thought of either as the capital investment necessary to start an informal business (as in Bianchi and Bobba (2013)), or as the necessary amount to spend on transport to find an informal job, for example as a domestic worker. Similarly, looking for a formal job implies a variable cost, c, which indicates again the amount spent on transport. All enjoy an exogenous source of income, $\bar{y}$, which is heterogeneous across individuals. Assuming no borrowing, period t is subject to the constraint $\bar{y} - c \geq 0$, i.e. job seekers cannot spend on job search more than they earn through non-employment sources.

In period $t+1$, an individual who will be employed in the formal sector with probability p_f and earn wage W_f. Alternatively, she will be employed in the informal sector with probability $1-p_f$, and earn wage W_I. This is meant to reflect the "free entry" nature of the informal sector: conditional on covering the minimum investment c_{min} in period t, informal sector employment is certain. This also implies that an informal job might be, for some, a "fall back" option when formal search fails. Both wages and p_f are heterogeneous across individuals. Someone who cannot search, because liquidity constraints are too binding $\bar{y} < c_{min}$, remains inactive in both periods.

The key step here is that the probability of having a formal job at time $t+1$ is a positive function of the amount spent in period t.[47] Therefore, p_f is a function of c, with $p_f = 0$ if $c \leq c_{min}$, p'_f greater than 0 between c_{min} and c_{max}, and $p'_f(c)$ equal to 0 when $c > c_{max}$. The implication of this are fairly intuitive: the probability of having a formal job in the next period increases with the amount of money spent when unemployed. Also, there is a critical point, which I denote as c_{max} at which the function is flat, i.e. increasing c will not increase (but also not decrease) the probability of finding a formal job. Lastly, I also set that $p_f = 0$ for $c \leq c_{min}$. This assumption simply states that finding a formal job requires an investment in period t that is always higher than the one necessary to find an informal job, which seems realistic. From this setup, and ignoring time-discounting, one can easily derive the following constrained optimization problem:

$$\begin{aligned} \underset{c}{\text{maximize}} \quad & 2\bar{y} - c + p_f(c)W_f + (1 - p_f(c))W_I \\ \text{subject to} \quad & \bar{y} - c \geq 0 \end{aligned} \tag{8}$$

Individuals in this simple framework are imagined to be risk-neutral and to maximize income rather than utility (as in the classic Harris and Todaro (1970) model). Ignoring the liquidity constraint in the first period, one can define c^* as the the value of c in the unconstrained optimization. Therefore, in the case without liquidity constraints during job search, this would be the value that satisfies the following condition:

$$\frac{\partial p_f}{\partial c} = \frac{1}{W_f - W_I} \tag{9}$$

This condition derives intuitively from the setup: the higher the wage in the formal sector an individual could earn *relative* to the wage in the informal sector for the same individual, the higher the optimal investment in period t. Intuitively, as the wage gap tends to zero, the optimal investment the first period will tend to c_{min}. On the contrary, as the formal sector premium increases, the optimal investment in period t will tend to c_{max}.

The model is solved by backward induction. It follows that a worker will choose *not* to look for a formal job, if the following inequality holds:

$$p_f(\tilde{c})(W_f - W_I) < \tilde{c} - c_{min} \tag{10}$$

[47] One could also posit that the formal wage in period $t+1$ is a positive function of the search cost in period t. As there is little information on wages in the data at hand, I make this simplification for now and take wages as exogenous.

where $\tilde{c}$ is the value that maximizes the constrained optimization problem in 8. Logically, this means that a worker will look for a formal job if and only if the expected wage gain is higher than the extra search cost in period t. In this framework, it also emerges clearly the difference between necessity vs. choice informality, and constrained and unconstrained formal job search, which I highlight in Figure A1.

Workers are heterogeneous across three dimensions: their initial endowment $\bar{y}$, their probability of finding a formal job, p_f, and wages in both sectors. A worker in this framework makes two, more or less constrained, decisions: whether to work or not, and whether to look for a formal job or not. The model is solved by backward induction. Workers choose whether they want to look for a formal or for an informal job, according to whether the expected benefit from a formal job exceeds the higher search cost, as stated in Inequality 10:

$$p_f(\tilde{c})(W_f - W_I) < \tilde{c} - c_{min}$$

The decision on whether to work or not is based on whether payoffs from working are higher than the cost. There are people for whom work is simply not profitable, and therefore would choose inactivity. This is population for which the following conditions hold simultaneously:

$$\begin{cases} W_I - c_{min} < 0 \\ p_f(\tilde{c})(W_f - W_I) + W_I - \tilde{c} < 0 \end{cases}$$

Similarly, there is also the sub-population for whom cash constraints are too binding, and cannot look for a job. These are people whose initial endowment is not sufficient even to cover entry cost into informal jobs, $\bar{y} < c_{min}$. Therefore, we can distinguish the population in four different groups: 1) those who cannot work; 2) those who could work, but do no want to; 3) those who look for an informal job only; 4) those with some level of formal sector search. Among these last two groups, we can further make the distinction into two sub-population each. Those who would look for an informal job no matter what (choice or comparative advantage informality), those who look for an informal job but would prefer a formal job under no constraint (necessity or segmentation informality). These two groups differ in terms of their Inequality 10 in the unconstrained case:

$$\begin{cases} p_f(\tilde{c})(W_f - W_I) < \tilde{c} - c_{min} \\ p_f(c^*)(W_f - W_I) \lesseqgtr c^* - c_{min} \end{cases}$$

where for one group any level of formal sector search is never profitable, while, in the unconstrained case, the other group would prefer formal employment to informal employment.

Instead, among those who do look for a formal job, i.e. the group for whom Inequality 10 does not hold and expected payoffs from formal search are positive, we can also distinguish among two subgroups. Those that look for a formal job without constraints, $\tilde{c} = c^*$ and those that look for a formal job with constraints, $\tilde{c} < c^*$. The difference being with respect

to the level of $\bar{y}$ and c^*, such that:

$$\bar{y} \lesseqgtr c^*$$

Figure A1 shows this simple framework in a stylized tree of occupational choice. Two groups of people are inactive: those that cannot work, and those for whom work is not profitable. There are two types of people who look only for an informal job: those that always prefer informal work, and those that only prefer it because they do not have enough resources to search properly for a formal job. There are also two types of people who look for a formal job: those that search with constraints and those that search without constraints.

With this in mind, the potential effects of the grant are straightforward. Conceptually, the grant is identical to an increase in unearned income, $\bar{y}$. The first implication is that the grant might increase employment by allowing people who are below $\bar{y} < c_{min}$ to look for a job. The relevance of this channel will depend on the size of the marginal population that is between $\bar{y} < c_{min}$ and $\bar{y} + CSG > c_{min}$. Also, it will depend on the preferences of this subpopulation, i.e. whether work is profitable for them or not. Therefore, a first prediction of the model is that the grant might have a positive effect on employment at the extensive margin. Whether this new employment will be formal or informal is ambiguous, and will depend again on the preferences of this marginal population and the constraints in their job search. Another potentially positive effect on employment might come from people for whom the expected benefits from a constrained formal job search without CSG are negative, but become positive in the presence of the grant, because the search is more likely to pay off.

Prediction 1. *The CSG grant might increase employment at the extensive margin in t+1*

A second effect of the grant may be to move people from informality by necessity to formal job search. Specifically, the relevance of this channel will depend on the marginal population for whom:

$$\begin{cases} p_f(\tilde{c})(W_f - W_I) < \tilde{c} - c_{min} \\ p_f(\tilde{c}_{csg})(W_f - W_I) > \tilde{c}_{csg} - c_{min} \end{cases}$$

These are people who would not have otherwise looked for a formal job, but for whom formal job search becomes profitable with the grant. This second prediction implies a shift from informal to formal, i.e. this predicts an increase in the number of people looking for a formal job.

Prediction 2. *The CSG grant might push people in necessity informality to look for a formal job*

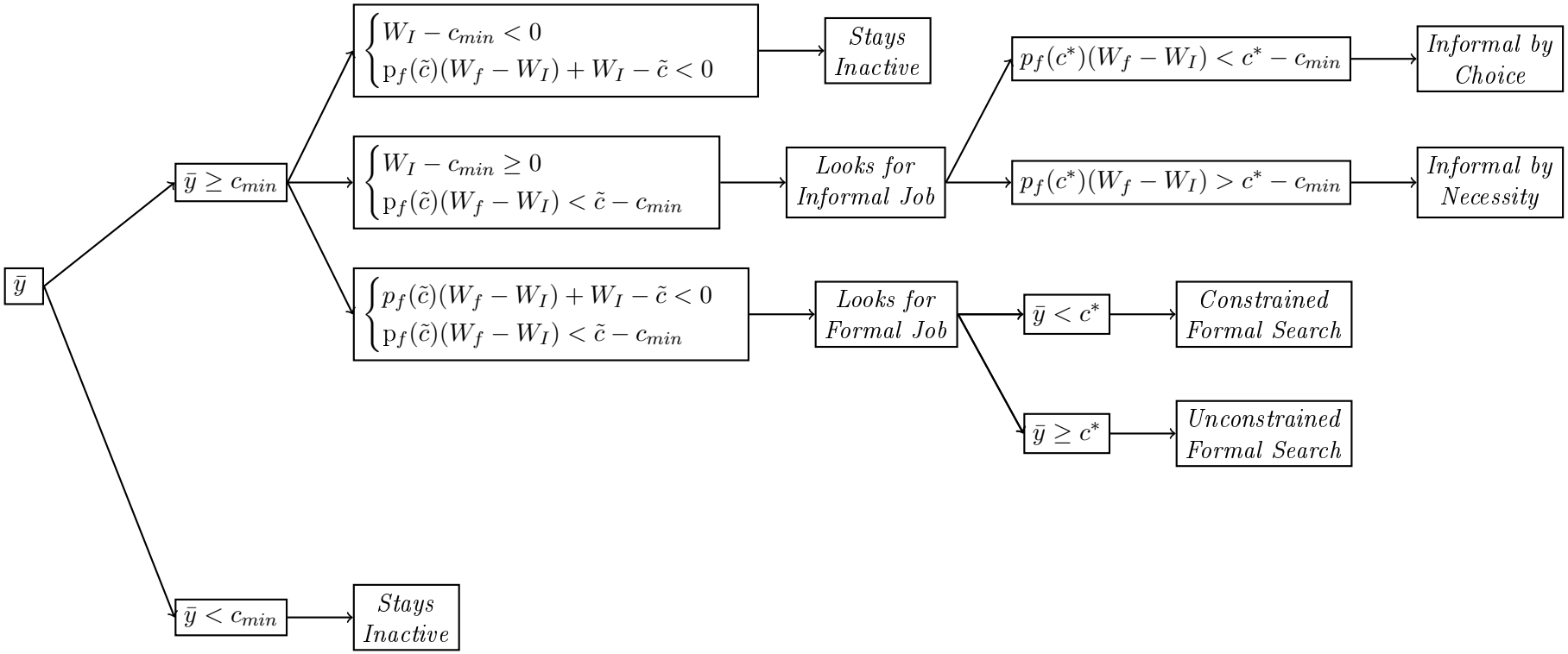
A third, predicted effect of the grant is that it may allow people who would have searched anyway for a formal job to search better, meaning less constrained. This does not predict an increase in the number of people looking for a formal job, but an increase in the formal search directly (either through longer duration, more effective search etc.)

Prediction 3. *The CSG grant might push people in constrained formal job search to look longer/more effectively for a formal job*

To sum up, setting up the framework like this, with costs to entry into work that are asymmetric between the formal and informal sector, leads to a few predictions about the labor market effects of an unconditional cash transfer. First, an unambiguously positive effect on employment at t+1. Second, an unambiguously positive effect on formal sector search for those who would be employed even without the grant. However, the overall effect on informality is ambiguous, as new entrants into the labor market might go towards informal jobs, the net effect is ambiguous.

For the purpose of this simple framework, I have assumed that individuals are risk neutral, which is a direct implication of the fact that they maximize income rather than utility. However, the role of risk aversion in the allocation across wage and self-employment (and formal vs. informal) is key, as shown empirically by both Bianchi and Bobba (2013), Falco (2014). In the context of an African labor market, Falco (2014) finds that risk-adverse individuals are more likely to queue for a formal job. Similarly, Bianchi and Bobba (2013) shows that in Mexico a cash transfers increases the willingness to bear risk, and hence entry into self-employment. In the framework presented above, the more "risky" strategy is one with some level of formal job search, while informal employment is certain. Therefore, in this framework, a risk-mitigating effect of the CSG would again increase formal employment. To reintroduce in the model this notion of the informal sector as the more "risky" sector, one should also model explicitly self-employment (and introduce utility).

Figure A1: Stylized Tree of Occupational Choice



A.2 Measuring Informality

The measurement of informality often poses some challenges. This is not necessarily a problem of data. Informality is not a sharply defined concept, but rather a blurry status with different shades of intensity (famously defined as the "murky" sector by Fields (1975)). There is no consensual definition of what exactly defines an informal job. The first theoretical distinction is between informal employment and the informal sector. A worker in informal employment is one for whom labor market legislation does not apply, while a worker in the informal sector is one employed by a firm operating informally, i.e. which does not follow labor market legislation. This distinction does not apply to the self-employed, for whom the two definitions coincide. For employees, informal employment and informal sector clearly overlap, but not perfectly. In theory, an informal firm cannot have a formal employee, but a worker can be informally employed in a formal firm, for example if a registered firm does not pay social security contributions for this employee. The trend in the literature has been to measure informal employment by affiliation to social security and the informal sector by whether the business is registered or not.

Census data has information on whether the sector of employment is formal or informal (based on whether the firm is registered), or whether the employer is a private household. There are some inconsistencies in the coding of this information in 2011 Census data.[48] The Labour Force Surveys have information on both the informal sector, similarly to the census, and informal employment, namely contract status (the presence of a written contract), social security affiliation for employees; firm size, and VAT tax registration for both employees and self-employed workers. This allows me test the correlation and the overlap between the informal sector and informal employment, which I present in Table A1.[49]

Throughout this paper, the main focus will be on employment in the informal sector, as this is the definition that is consistently defined across Census waves. It should be underlined that this is a lower bound of overall informal employment, and that the formal sector is also comprised of a portion of workers who are not covered by labor market legislation. Instead, when running robustness estimations on the NIDS panel data, the main outcome will be informal employment, as this is the only definition available. The exact definitions of informal sector/employment used throughout the paper are the following:

[48] Workers are asked separately their industry of employment and their sector with three options: 1) Formal sector, 2) Informal Sector and 3) Private Households. This leads to a share of workers reporting to work in a private household, but whose reported industry of employment is not a private household. For these individuals, who account for around 10% of total employment, an informality status is not defined and reported as missing. In my main estimations, I will impute their informality status based on the their industry and occupation.

[49] Overall, the theoretical distinction presented before seems to hold in the data. Individuals employed in the informal sector (non-registered firms) report not being affiliated to social security and not having a written contract in around 80% of the cases. Given that this is all self-reported data on the side of the worker, we can imagine that the remaining gap is due to measurement error, as respondents might not have clear information about the registration of the business or whether the employer is paying social security contributions. Consistently, informal employment is significantly larger than employment in the informal sector (i.e. we can think of informal sector employment as a subset of informal employment). With respect to the self-employed, we also observe that own-account workers are almost entirely in the informal sector, which is, however, also composed by some employers (i.e. self-employed individuals whose business employs other people).

1. *Informal Sector Employment* (Census/LFS)= Employment in Non-Agricultural Unregistered Businesses + Employment in Private Households + Employment in Agriculture[50]

2. *Informal Employment* (NIDS/LFS) = Employees without a Written Contract + Self-Employed without VAT registration

Table A1: Overlap between Informal Sector and Informal Employment

Employees		Informal Employment	
	Informal Sector	Social Security	Written Contract
Social Security	78.89	1	64.52
Written Contract	77.88	74.99	1
Informal Sector	1	50.49	42.77
Self-employed		Informal Employment	
	Informal Sector	VAT Tax	Own Account
VAT Tax	98.21	1	92.95
Own Account	67.06	66.72	1
Informal Sector	1	95.49	90.75

To be read as: 78.89% of employees in the informal sector are not affiliated to social security. *Note:* Informal sector refers to individuals employed in non-registered businesses. Social security refers to individuals whose employer does not pay any social security contributions (pension, medical or unemployment insurance). Written contract refers to the presence of a written agreement between the employer and the employee. VAT tax refers to the business being registered for Value-Added Tax. Own account workers are self-employed workers with no employees.
Source: Author's calculations on LFS (2002-2007)

[50]For simplicity, when constructing a measure of informal sector employment, I include employment in agriculture in the informal sector. The advantage of this approach is that it allows me to divide total employment in just two sectors, formal and informal. This simplification does not change the results in any way.

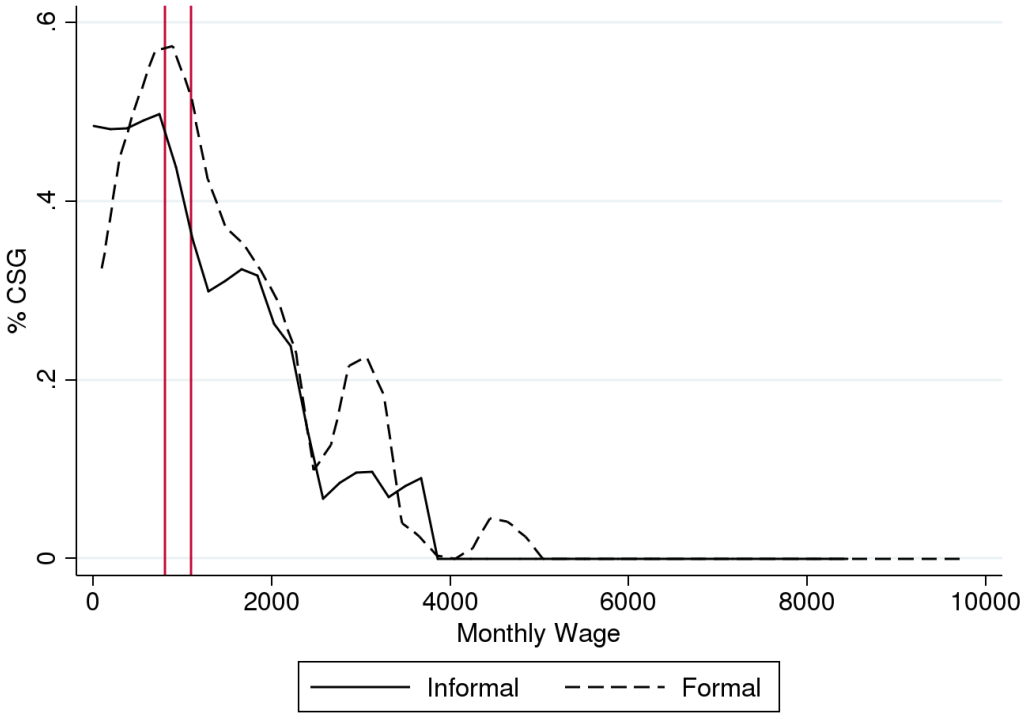
A.2.1 Means-Test

It is worth discussing more in detail to what extent the presence of a means-test could drive these results. While, as mentioned before, there is little evidence that the means-test is applied in practice, or binding in any way, it is still possible that it affects the behavior of workers. Indeed, we could think that the decrease in formal employment in 2007 is due to workers hesitating to take formal jobs for fear of losing the grant.[51] This could also explain why we do not observe a similar drop in informal employment while the grant is received.

While consistent with what we observe, this channel seems unlikely. As shown in Table 5, median wages in the formal sector are more than 10 times larger than the grant. Moreover, virtually no formal job pays less than one CSG grant, and only 8% less than the means-test (R 800). This makes it less intuitive to think that mothers would be willing to forego significantly higher earnings to keep the grant. This would be more plausible if the decrease in formal employment in 2007 would have been matched by an increase informal employment, meaning that mothers would have reallocated the same job from formal to informal simply to escape the means-test. However, we observe no increase in informal employment, suggesting that the adjustment is at the extensive margin of employment, rather than in the composition of jobs. There is also some direct evidence that "means-test" mechanism is not at play. The first wave of the NIDS (early 2008) allows to look at take-up of the CSG by wages for both formal and informal jobs, before the means-test was changed. In Figure A2 we observe that the drop is smooth, and that take-up decreases gradually and not sharply at the threshold. More importantly, this decrease is similar for both formal and informal jobs. Again, this evidence points against a strict application of the means-test, or that mothers adjust their formal employment at the extensive margin to pass the means-test.

[51] We know this is not the case, because the means-test is only checked, through an affidavit, at the application stage, but the worker might not.

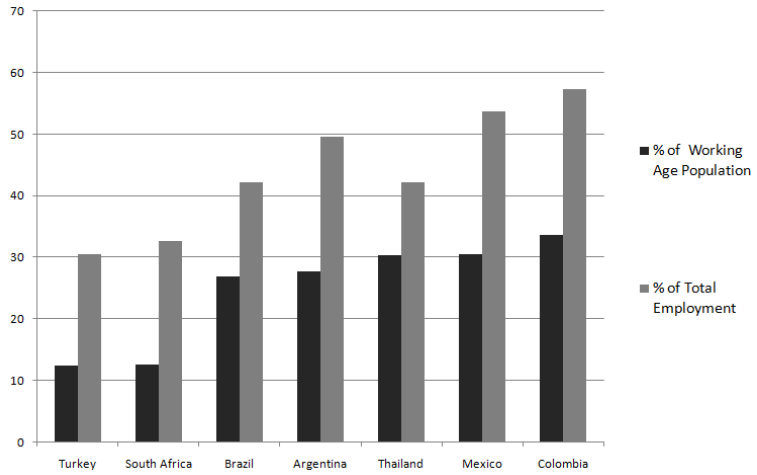
Figure A2: Take-Up by Wage in the Formal and Informal Sectors, Mothers, NIDS 2008



Note:This graph plots take-up of the CSG for both formal and informal jobs in early 2008, before the reform of the means-test. The first red line indicates the means-test in urban areas (800), the second line in rural areas (1100)
Source: NIDS (Wave 1 - Interviews Before July)

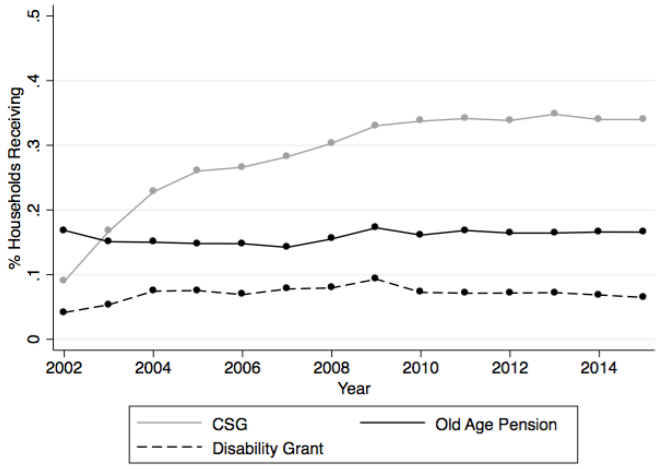
A.3 Descriptive Statistics

Figure A3: Informality Levels in Selected Countries, 2010



Note: This graph plots average informality levels in 2010 (or 2009, when not available) for countries with similar levels of GDP per capita in 2010. The darker bar provides the share of informal employment over working age population. The grey bar plots the share of informality over total non-agricultural employment.
Source: World Bank Database (`http://databank.worldbank.org/data/home.aspx`)

Figure A4: Share of Households Receiving Social Grants in South Africa, 2002-2015



Note: This graph draws the evolution of the three main social grants in South Africa over the period 2002 to 2015. The CSG experienced a dramatic increase in its coverage relative to the other grants, due to the reforms in age eligibility. Coverage refers to the percentage of households with at least one member receiving the grant.
Source: Author's calculations on GHS

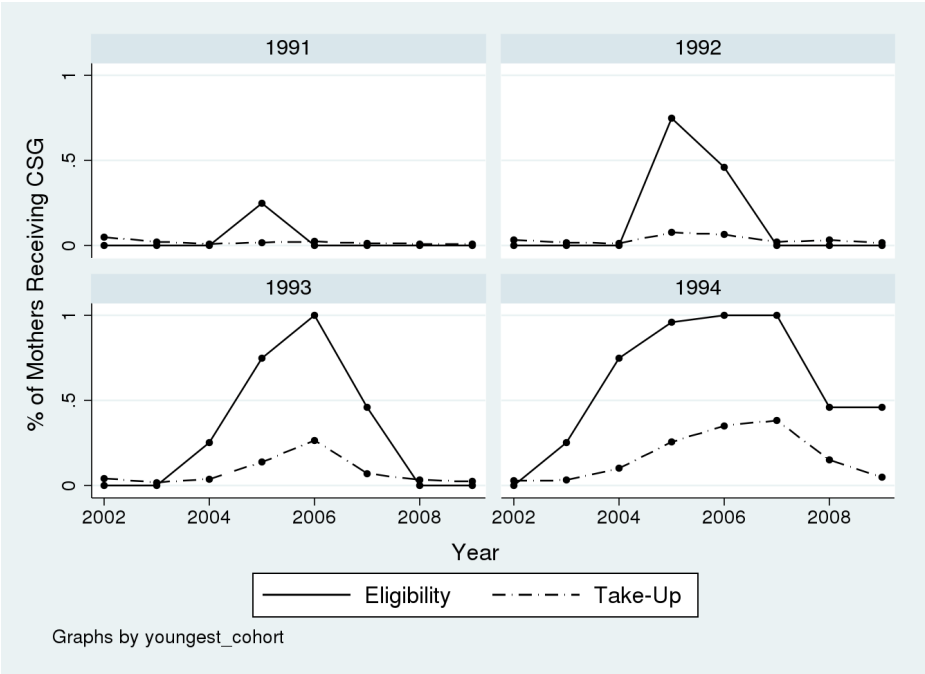
Table A2: Characteristics by Labor Market Status, 2002-2007

Characteristics (pop %)	Informal	Formal	Inactive	Unemployed
Socio-Demographics				
African (76.71%)	89.00	59.96	82.79	88.27
White (10.85%)	3.44	21.90	6.99	2.38
Women (51.79%)	56.29	37.41	63.75	53.39
Young (<30) (40.74%)	26.94	28.03	50.74	56.84
No Schooling (27.30%)	44.12	17.55	33.21	22.32
Job Characteristics				
Agriculture (4.48%)	12.47	8.15	-	-
Self (8.79%)	48.92	6.55	-	-
Public job (6.93%)	1.18	19.58	-	-
Union (11.96%)	1.88	33.88	-	-
Part Time (8.21%)	21.67	3.13	-	-
Temporary Contract (6.61%)	-	20.04	-	-

To be read as: 89.00 per cent of informal workers are African, while 59.96 per cent of formal workers are African. *Note:* Informal sector refers to individuals employed in non-registered businesses. *Young* is a binary variable indicating individuals less than 30 years old. *No schooling* indicates individual with no educational attainment. *Self* indicates self-employed workers. The sample is restricted to the working age population aged 18 to 60. *Public Job* refers to an individual being employed by the national or local government, or by a government agency. *Union* refers to the worker belonging to a worker union. *Part Time* workers are those working less than 30 hours per week on average.
Source: Author's calculations on LFS (2002-2007)

A.4 Empirical Analysis

Figure A5: CSG Eligibility and Take-Up by Cohort of Birth, 2002-2011



Note: This graph gives the average yearly eligibility and the average take-up of mothers whose youngest child was born in 1991, 1992, 1993 or 1994 for the period 2002-2011, in the month of July, when the GHS takes place. Take-up before 2002 was virtually zero for these cohorts. Eligibility is calculated using the information in Table 1 and is averaged over the full year.
Source: Author's calculations on GHS

A.4.1 Bias a in Diff-in-Diff Estimator with Cohorts and Age Effects

A simple difference-in-differences estimator compares the realized outcomes of mothers whose youngest child was in a treated with those whose youngest child was in an untreated cohort. If we take the smallest possibles window, cohort 1992, the last unexposed cohort, and cohort 1993, the first expost cohort, over a 10-year period, the DiD estimator equals:

$$\begin{aligned} DiD = &[\mathbb{E}(y_i|c_i = 1993,\ year = 2011) - \mathbb{E}(y_i|c_i = 1993,\ year = 2001)] \\ &-[\mathbb{E}(y_i|c_i = 1992,\ year = 2011) - \mathbb{E}(y_i|c_i = 1992,\ year = 2001)] \end{aligned}$$

For unbiasedness, this estimator relies on the "common trend" identification assumption, which states that, in the absence of the treatment, the evolution of treated and non-treated cohorts would have been the same. Formally, using y_0 to express the potential outcome in the absence of the treatment, the condition needed for correct identification is the following:

$$\begin{aligned} &[\mathbb{E}(y_{0,i}|c_i = 1993,\ year = 2011) - \mathbb{E}(y_{0,i}|c_i = 1993,\ year = 2001)] \\ &= [\mathbb{E}(y_{0,i}|c_i = 1992,\ year = 2011) - \mathbb{E}(y_{0,i}|c_i = 1992,\ year = 2001)] \end{aligned}$$

which means that the time evolution of labor market oucomes for mothers of treated and untreated cohorts would have been the same without the CSG. One can replace cohort

with $year - cohort$, in order to obtain the discrete age value in a given year, we obtain that the identification assumption equally implies:

$$\begin{aligned}&[\mathbb{E}(y_{0,i}|age_i = 18,\ year = 2011) - \mathbb{E}(y_{0,i}|age_i = 8,\ year = 2001)]\\ =&[\mathbb{E}(y_{0,i}|age_i = 19,\ year = 2011) - \mathbb{E}(y_{0,i}|age_i = 9,\ year = 2001)]\end{aligned}$$

We can write the total difference between the age value in 2011 and the one in 2001 as the sum of the differences between each subsequent age value, such that the previous expression becomes:

$$\begin{aligned}&\sum_{j=0}^{10}[\mathbb{E}(y_{0,i}|age_i = 8+j+1,\ year = 2001+j+1) - \mathbb{E}(y_{0,i}|age_i = 8+j,\ year = 2001+j)]\\ =&\sum_{j=0}^{10}[\mathbb{E}(y_{0,i}|age_i = 9+j+1,\ year = 2001+j+1) - \mathbb{E}(y_{0,i}|age_i = 9+j,\ year = 2001+j)]\end{aligned}$$

We can then split this difference into the age evolution for those age values that we observe for both cohorts over the 10-year period, and those age values that we do not observe for both cohorts, therefore:

$$\begin{aligned}&\sum_{j=1}^{10}[\mathbb{E}(y_{0,i}|age_i = 8+j+1,\ year = 2001+j+1) - \mathbb{E}(y_{0,i}|age_i = 8+j,\ year = 2001+j)]\\ &\qquad+[\mathbb{E}(y_{0,i}|age_i = 9,\ year = 2002) - \mathbb{E}(y_{0,i}|age_i = 8,\ year = 2001)]\\ =&\sum_{j=0}^{9}[\mathbb{E}(y_{0,i}|age_i = 9+j+1,\ year = 2001+j+1) - \mathbb{E}(y_{0,i}|age_i = 9+j,\ year = 2001+j)]\\ &\qquad+[\mathbb{E}(y_{0,i}|age_i = 19,\ year = 2011) - \mathbb{E}(y_{0,i}|age_i = 18,\ year = 2010)]\end{aligned}$$

Therefore, the previous equation shows that the difference-in-differences estimator will give an unbiased estimate of the effect of the CSG only if: 1) age effects are constant over time, and do not change from year to year; 2) the age effect for those age values that we do not observe for both cohorts are equal. The age values that we do not observe for both cohorts are the oldest one for the older cohort, and the youngest one for the younger cohort. The estimator will be unbiased if there are no age effects of the child's on the mother's labor market outcomes, if age effects are linear, if these age effects somehow cancel out. Otherwise, even assuming that age effects are constant over time, and therefore equal for each cohort, and setting β as the true effect of the CSG, the DiD estimators will suffer from the following bias:

$$DiD = \beta + [\Delta_{18-19} - \Delta_{8-9}]$$

where Δ_{18-19} denotes the age effect from 18 to 19 years, and Δ_{8-9} denotes the age effect from 8 to 9 years.

Figure A6: CSG Effect on Employment Rate (Residuals)

(a) 2001 (b) 2007 (c) 2011

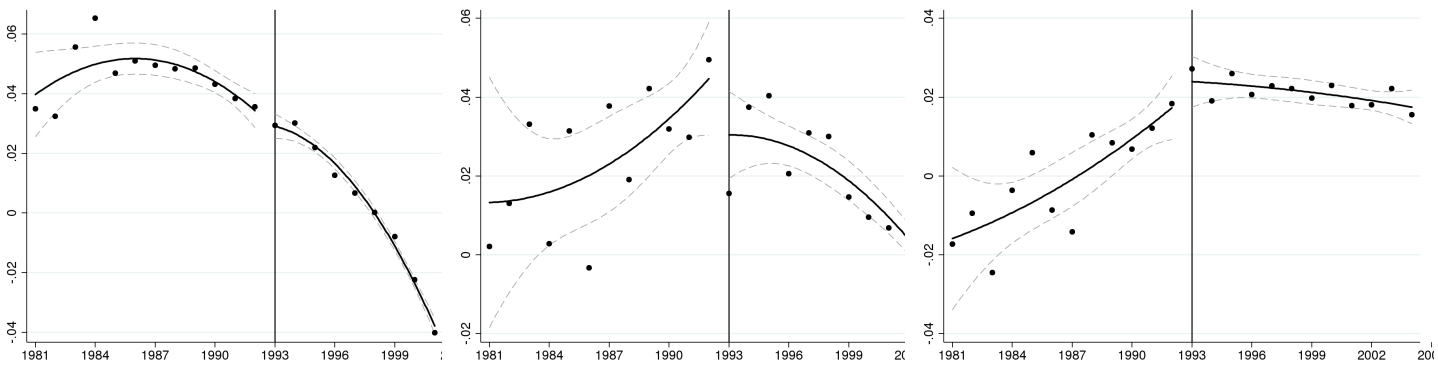


Figure A7: CSG Effect on Unemployment Rate (Residuals)

(a) 2001 (b) 2007 (c) 2011

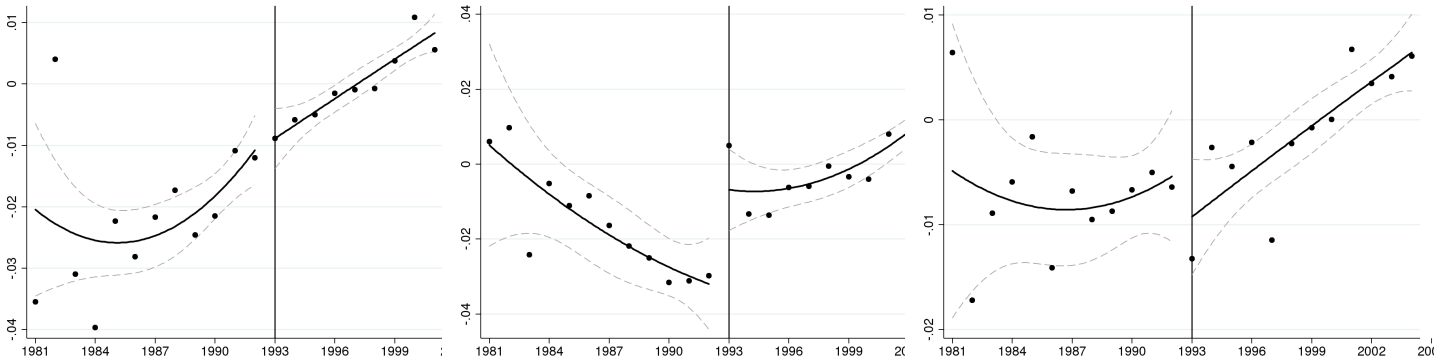


Figure A8: CSG Effect on Informal Sector Employment Rate (Residuals)

(a) 2001 (b) 2007 (c) 2011

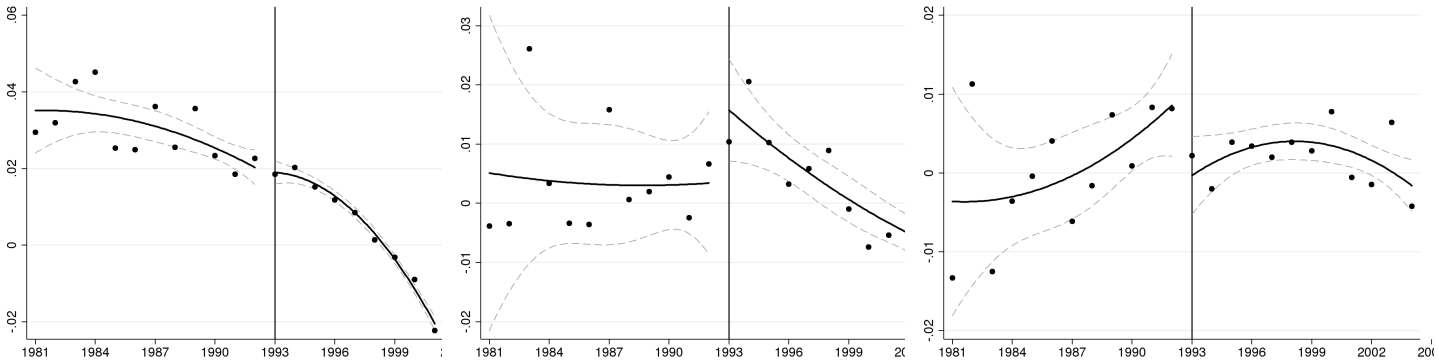
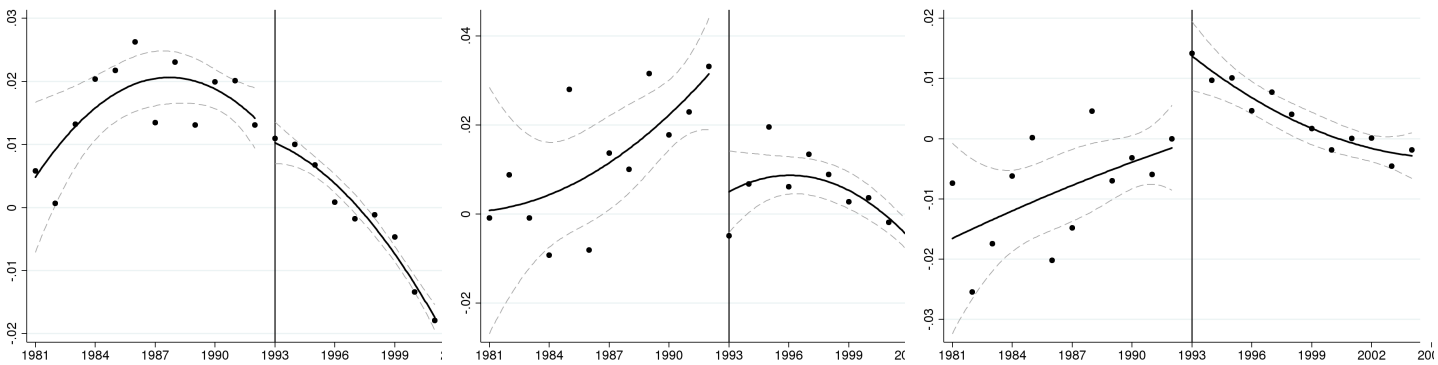


Figure A9: CSG Effect on Formal Sector Employment Rate (Residuals)

(a) 2001 (b) 2007 (c) 2011



Note: These graphs report the estimations of Table 3 in graphical form. The residuals are plotted after controlling for the same variables as listed in Table 3. A quadratic function is fitted on both sides of the threshold for the three separate years. The window, 1981 to 2004, is chosen according to an Akaike Information Criterion (AIC) that maximizes the window for a quadratic function.

Table A3: Labor Market Effects of the CSG on Mothers, Within and Across Jobs, 2011

	(1) Informal	(2) Informal in Private Household	(3) Informal outside Private Household	(4) **Within**-Occupation Variation	(5) **Across**-Occupation Variation
CSG	-0.0254***	-0.0147*	-0.0107	-0.0112	0.0005
	(0.0097)	(0.0089)	(0.0072)	(0.0068)	(0.0024)
Observations	112,967	112,967	112,967	112,967	112,967
R-squared	0.1521	0.1317	0.0575	0.0410	0.0470

Note: *** p<0.01, ** p<0.05, * p<0.1. I decompose the decrease in informality for employed mothers in 2011 in the following way: The total effect is in Column (1). The probability of working for a private household is in Column (2). The probability of working informally outside a private household is in Column (3). Then, I estimate the following equation: $Informal_i = occupation_i + \epsilon_i$, and then separately predict the fitted values and the residuals of this equation. I then estimate Equation 1 having as a dependent variable the residuals (Column (4)) and the fitted values (Column (5)). The coefficients add up in the following way: (1) = (2) + (3) and (3)=(4) +(5). All estimations include controls for: age of the mother, education, race, marital status, province and household size. Standard errors are clustered at the household level.
Source: Author's calculations on Census 2011

Table A4: Effects on the Employment Composition of Mothers, 2007 & 2011

	Occupational Status		**Sector×Occupational Status**			
	Wage-Employed (1)	Self-Employed (2)	Informal Self (3)	Informal Wage (4)	Formal Self (5)	Formal Wage (6)
2007 - "During"						
CSG	-0.0206	0.0021	0.0053	0.0072	-0.0032	-0.0278**
	(0.0133)	(0.0064)	(0.0057)	(0.0100)	(0.0029)	(0.0117)
Mean Y at Threshold	0.4844	0.0523	0.0417	0.1683	0.0105	0.3161
Observations	90,084	90,084	90,084	90,084	90,084	90,084
R-squared	0.1509	0.0172	0.0190	0.0593	0.0083	0.1971
2011 - "After"						
CSG	0.0074	-0.0046	-0.0058**	-0.0056	0.0012	0.0129**
	(0.0071)	(0.0038)	(0.0025)	(0.0053)	(0.0028)	(0.0061)
Mean Y at Threshold	0.4204	0.0652	0.0299	0.1518	0.0352	0.2685
Observations	247,032	247,032	247,032	247,032	247,032	247,032
R-squared	0.1347	0.0083	0.0060	0.0339	0.0112	0.1666

Note: *** p<0.01, ** p<0.05, * p<0.1. This table gives the OLS estimates of Equation 1 on mothers' occupational status (Column (1) and (2)), also decomposed by sector (Column (3) to (6)). Self-employed are individuals who run their own business. The coefficients add-up in the following way: (2)=(3)+(5), (1)=(4)+(6) and (1)+(2)=(3)+(4)+(5)+(6). In the upper panel, the estimation is in 2007, when the CSG is still received by cohort 1993. In the lower panel, the estimation is in 2011, 5 years after the grant has stopped. Standard errors are clustered at the household level.
Source: Author's calculations on Community Survey (2007) and Census (2011)

Table A5: Labor Market Effects of the CSG on Other Adult Household Members (15 to 60 years old), 2001, 2007 & 2011

	Year 2001 - "Before"				
	(1)	(2)	(3)	(4)	(5)
	Active	Unemployed	Employed	Informal	Formal
CSG	-0.0001	-0.0041	0.0039	0.0027	0.0021
	(0.0046)	(0.0042)	(0.0038)	(0.0028)	(0.0039)
Mean Y at Threshold	0.5508	0.2413	0.3094	0.0807	0.2287
Observations	741,882	741,882	741,882	741,882	741,882
R-squared	0.3930	0.0877	0.3450	0.1191	0.2920
	Year 2007 - "During"				
	(1)	(2)	(3)	(4)	(5)
	Active	Unemployed	Employed	Informal	Formal
CSG	0.0061	0.0036	0.0025	0.0046	-0.0021
	(0.0106)	(0.0106)	(0.0101)	(0.0072)	(0.0092)
Mean Y at Threshold	0.5508	0.2145	0.3363	0.0964	0.2398
Observations	130,146	130,146	130,146	130,146	130,146
R-squared	0.3845	0.0951	0.3593	0.0955	0.2986
	Year 2011 - "After"				
	(1)	(2)	(3)	(4)	(5)
	Active	Unemployed	Employed	Informal	Formal
CSG	0.0003	-0.0080	0.0083	0.0010	0.0073
	(0.0075)	(0.0067)	(0.0063)	(0.0039)	(0.0058)
Mean Y at Threshold	0.4852	0.1891	0.2960	0.0702	0.2257
Observations	296,970	296,970	296,970	296,970	296,970
R-squared	0.2956	0.0507	0.3182	0.0727	0.2518

Note: *** p<0.01, ** p<0.05, * p<0.1. This table gives the OLS estimates of Equation 1 on other adult (15 to 60) household members' labor market outcomes in 2001, 2007 and 2011 respectively (Black and Coloured population only). The forcing variable is the cohort of birth of the youngest child ever born to a given mother in the same household. CSG is a binary variable for the child being born in or after 1993, which indicates being part of a cohort that had access to the CSG. Mean Y at Threshold gives the mean of the outcome for the cohort 1992 (last unexposed cohort). All estimations include controls for: age (cubic), education, race, marital status, municipality and household size. Standard errors are clustered at the household level.
Source: Author's calculations on Census 2001 & 2011, and Community Survey (2007)

Table A6: Difference-in-Differences Estimates of the Effect of the CSG on Mothers

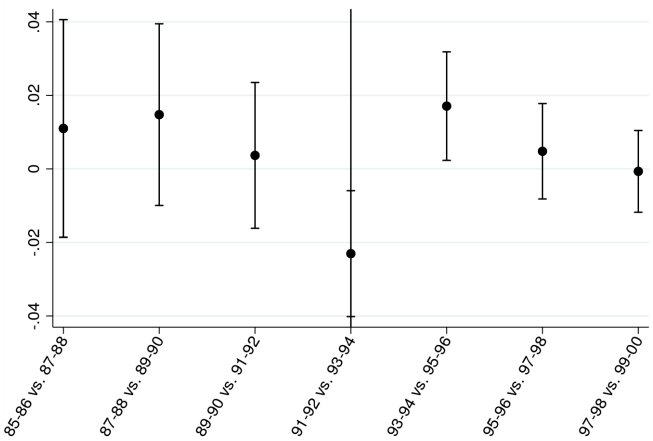
	(1) Active	(2) Unemployed	(3) Employed	(4) Informal	(5) Formal
Window: 1992 vs. 1993					
CSG×2007	0.0022	0.0322**	-0.0300**	0.0080	-0.0379***
	(0.0121)	(0.0127)	(0.0142)	(0.0116)	(0.0124)
CSG×2011	0.0032	-0.0091	0.0123	-0.0022	0.0145*
	(0.0082)	(0.0074)	(0.0086)	(0.0068)	(0.0075)
Observations	59,614	59,614	59,614	59,614	59,614
R-squared	0.1203	0.0960	0.1480	0.0670	0.1982
Window: 1993 vs. 1994					
CSG×2007	0.0012	-0.0185	0.0196	0.0075	0.0122
	(0.0114)	(0.0122)	(0.0133)	(0.0109)	(0.0114)
CSG×2011	0.0005	0.0075	-0.0070	-0.0037	-0.0033
	(0.0077)	(0.0070)	(0.0080)	(0.0063)	(0.0070)
Observations	70,566	70,566	70,566	70,566	70,566
R-squared	0.1197	0.0941	0.1498	0.0655	0.2007
Window: 1991-1992 vs. 1993-1994					
CSG×2007	0.0130	0.0213**	-0.0082	0.0130	-0.0212**
	(0.0088)	(0.0090)	(0.0102)	(0.0084)	(0.0089)
CSG×2011	0.0081	-0.0077	0.0158**	-0.0022	0.0180***
	(0.0059)	(0.0054)	(0.0062)	(0.0049)	(0.0054)
Observations	118,669	118,669	118,669	118,669	118,669
R-squared	0.1182	0.0914	0.1434	0.0622	0.1972

Note: *** p<0.01, ** p<0.05, * p<0.1. This table gives the estimates of Equation 2 on mothers' labor market outcomes. CSG is a binary variable for the child being born in or after 1993, upper panel, 1994, middle panel, 1993 or 1994, lower panel. All estimations include controls for: age (cubic), education, race, marital status, municipality and household size. Standard errors are clustered at the household level.
Source: Author's calculations on Census 2001 & 2011, and Community Survey (2007)

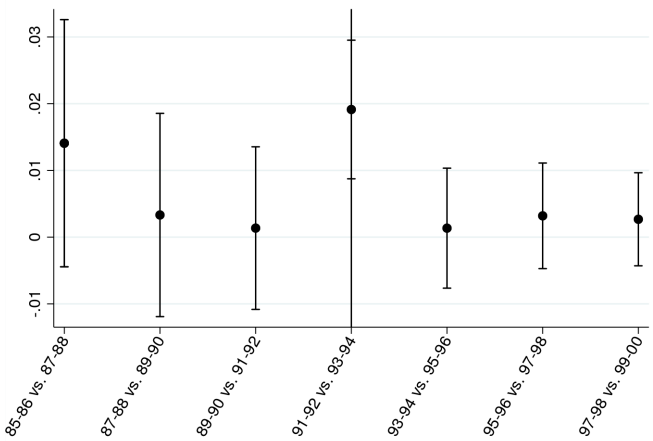
Figure A10: Difference-in-Differences Estimates with Varying Windows

Formal Employment

(a) 2007

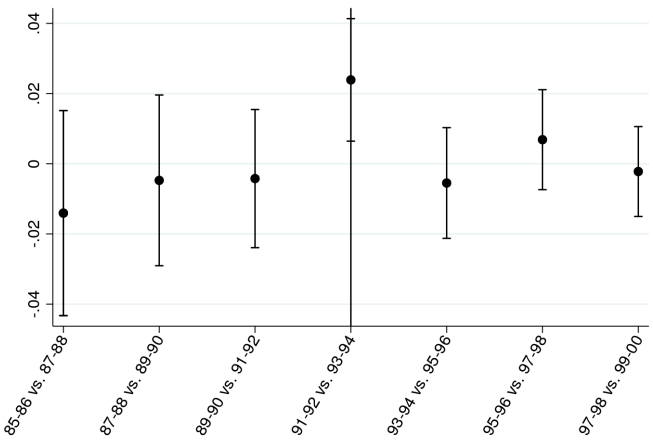


(b) 2011

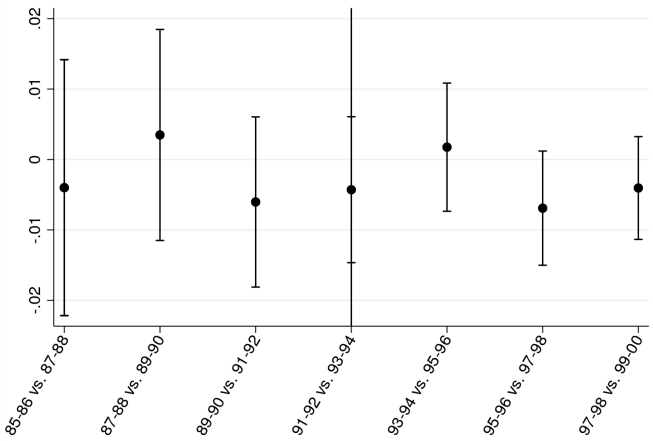


Unemployment

(a) 2007



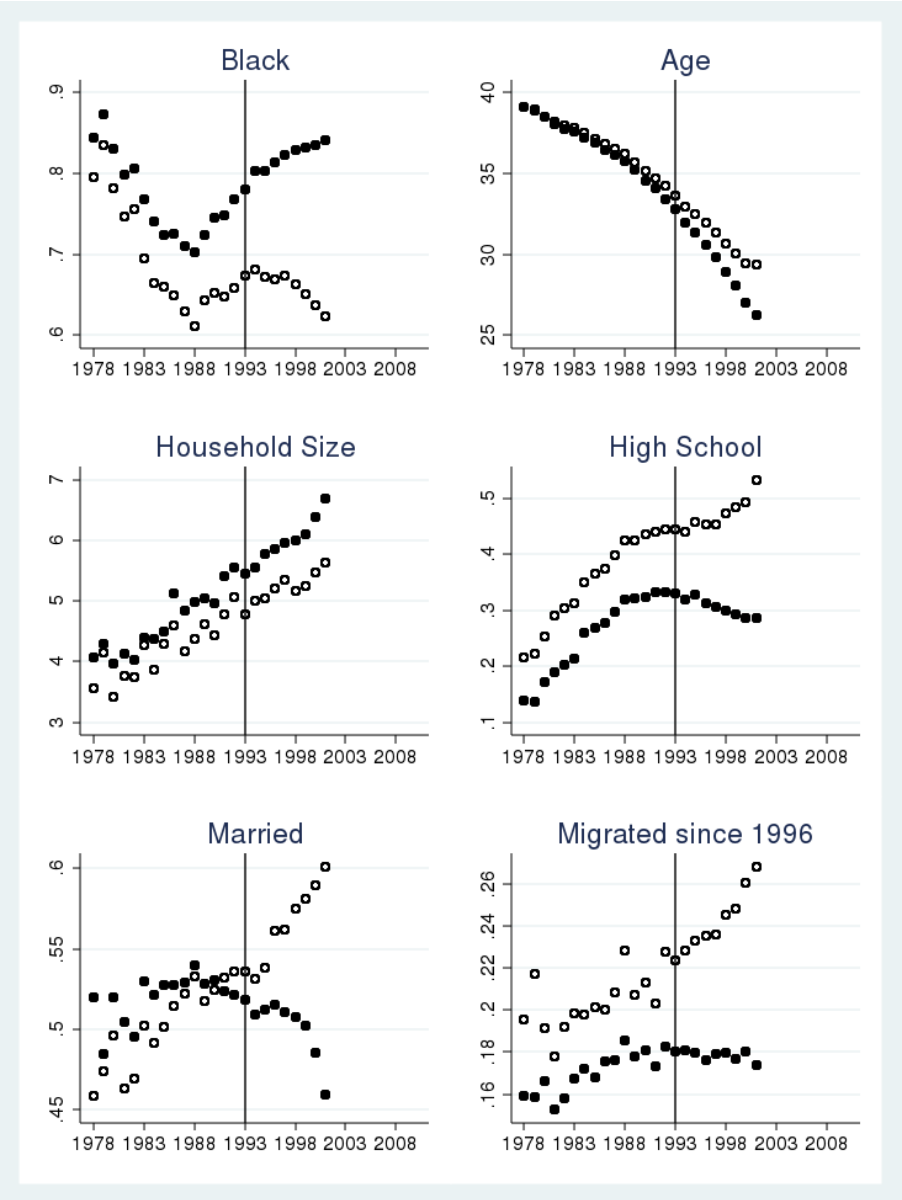
(b) 2011



Note: This graph gives the coefficients of Equation 2, the difference-in-differences estimation, with varying cohort windows, on both formal sector employment and unemployment, for both 2007 and 2011. The first two cohorts indicate the control group, while the last two the treated group (for example, 85-86 vs. 87-88, means that in the regression those mothers whose children are born in '85, 86' the control group, and '88,'89 are the treatment group. The dots represent the point estimate, while the vertical bars are 95% confidence intervals.
Source: Author's calculations on Census 2001 & 2011, and Community Survey (2007)

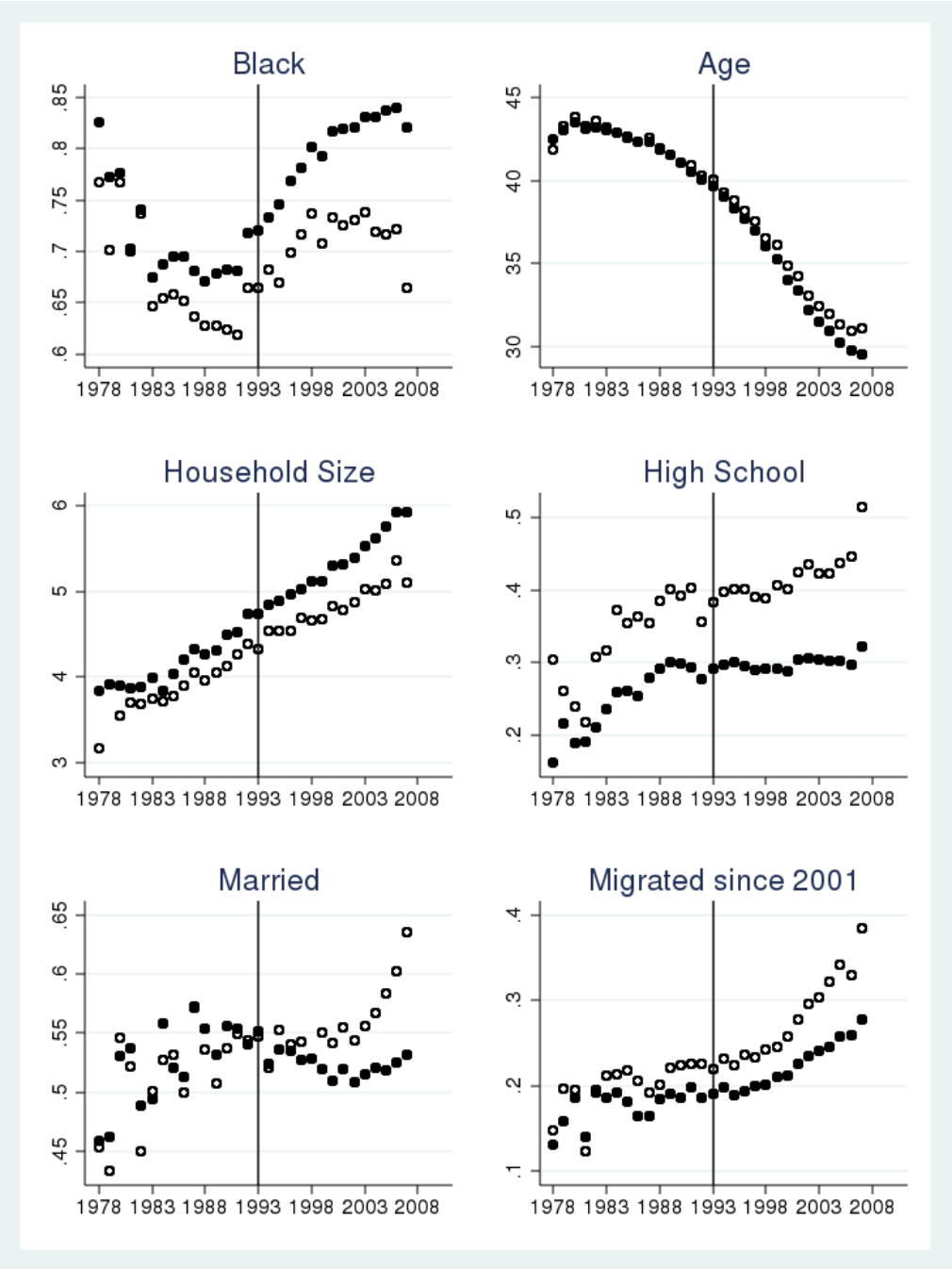
A.5 Robustness Checks

Figure A11: Observable Characteristics (All Mothers and Employed only), 2001



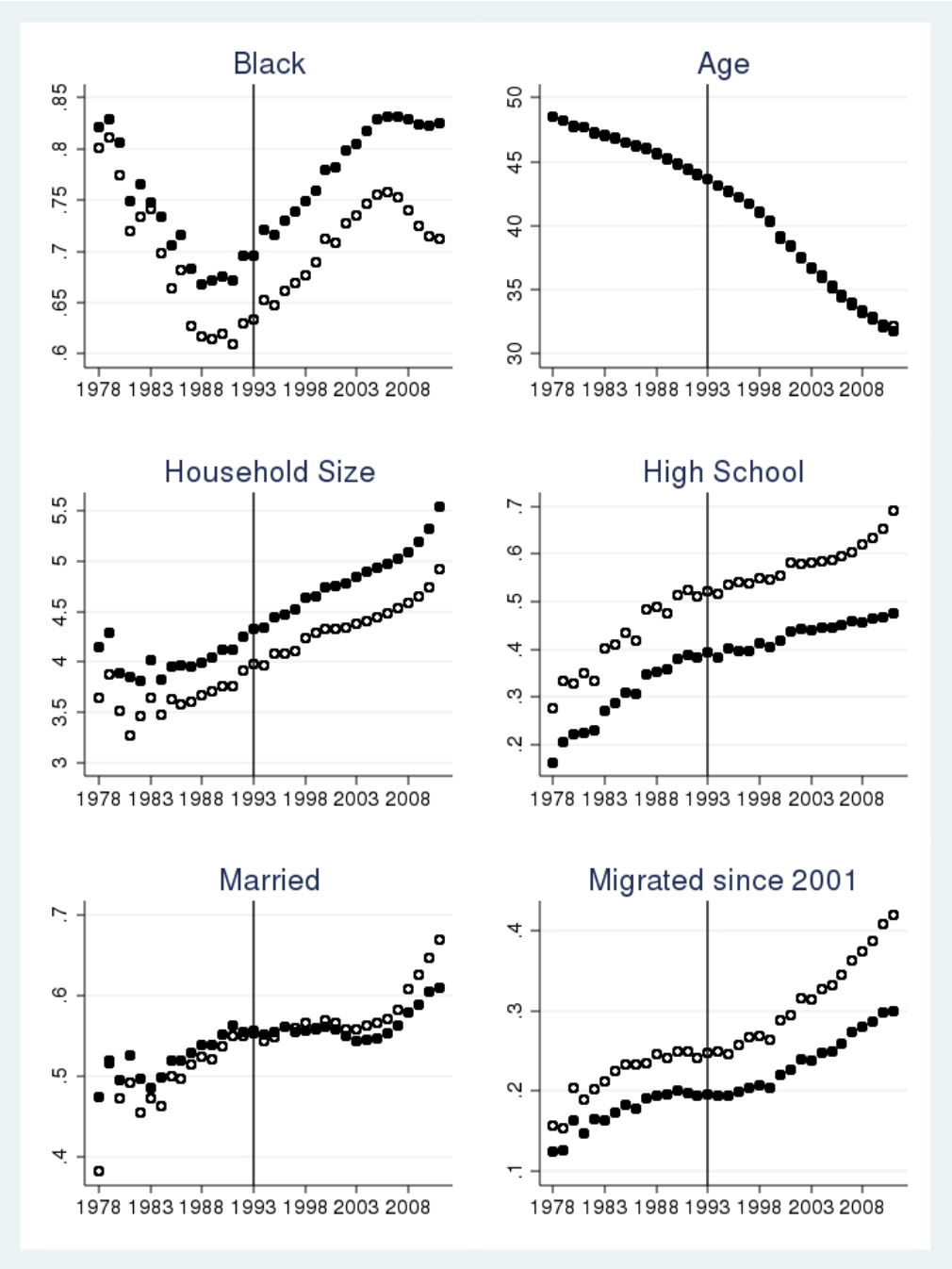
Note: These graphs show the distribution of observable characteristics by birth cohort of the youngest child, for all mothers (black dots) and employed mothers only (hollow dots). *Black* refers to the share of mothers who are neither Coloured, nor Indian, nor White. *Age* is the age of the mother, not of the child. *Household size* refers to the number of individuals in the household. *High School* refers to the share who has obtained an high school diploma. *Married* plots the share of mothers either married or living like married. *Migrated* gives the share of mothers who have moved at least once since 1996.
Source: Census (2001)

Figure A12: Observable Characteristics (All Mothers and Employed only), 2007



Note: These graphs show the distribution of observable characteristics by birth cohort of the youngest child, for all mothers (black dots) and employed mothers only (hollow dots). *Black* refers to the share of mothers who are neither Coloured, nor Indian, nor White. *Age* is the age of the mother, not of the child. *Household size* refers to the number of individuals in the household. *High School* refers to the share who has obtained an high school diploma. *Married* plots the share of mothers either married or living like married. *Migrated* gives the share of mothers who have moved at least once since 2001.
Source: Community Survey (2007)

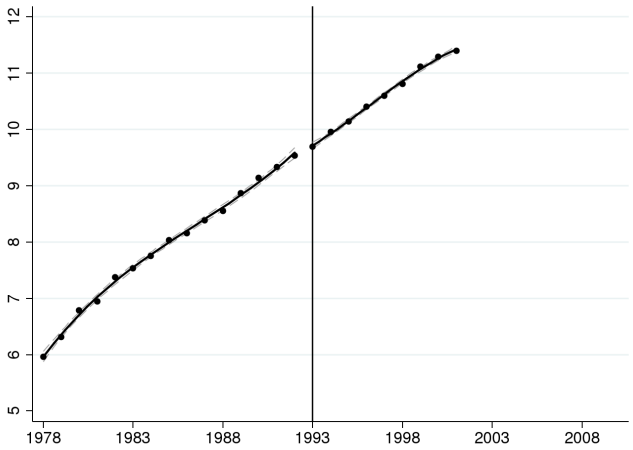
Figure A13: Observable Characteristics (All Mothers and Employed only), 2011



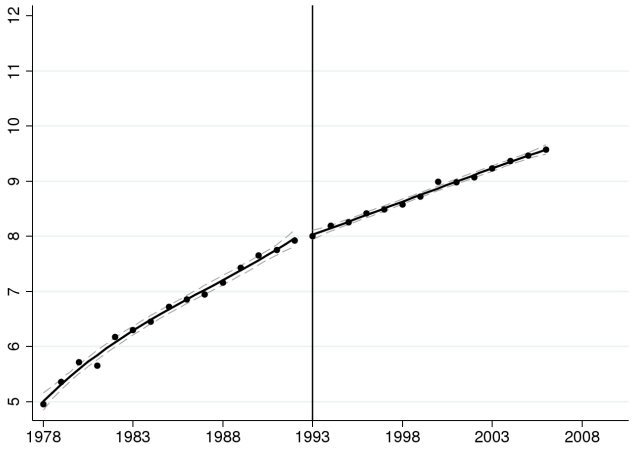
Note: These graphs show the distribution of observable characteristics by birth cohort of the youngest child, for all mothers (black dots) and employed mothers only (hollow dots). *Black* refers to the share of mothers who are neither Coloured, nor Indian, nor White. *Age* is the age of the mother, not of the child. *Household size* refers to the number of individuals in the household. *High School* refers to the share who has obtained an high school diploma. *Married* plots the share of mothers either married or living like married. *Migrated* gives the share of mothers who have moved at least once since 2001.
Source: Census (2011)

Figure A14: Density, 2001, 2007 & 2011

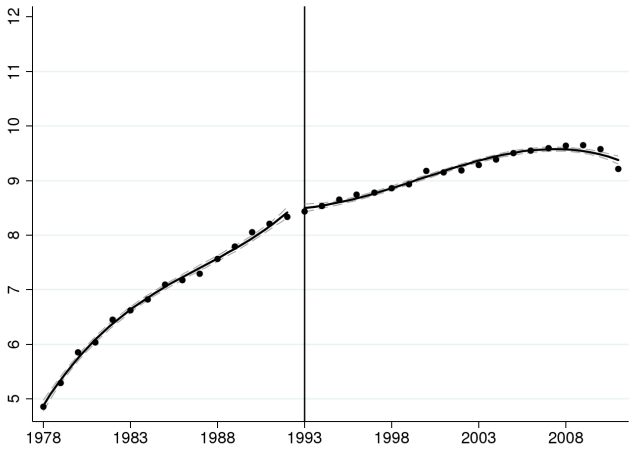
(a) 2001



(b) 2007



(c) 2011

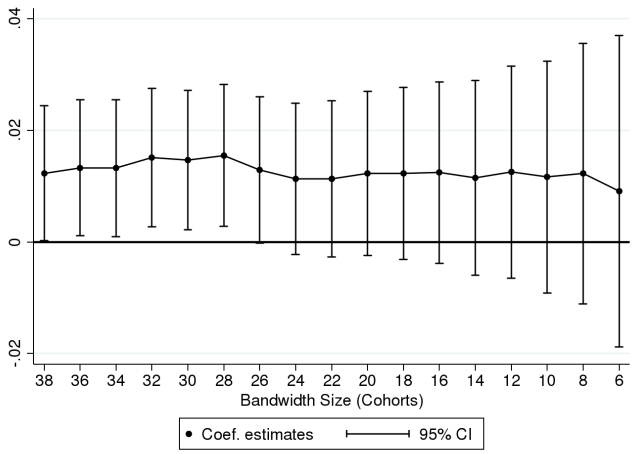


Note: These graphs give the log-density of mothers by year of birth of their youngest child. In order to compare the same population over time, the sample is limited to mothers born between 1960 and 1985, due to the questionnaire design of the census, where only women under 50 are asked for fertility information.
Source: Census (2001 & 2011) and Community Survey (2007)

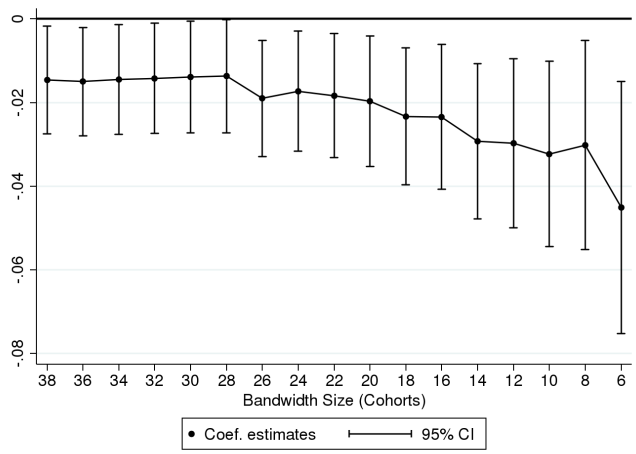
Figure A15: Bandwidth Sensitivity, 2007

Linear

(a) Informal

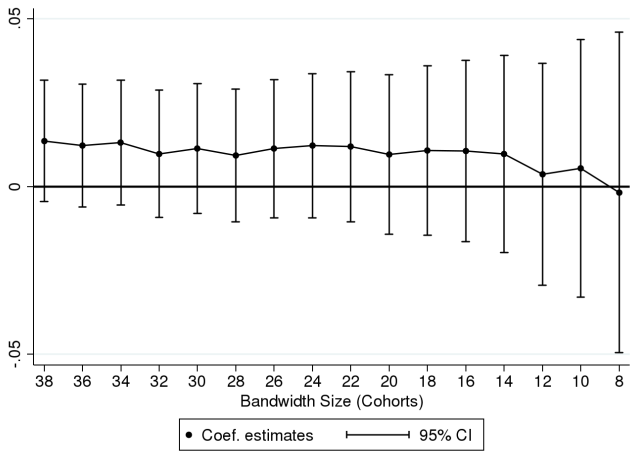


(b) Formal

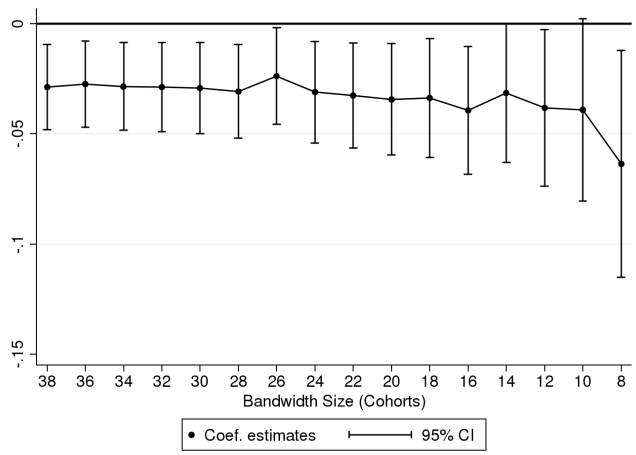


Quadratic

(a) Informal



(b) Formal

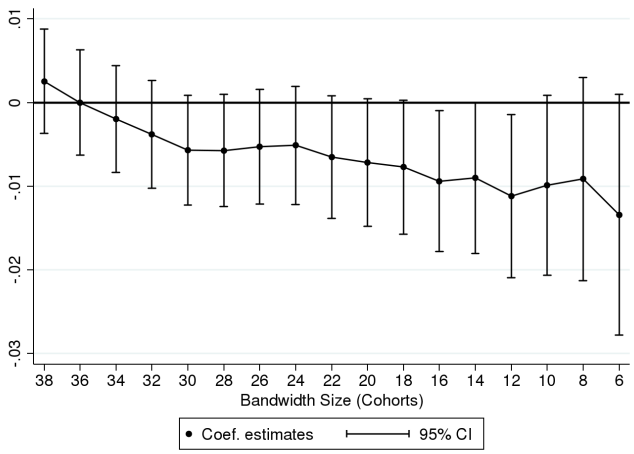


Note: Panel (a) tests the sensitivity of the estimates of Equation 1 on informal employment to the size of the bandwidth with a linear fit (upper panel) and a quadratic fit (lower panel). Panel (b) performs the same exercise for formal employment. The boundaries around the coefficients are 95% confidence intervals.
Source: Author's calculations on Community Survey (2007)

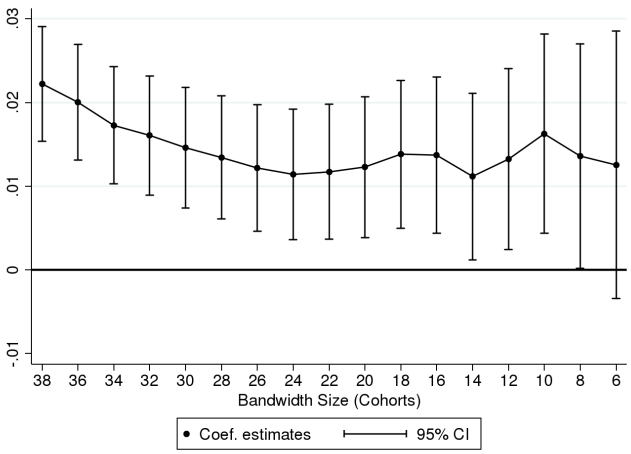
Figure A16: Bandwidth Sensitivity, 2011

Linear

(a) Informal

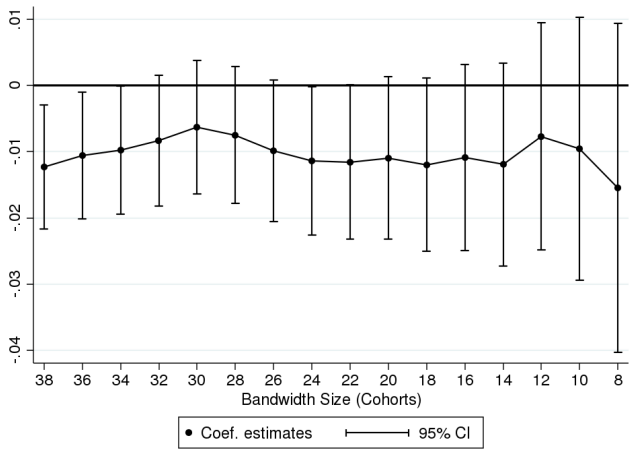


(b) Formal

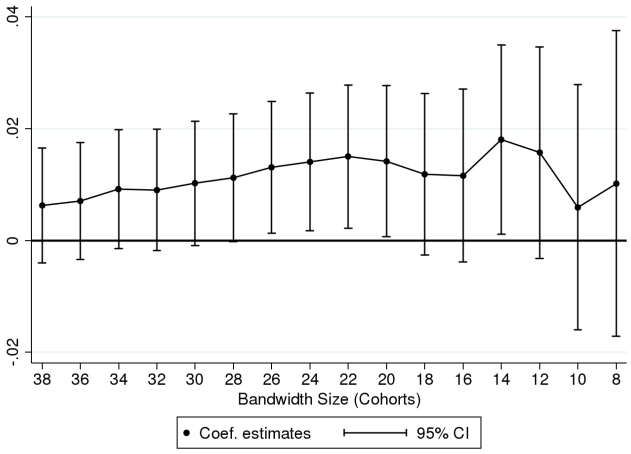


Quadratic

(a) Informal



(b) Formal



Note: Panel (a) tests the sensitivity of the estimates of Equation 1 on informal employment to the size of the bandwidth with a linear fit (upper panel) and a quadratic fit (lower panel). Panel (b) performs the same exercise for formal employment. The boundaries around the coefficients are 95% confidence intervals.
Source: Author's calculations on Census (2011)

Figure A17: Placebo Test - White Mothers Only, 2007 & 2011

2007

Figure A18: Informal

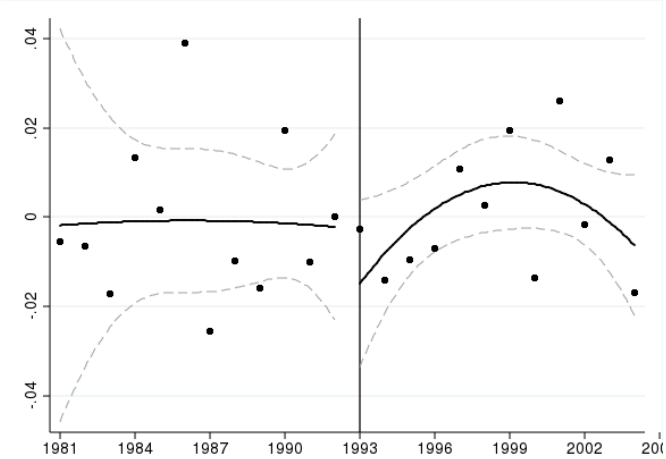
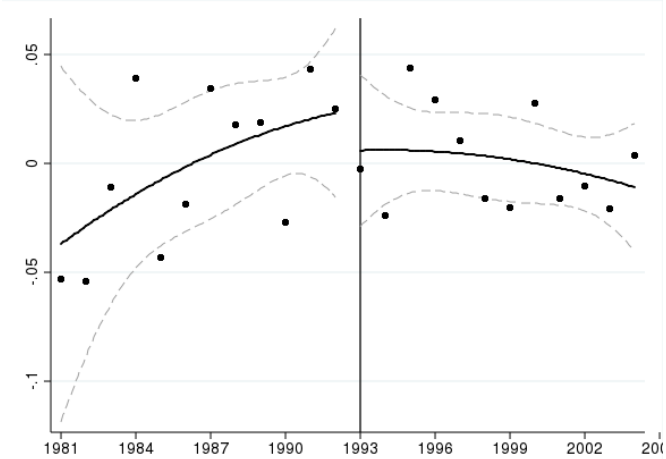


Figure A19: Formal



2011

Figure A20: Informal

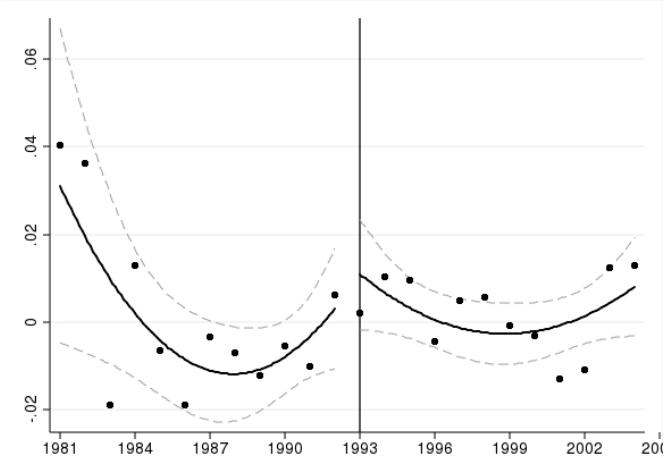
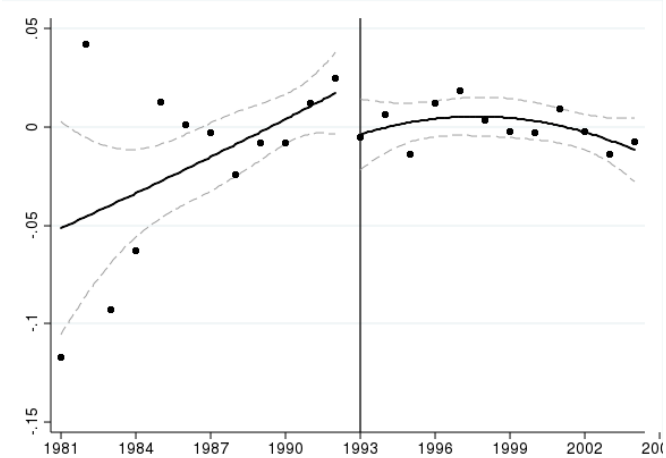


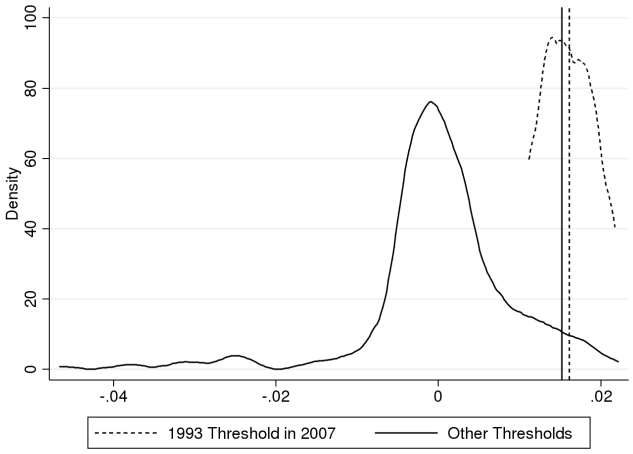
Figure A21: Formal



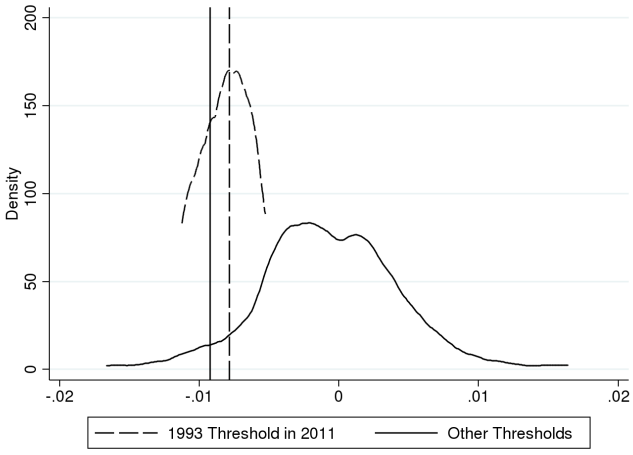
Note: These graphs give the estimates of Equation 1 on informal and formal employment for White mothers only in 2007 (upper panel) and 2011 (lower panel).

Figure A22: Placebo Threshold

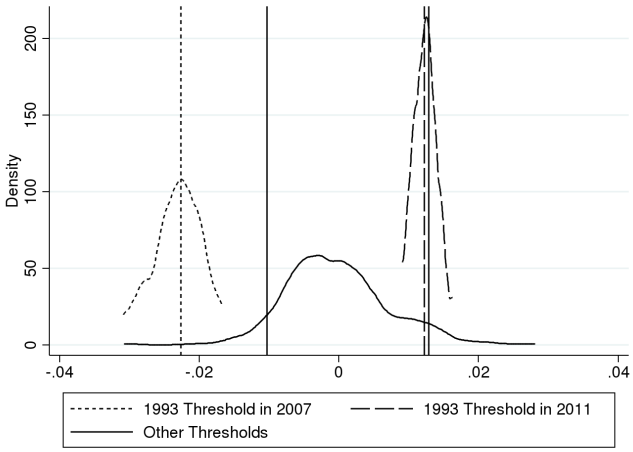
(a) Unemployment



(b) Informal Employment



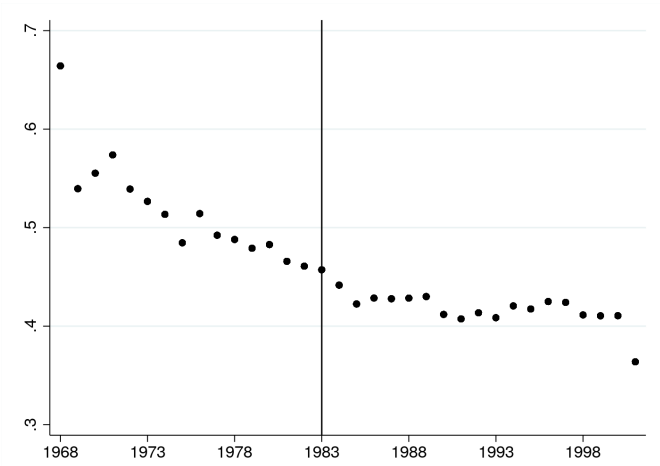
(c) Formal Employment



Note: These graphs give the distribution of the CSG coefficient of Equation 1, when varying both the bandwidth and the threshold location, on unemployment (panel (a), informal employment (panel (b)), and formal employment (panel (c)). In all specifications, the function is chosen based on an AIK criterion test. The dashed line gives the distribution of coefficients for the discontinuity when varying the bandwidth (from ± 14 to ± 5) and setting the correct threshold at cohort 1993 in 2007 and 2011. The solid line gives the distribution of coefficients when setting placebo thresholds, and varying the bandwidth (from ± 14 to ± 5). The solid vertical lines give the 5th and 95th percentiles of the distributions.
Source: Census (2001 & 2011) and Community Survey (2007)

Figure A23: Share of Informal Employment by Birth Cohort of Youngest Child (Cohorts of the Same Age), 2001 & 2011

(a) Year 2001 - Cohort 1983

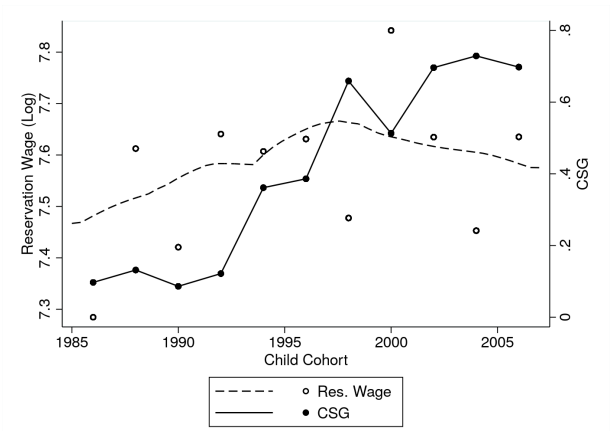


(b) Year 2011 - Cohort 1993

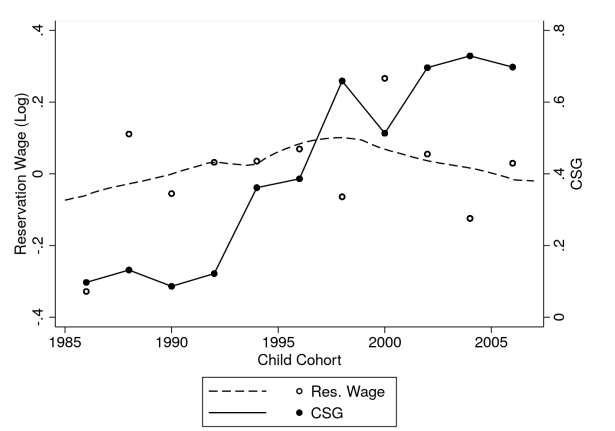
Note: These graphs give the probability of being employed in the informal sector, conditional on being employed, for mothers by cohort of birth of the youngest child ever born, in 2001 and 2011 respectively. In the left panel, the threshold is set at cohort 1983, who is exactly the same age as cohort 1993 in 2011. Both censuses take place in the month of October.
Source: Census (2001 & 2011)

Figure A24: Reported Reservation Wages and CSG Take-Up by Cohort of Birth of Youngest Child, NIDS 2010-2014, Unemployed Mothers only

a) Unconditional



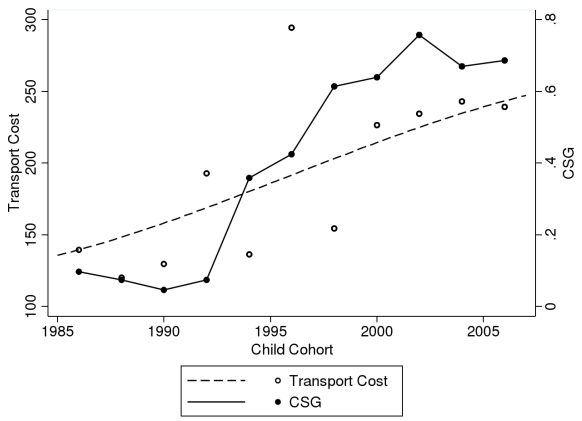
b) Conditional



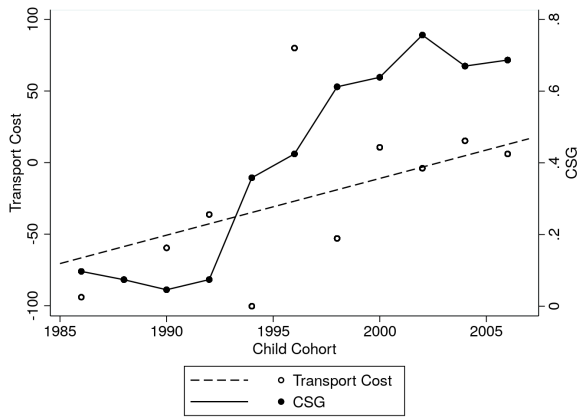
Note: These graphs plot the correlation between reported reservation wages (in logs) for the unemployed, and CSG take-up by cohort of birth of the youngest child. The left panel plots the unconditional reservation wages, while the right panel plots the residuals controlling for a set of observable characteristics. The sample is limited to unemployed mothers.
Source: NIDS (2010-2014)

Figure A25: Monthly Transport Cost to Look for a Job for Unemployed and Source of Money

a) Unconditional



b) Conditional



Note: These graphs plot the correlation between reported transport cost when looking for a job for the unemployed, and CSG take-up by cohort of birth of the youngest child. The left panel plots the unconditional value, while the right panel plots the residuals controlling for a set of observable characteristics. The sample is limited to unemployed mothers.
Source: NIDS (2008-2014)