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# Cash transfers, employment and informality in South Africa

Alessandro Tondini

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UNIVERSITÉ PARIS 1 PANTHÉON SORBONNE  
PARIS SCHOOL OF ECONOMICS

## PhD Thesis

for the degree of Doctor of Philosophy in Economics

*Prepared and defended at the Paris School of Economics on June 25<sup>th</sup>, 2019 by:*

ALESSANDRO TONDINI

# Cash Transfers, Employment and Informality in South Africa

under the supervision of Luc BEHAGHEL (INRA, PSE) and Jérôme GAUTIE (Paris 1)

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UNIVERSITÉ PARIS 1 PANTHÉON SORBONNE  
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## THÈSE

Pour l'obtention du grade de Docteur en Sciences Économiques

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ALESSANDRO TONDINI

# Transferts Monétaires et Emploi dans le Secteur Informel en Afrique du Sud.

Sous la direction de Luc BEHAGHEL (INRA, PSE) et Jérôme GAUTIÉ (Paris 1)

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# Introduction

## General Introduction

This dissertation gathers evidence on employment responses to cash transfers in South Africa with the aim to draw general lessons on the functioning of labor markets in middle-income countries. At this stage of development, labor markets are particularly intricate, as they combine multiple sectors where formal and informal jobs coexist. This segmentation requires to think about why and how workers place themselves across different sectors. Similarly, policy design and evaluation need to take into account the presence of an unregulated informal sector, and how this might distort incentives when interacting with social programs. The relevance of this concern, and more generally which policies can lead to welfare-improvements, strongly depend on the nature of this “shadow” sector of the labor market. By analyzing the effects of South Africa’s unique cash transfer programs, which offer practical policy experiments to test the role of constraints and incentives in the allocation of workers, I argue that a few important conclusions can be drawn.

First, it is important to explain why this dissertation focuses on South Africa, and to what extent its findings are South Africa-specific. South Africa’s labor market is unlike that of a “standard” middle-income country. Contrary to what is usually observed in countries at a similar level of development, it is characterized by a persistently high unemployment rate, a symmetrically low employment rate, and a relatively small informal sector. These well-known issues have been extensively documented since the end of the Apartheid period, yet it is difficult to find in the literature explanations or hypotheses for these anomalies. With a few exceptions

([Kingdon and Knight \(2004\)](#), [Banerjee et al. \(2008\)](#)), most of the economic literature on South Africa has tried to draw lessons of more general interest, rather than addressing the issues that are specific or unique to its labor market. The country's innovative social programs provide interesting policy experiments, with features that are difficult to find in other middle income countries. Paradoxically, while this has led to a large research output *in* South Africa, there is much less *about* South Africa. This dissertation is not entirely exempt from this shortcoming. Most of what is presented in the following chapters are attempts to use South Africa's uncommon cash transfer system to draw lessons of more general interest, and policy implications that are not necessarily country-specific. However, focusing entirely and extensively on one country has allowed me to elaborate an explanation for some of the distinct dysfunctionalities of the South African labor market, and how they interact with issues usually present in other middle-income countries. I mostly elaborate this reasoning in the third chapter, albeit in a more descriptive way, but building onto some of the reduced-form results of the first two chapters.

Overall, the dissertation attempts to contribute to two strands of the economic literature. First, it expands on the large literature on cash transfers, which have received growing attention given their pervasiveness in developing countries ([Molina Millán et al. \(forthcoming\)](#), [Haushofer and Shapiro \(2016\)](#)). However, while many of these programs have been extensively studied along different aspects, we still know very little about their labor market effects on adult recipients. Results from several randomized interventions suggest that disincentive effects on work are likely to be small ([Banerjee et al. \(2017\)](#)). Moreover, there are theoretical reasons to believe that cash transfers might actually have positive effects on employment outcomes of adults ([Baird, McKenzie and Özler \(2018\)](#)). From a policy perspective, it is key to know whether the shock induced by a cash transfer can have lasting benefits on the income-generating activities of recipients, especially their labor market outcomes. This connects to the debate on the presence of liquidity constraints in developing countries, and more generally to the concerns about poverty traps, i.e. the inability of people to make investments that could lead to large and lasting

benefits. South Africa has arguably the most extensive and generous cash transfer system in the developing world, and hence it is particularly suited to answer these questions. Moreover, these cash transfers are often unconditional, meaning that they do not impose any conditionalities on recipients' behavior. Because of these unique features, the labor market effects of these programs can be more easily interpreted as a consequence of the extra income that they provide, which is important both to understand and to generalize these findings.

Also, this dissertation relates to the branch of the literature studying labor market segmentation and informality across middle-income countries. This is the stage of development where the issue of segmentation is most pressing. The formal sector is usually very limited in poor countries, and omnipresent in rich ones, which makes this concern less relevant. There is a longstanding debate on the nature of this segmentation, which is as old as the literature itself ([Harris and Todaro \(1970\)](#), [Hart \(1973\)](#)). This has been centered around the question of whether the driving force of sectoral allocation is necessity or choice ([Günther and Launov \(2012\)](#)). An optimistic view of the informal sector proposes that workers allocate themselves according to comparative advantage within a dual labor market where regulations are weakly enforced. From this perspective, the informal sector is the result of workers' choices, who have higher returns (or utility) in the unregulated sector of the economy. Instead, a negative view of segmentation portrays it as the result of barriers and frictions that impede an optimal allocation of workers, or of a limited stock of jobs in either sector. In this view, workers would have higher earnings in another sector, but do not manage to enter it for whatever reason. In terms of policy implications, distinguishing between these views has important repercussions on what is the most effective way to counter segmentation, or whether it should be countered at all. For example, if the decision of holding an informal job comes from an individual unconstrained choice, then policies that play on the incentives margin will have a greater impact. On the other hand, if informality provides a "job of last resort" when there are constraints to enter either sector, then policies that relax these constraints could be welfare improving.

This dissertation uses labor market responses to cash transfers as a way to disentangle these two views. In spirit, this is a “revealed preferences” approach to the question of segmentation, as I argue that workers’ responses to these cash transfer shocks are indirectly revealing of the drivers of segmentation. If liquidity constraints are one of the reasons behind labor market segmentation in the presence of fixed costs to enter either sector, then an unconditional cash transfer would have lasting impacts on sectoral allocation. If, instead, only relative payoffs across sectors matter, then a policy that makes a large amount conditional on a means-test on earnings only in one sector might also induce some type of re-allocation.

The first chapter of this dissertation studies the labor market effects, both in the short and long term, of an unconditional cash transfer program targeted at mothers, the *Child Support Grant* (CSG). An empirical analysis of this program is particularly informative for several reasons. On the one hand, it is similar to other child grants observed in most developing countries, both in terms of real amount per child, and the labelling of the transfer. On the other hand, the CSG is a purely unconditional transfer, which is not the case for most child grants programs. It has no requirements attached to the grant, and its means-test is not actually applied in practice. These features allow to interpret its effect on labor market outcomes of mothers as a pure wealth shock, but framed in the same way as similar programs across the developing world. With respect to the drivers of workers’ allocation in a segmented labor market, the CSG can be seen as an “instrument” to test the importance of the “necessity” channel; an unconditional cash transfer relaxes liquidity constraints, while not affecting relative payoffs across sectors. In practice, it allows to test whether workers who are slightly richer as a result of the transfer, but similar in everything else, end up in different sectors.

In labor and public economics, the impact of more cash-on-hand on job search, and subsequent job quality, is a central topic. A large literature has studied how more generous unemployment benefits or severance pay lengthen job search, with contrasting empirical results on whether longer search leads to better or worse jobs ([Card, Chetty and Weber \(2007\)](#), [Nekoei and Weber \(2017\)](#)). For the most part, this

literature has focused on developed countries, while little is known about the impact of social programs on job search and job quality in developing countries. However, this question may be even more relevant in these contexts, exactly because of the co-existence of different sectors with good and very bad jobs within the same labor market. Also, a key take-away from the unemployment insurance (UI) literature is the importance of taking a dynamic look: “*How can we distinguish whether UI subsidizes unproductive leisure or productive job search? The best way to do so is to study the quality of post-UI job matches*” (Gruber (2005)). The same logic can be applied to the labor market effects of cash transfers: to disentangle whether a cash transfer is subsidizing leisure or search, one can look at the jobs recipients find after. Empirically, the challenge lies in estimating the long-term effects of cash transfers, which I argue is possible with the identification strategy I employ for the *Child Support Grant*.

In this chapter, I exploit discontinuous exposure across children’s birth cohorts caused by reforms in the age threshold, because a child is eligible to the grant only up to a certain age. As this identification allows me to track cohorts over time, I am able to estimate both immediate and persistent effects. In the short term, mothers who receive the *Child Support Grant* search for a job longer than mothers who are never eligible for it. Five years after the transfer was received, this longer search has translated into substantial job quality gains. Indeed, I show that mothers who have received at least one year of grant are significantly more likely to work in the formal sector, and symmetrically less likely to work in the informal sector. These gains are concentrated on single mothers, which I argue is consistent with the liquidity explanation. The impact on total employment in the long term is null, suggesting that cash transfers do not lead to higher employment at the extensive margin.

These persistent job quality gains appear as the result of different targeting during job search, when mothers are less likely to pick up low-quality jobs in the formal sector, in industries and occupations with low wages and retention rates. This is also consistent with the observation that mothers with eligible children spend more on transport when looking for a job. Overall, these findings are in line with the



main predictions of the job search literature, where an exogenous cash grant should unambiguously increase unemployment length. To explain the effect on job quality, I develop a stationary, directed search model with exogenous effort that shows how the impact on job quality is ambiguous: on the one hand, richer individuals gain less from targeting better jobs; on the other hand, they find it less costly to target better jobs as they have more resources to do so, and can search for longer. It is intuitive that this second channel will be of greater relevance the more severe liquidity constraints are.

Overall, the main contribution of this chapter is to show that unconditional cash transfers can have persistent positive effects on job quality in a segmented labor market, which is a new result in the literature. There is, to my knowledge, no other paper showing long-term effects of cash transfers on the labor market outcomes of adult recipients<sup>1</sup>, mostly due to difficulty in having exogenous variation that persists in the long term. The other main take-away is that workers who receive this shock are more likely to end up in the formal sector. This provides evidence in favor of a more negative view of informality, where constraints are in part at play in driving workers across sectors.

The second chapter of this dissertation (co-authored with P. Dutronc-Postel) explores the role of incentives for sectoral allocation in a segmented labor market. In the same spirit as the first chapter, it exploits a policy shock to identify workers' labor responses, from which it tries to draw conclusions about their preferences. A recent literature has raised concerns about the distortions that social programs might introduce in a labor market where there is an important informal sector ([Azulara and Marinescu \(2013\)](#), [Bergolo and Cruces \(2014\)](#), [Gerard and Gonzaga \(2016\)](#)). If workers allocate themselves freely according to expected earnings across sectors, means-tested social policies may trigger movement towards the unregulated, informal sector. To tackle this question, the chapter analyzes the largest social program in South Africa, the *Old Age Pension*, and investigates whether there is any evidence

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<sup>1</sup>The only exception is a recent extension of [Haushofer and Shapiro \(2016\)](#) at the two-year horizon.

of this. As earnings from an informal job *de facto* do not enter the means-test, individuals have the implicit incentive to cumulate the pension with hidden wages from informal work. The relevance of this channel will depend on why workers located in either sector in the first place, their preferences, and their potential wage in the informal sector. As the means-test is located in a portion of the wage distribution where (monthly) wages across sectors largely overlap, there are reasons to believe that this concern might be relevant.

To study this question in detail, we take advantage of a reform of the public, means-tested pension system of South Africa, which lowered the pension age for men from 65 to 60, while leaving it unchanged for women. We show that correct identification of the labor market effects of this program requires the extra variation introduced by the reform. Bias from other private pension would otherwise lead to estimate responses that are twice as large, and that would also drastically change the qualitative interpretation across the formal/informal dimension. Our results show a large extensive margin response in informal employment, as workers quit their informal job when they become eligible to the pension. Contrary to what a standard leisure-work model would predict, this response is of similar size at all levels of the informal wage distribution, suggesting it is not only workers at the lowest level of wages who drop out. On the contrary, the extensive margin response for formal employment is small and insignificant: only formal workers at wages equivalent to the 1st quartile of the *informal* wage distribution quit their jobs, while those with higher wages do not respond. This implies that differential employment responses across formal/informal are not driven by lower wages in the informal sector. At the same level of wages, informal workers quit their job, and formal workers do not, which we interpret either as an indication of heterogeneous preferences/characteristics of the worker, or that these jobs are different across other dimensions. Overall, we find no evidence of any significant substitution from formal to informal, suggesting that concerns about the distortions introduced by means-tested programs might be less relevant.

Lastly, we address whether lowering (public) pension age can free up jobs for

younger workers. As this is motivation often enters the policy debate, even in developing countries, it is a question with important policy implications. Our results suggest that this is unlikely to be the case. Although the extensive margin response from the elderly is fairly large, this group is too small to make a significant difference. We calculate that the reform has freed up at most between 20 000 and 30 000 jobs. By exploiting the differential effect of the reform across occupation and industries, we are able to reject a one-to-one substitution on the closest substitute workers. However, we lack the statistical power to judge whether these jobs have been “lost” or picked up by other workers. In any way, we conclude that, in countries with a similar demographic structure to South Africa, this kind of reform unlikely to have any significant impact on the stock of jobs available to younger workers.

The third chapter addresses an issue that is South Africa-specific, and arguably the main anomaly of the South African labor market: the pervasive absence of self-employment activities. The self-employment rate in South Africa is astonishingly low, only a fraction of what is observed in countries at a similar level of development. In fact, missing self-employment can account for virtually all of the gap with the average employment rate of countries with the same GDP per capita. This peculiarity of the South African labor market is well-known ([Kingdon and Knight \(2007\)](#), [Banerjee et al. \(2008\)](#), [Grabrucker and Grimm \(2018\)](#)), but it is not yet clear what is the reason behind it. Common explanations, such as the high crime rate, or strict labor market regulation, have failed to provide evidence that can be reconciled with the magnitude of this phenomenon ([Grabrucker and Grimm \(2018\)](#), [Magruder \(2012\)](#)). It is also unlikely to be a measurement problem, as the rate remains low across surveys and time, despite alternative measurements of self-employment and significant effort from the statistical agency to get an accurate count. Moreover, the absence of self-employment is limited to the native Black and Coloured population, while the self-employment rate among migrants from other African countries is high. This shows that it is not impossible to be self-employed in South Africa; the question is then why most South Africans do not consider or cannot enter this occupation.

Building onto the results of Chapter 1 and 2, I show that households do not

increase their self-employment when receiving large, unconditional cash transfers. This contrasts with an established finding in the literature in development economics, which has shown that cash transfers increase entry into self-employment ([Bianchi and Bobba \(2013\)](#), [Blattman, Fiala and Martinez \(2013\)](#)). In theory, a cash transfer might promote self-employment by relaxing both liquidity and insurance constraints, as it provides extra income while also insuring against the higher earnings volatility of running a business (relative to wage-employment). Instead, this response is not observed in South Africa, where cash transfer recipients are as likely to be self-employed as non-recipients. For these reasons, I argue that liquidity constraints and risk aversion are unlikely to be the main barrier to self-employment in South Africa. Importantly, this also shows that self-employment is not simply absorbed by the welfare state.

In this chapter, I put forward the hypothesis that the lack of self-employment is likely to have historical roots. During Apartheid, black South Africans were prevented to be self-employed, either in the formal or informal sector. Repression of informal self-employment activities during this period is extensively documented ([Rogerson \(2003\)](#)). With the end of Apartheid, and of these restrictions, policy-makers hoped that informal self-employment would flourish, and absorb most of the growing labor supply ([Rogerson \(2000\)](#)). In the post-Apartheid years, the official government policy switched to one of promotion of the growth of micro business enterprises, which were believed to be a key component of economic growth in South Africa.

This, however, did not happen. Self-employment in South Africa has remained remarkably low since 1994, with no visible growth in the number of self-employed jobs to this day. The hypothesis that the lack of self-employment in South Africa stems from Apartheid is not new. [Kingdon and Knight \(2004\)](#) already underlined the potentially long-lasting consequences of the Apartheid repression that would have inhibited the development of the social capital necessary to enter this occupation. I build upon this argument with insights from the literature in socio-economics, which points to the strong inter-generational component in the transmission of self-employment. Indeed, a constant empirical observation is that the children of

entrepreneurs are much more likely to be entrepreneurs themselves. This correlation is mostly attributed to a role-modelling effect: by observing an entrepreneur parent, children become more aware of this occupational possibility. Intuitively, this would offer an explanation of why younger generations, who have not been directly touched by the Apartheid years, still do not enter self-employment. Simply, role-modelling could not be at play for young South Africans today, as their parents were not allowed to be self-employed. In the chapter, I present some evidence along these lines for migrants to South Africa: those arrived after Apartheid are more likely to be self-employed, and so are their children.

These conclusions give some justification for policy action, suggesting that an intervention may potentially compensate for these missing inter-generational channels. In the chapter, I discuss potential avenues for future research, and what mechanisms, in my opinion, remain to be tested. Whether policy-makers should incentive entry into self-employment in South Africa, as opposed to trying to increase wage-employment, is not uncontroversial. However, wage-employment in South Africa is not low. The wage-employment rate is actually roughly comparable to the average of other middle-income countries. If the policy goal is to increase employment, it seems therefore likely that this will have to come from self-employment, at least in part.

In conclusion, the goal of this PhD dissertation is to bring together an analysis of employment responses to cash transfer programs in South Africa to draw lessons about the functioning of labor markets in middle-income countries. In my reading of these results, the main take-aways of general interest are the following: i) unconditional cash transfers can have lasting consequences on labor market outcomes of adult recipients, in particular my findings show that they can improve job quality, through longer search and presumably targeting of better jobs; also, ii) cash transfer recipients are more likely to end up in the formal sector, which is indirectly revealing of the drivers of segmentation in the labor market of a middle-income country. With respect to means-tested, large public pensions, I find evidence that iii) a means-test does not push people to switch to informal jobs; iv) at a given wage, informal workers quit their jobs, while formal workers do not, when they become eligible for the

public pension. From these findings, I also draw some conclusions that are South Africa-specific, namely that v) self-employment in South Africa does not increase as result of cash transfer programs, which indicates that barriers to entry are of a different nature; and that vi) the self-employment rate in South Africa is unlikely to recover by itself, without some sort of policy intervention to compensate for past restrictions in its labor market.

## **Introduction Générale**

Cette thèse regroupe trois études portant sur les réponses des travailleurs aux transferts monétaires en Afrique du Sud. Elle a pour but d'en tirer des leçons générales sur le fonctionnement des marchés du travail dans les pays à revenu intermédiaire. À ce stade de développement d'une économie, les marchés du travail sont particulièrement complexes, car ils combinent de multiples secteurs où des emplois formels et informels coexistent. Cette segmentation impose de réfléchir à pourquoi et à comment les travailleurs choisissent de se porter vers les différents secteurs. De même, la formulation et l'évaluation des politiques publiques doivent tenir compte de la présence d'un secteur informel non réglementé et de la manière dont l'existence de celui-ci pourrait modifier les incitations des agents lorsqu'ils sont visés par des programmes sociaux.. La pertinence de cette préoccupation, et la question plus générale des politiques publiques qui peuvent conduire à des améliorations du bien-être des travailleurs, dépendent fortement de la nature de ce secteur "parallèle" du marché du travail. Les programmes de transferts monétaires exceptionnels de l'Afrique du Sud offrent des cadres empiriques d'"expériences naturelles" propices pour tester le rôle des contraintes et des incitations dans la répartition des travailleurs par secteur. En les analysant, je tire plusieurs conclusions importantes.

Tout d'abord, il faut expliquer pourquoi cette thèse se concentre sur l'Afrique du Sud, et dans quelle mesure les résultats qu'elle présente sont spécifiques à ce pays. Le marché du travail sud-africain est différent de celui d'un pays à revenu intermédiaire "typique". Contrairement à ce que l'on observe habituellement dans des pays ayant un niveau de développement économique similaire, il est caractérisé par un taux de chômage constamment élevé, un taux d'emploi symétriquement bas et un secteur informel relativement petit. Ces problèmes sont bien connus et documentés depuis la fin de l'apartheid, mais il est difficile de trouver dans la littérature des explications de ces anomalies. À part quelques exceptions ([Kingdon and Knight \(2004\)](#), [Banerjee et al. \(2008\)](#)), la plupart des études économiques sur l'Afrique du Sud ont tenté de tirer des leçons d'intérêt plus général, au lieu de traiter les questions qui sont spécifiques ou uniques à son marché du travail. Les programmes sociaux

innovants du pays fournissent des contextes intéressants qui se prêtent aux méthodes économétriques d'évaluation, avec des caractéristiques qui sont difficiles à trouver dans d'autres pays à revenu intermédiaire. Paradoxalement, si cela a donné lieu à un important volume de recherches en Afrique du Sud, le contexte sud-africain en soi est beaucoup moins bien connu. Cette thèse n'est pas entièrement exempte de cette défaillance. La plupart des analyses présentées dans les chapitres suivants tentent d'utiliser le système peu commun de transferts monétaires de l'Afrique du Sud pour en tirer des leçons d'intérêt plus général et des implications politiques qui ne sont pas nécessairement spécifiques au pays. Toutefois, l'avantage de se concentrer entièrement et largement sur un pays est que cela m'a permis d'expliquer certains des dysfonctionnements spécifiques au marché du travail sud-africain, et comment ils interagissent avec les problèmes généralement présents dans d'autres pays à revenu intermédiaire. Je développe ce raisonnement principalement dans le troisième chapitre, quoique de manière plus descriptive, mais en m'appuyant sur certains des résultats des deux premiers chapitres.

Dans l'ensemble, cette thèse de doctorat contribue à deux domaines de la littérature économique. Tout d'abord, elle vise à étendre l'abondante littérature sur les transferts monétaires. Ils ont reçu une attention croissante en raison de leur omniprésence dans les pays en développement ([Molina Millán et al. \(forthcoming\)](#)), [Haushofer and Shapiro \(2016\)](#)). Cependant, bien que plusieurs de ces programmes aient fait l'objet d'études approfondies sous différents aspects, nous en savons encore très peu sur leurs effets sur le marché du travail des bénéficiaires adultes. Les résultats de plusieurs interventions, menées sur la base d'expériences aléatoires contrôlées, suggèrent que les effets dissuasifs sur le travail sont faibles ([Banerjee et al. \(2017\)](#)). En outre, il y a des raisons théoriques de croire que les transferts monétaires pourraient effectivement avoir des effets positifs sur les emplois des adultes ([Baird, McKenzie and Özler \(2018\)](#)). Du point de vue des politiques publiques, il est essentiel de savoir si le choc induit par un transfert monétaire peut avoir des avantages durables sur l'activité des bénéficiaires, notamment à travers le marché du travail. Cette question est étroitement liée au débat sur l'existence de contraintes de liquidité dans les pays



en développement, et plus généralement aux préoccupations concernant les trappes à pauvreté, c'est-à-dire l'incapacité des agents à engager des investissements qui pourraient entraîner des bénéfices importants et durables, du fait d'une impossibilité de financer ces investissements. L'Afrique du Sud possède sans doute le système de transferts monétaires le plus étendu et le plus généreux du monde en développement. Elle est donc un objet d'étude particulièrement indiqué pour répondre à ces questions. De plus, ces transferts sont souvent inconditionnels, ce qui signifie qu'ils n'imposent aucune condition sur le comportement des bénéficiaires. En raison de ces caractéristiques uniques, les effets de ces programmes sur le marché du travail peuvent être interprétés plus facilement comme une conséquence du revenu supplémentaire qu'ils procurent, ce qui est important tant pour comprendre que pour généraliser ces résultats.

Cette thèse s'inscrit également au sein d'une littérature académique qui étudie la segmentation du marché du travail et l'informalité dans les pays à revenu intermédiaire. C'est à ce stade de développement économique d'un pays que la question de la segmentation des marchés du travail est la plus cruciale. En effet, le secteur formel est généralement très limité dans les pays pauvres et omniprésent dans les pays riches, ce qui y rend cette problématique moins pertinente. Il y a un débat de longue date sur la nature de cette segmentation, qui remonte aux origines de cette littérature elle-même ([Harris and Todaro \(1970\)](#), [Hart \(1973\)](#)). Ce débat a pour coeur l'origine de l'allocation sectorielle des individus, et plus précisément, le fait que l'allocation sectorielle se fasse par "nécessité ou [par] choix" ([Günther and Launov \(2012\)](#)). Deux conceptions du secteur informel s'opposent dans ce débat. La première, optimiste, suppose que les travailleurs se répartissent en fonction de leur avantage comparatif au sein d'un marché du travail dual où les réglementations sont asymétriquement appliquées. De ce point de vue, le secteur informel naît du choix des travailleurs qui ont un rendement (ou une utilité) plus élevé dans le secteur non réglementé de l'économie. Au contraire, une deuxième conception, négative, de la segmentation la présente comme le résultat d'obstacles et de frictions qui empêchent une répartition optimale des travailleurs ou d'un stock limité d'emplois dans les

deux secteurs. De ce point de vue, les travailleurs auraient des revenus plus élevés dans le secteur formel, mais ne parviennent pas à y entrer pour une raison ou une autre. En termes d'implications pour les politiques publiques, la distinction entre ces points de vue a des répercussions importantes sur la manière la plus efficace de lutter contre la segmentation, ou même sur la pertinence de s'en préoccuper. Par exemple, si la décision de travailler dans le secteur informel provient d'un choix individuel sans contrainte, alors les politiques qui jouent sur la marge des incitations auront un impact plus important. D'un autre côté, si l'emploi informel constitue un "emploi de dernier recours" lorsqu'il y a des contraintes pour entrer dans l'un ou l'autre secteur, alors les politiques qui assouplissent ces contraintes pourraient potentiellement améliorer le bien-être des travailleurs.

Cette thèse utilise les réponses du marché du travail aux transferts monétaires pour faire la distinction entre ces deux points de vue. Dans l'esprit, il s'agit d'une approche de la question de la segmentation dite de "préférences révélées". Je soutiens que les réponses des travailleurs à ces chocs de transfert monétaire sont indirectement révélatrices des déterminants de cette segmentation. Si les contraintes de liquidité sont l'une des raisons de la segmentation du marché du travail en présence de coûts fixes pour entrer dans l'un ou l'autre secteur, alors un transfert monétaire inconditionnel aurait des effets durables sur la répartition sectorielle. Si, au contraire, seuls les gains relatifs d'un secteur par rapport à l'autre comptent, une politique qui subordonne un montant important à un critère de ressources sur les gains d'un seul secteur pourrait également induire un certain type de réaffectation.

Le premier chapitre de cette thèse étudie les effets sur le marché du travail, à court et à long terme, d'un programme de transferts monétaires inconditionnels destinés aux mères, le *Child Support Grant* (CSG). L'analyse empirique de ce programme est particulièrement intéressante pour plusieurs raisons. D'une part, cette aide est similaire à d'autres allocations familiales observées dans la plupart des pays en développement, tant en termes de montant réel par enfant qu'en ce qui concerne la dénomination du transfert. D'autre part, le CSG est un transfert purement inconditionnel, ce qui n'est pas le cas de la plupart des programmes

de subventions pour enfants. La subvention n'est subordonnée à aucune exigence particulière et sa condition de ressources n'est pas réellement appliquée en pratique. Ces caractéristiques permettent d'interpréter son effet sur l'emploi des mères comme un choc purement monétaire, mais de le formuler de la même manière que des programmes similaires dans le monde en développement. En ce qui concerne les déterminants de l'allocation des travailleurs dans un marché du travail segmenté, le CSG peut être considéré comme un "instrument" pour tester l'importance des contraintes monétaires; un transfert sans condition permet d'atténuer les contraintes de liquidités, sans pour autant affecter les gains relatifs entre secteurs. En pratique, elle permet donc de vérifier si des travailleurs légèrement plus riches du fait du transfert, mais similaires dans tous les autres domaines, se retrouvent dans des secteurs différents.

En économie du travail et en économie publique, l'impact de ressources monétaires supplémentaires sur la recherche d'emploi et la qualité de l'emploi qui en découle est un sujet central. Un grand nombre d'études ont montré comment des allocations de chômage ou des indemnités de départ plus généreuses allongent la durée de la recherche d'emploi, avec des résultats empiriques contradictoires sur le fait que la recherche prolongée permet ou non l'obtention d'un meilleur emploi ([Card, Chetty and Weber \(2007\)](#), [Nekoei and Weber \(2017\)](#)). Cette littérature s'est surtout concentrée sur les pays développés, alors que l'on sait peu de choses sur l'impact des programmes sociaux sur la recherche d'emploi et la qualité des emplois dans les pays en développement. Cependant, cette question est peut-être encore plus pertinente dans ces pays, précisément en raison de la coexistence de différents secteurs avec de bons et de très mauvais emplois au sein d'un seul et même marché du travail. De plus, l'un des principaux points à retenir de la littérature sur l'assurance-chômage (AC) est l'importance d'adopter une perspective dynamique : *“Comment peut-on distinguer si l'assurance-chômage subventionne le loisir improductif ou la recherche d'emploi productive ? La meilleure façon de le faire est d'étudier la qualité des appariements après l'assurance-chômage”* ([Gruber \(2005\)](#)). La même logique peut s'appliquer aux effets des transferts monétaires sur le marché du travail : pour déterminer si un

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transfert monétaire subventionne le loisir ou la recherche d'emploi, on peut examiner les emplois que les bénéficiaires trouvent à la suite de ce transfert. Empiriquement, le défi consiste à estimer les effets à long terme des transferts monétaires, ce qui est possible grâce à la stratégie d'identification que j'emploie pour l'étude du *Child Support Grant*.

Un enfant n'est éligible à la subvention que jusqu'à un certain âge. Dans ce chapitre, j'exploite l'exposition discontinue des cohortes de naissance des enfants due à l'existence d'un seuil d'âge qui a fait l'objet d'une réforme. Comme cette identification me permet de suivre les cohortes dans le temps, je suis en mesure d'estimer à la fois les effets immédiats et persistants de la réforme. À court terme, les mères qui reçoivent le *Child Support Grant* cherchent un emploi plus longtemps que les mères qui n'y sont jamais éligibles. Cinq ans après la réception du transfert, cette recherche plus longue donne lieu à des gains substantiels en termes de qualité de l'emploi. En effet, je montre que les mères qui ont reçu au moins un an d'allocations sont beaucoup plus susceptibles de travailler dans le secteur formel et, symétriquement, moins susceptibles de travailler dans le secteur informel. Ces gains sont concentrés sur les mères seules, ce qui est cohérent avec l'hypothèse d'un choc de liquidité. L'impact sur la probabilité d'emploi total à long terme est nul, ce qui donne à penser que les transferts monétaires n'entraînent pas une hausse du volume d'emploi.

Ces gains persistants sur le plan de la qualité de l'emploi s'expliquent par un comportement différent au cours de la recherche d'emploi: les mères sont moins susceptibles de rechercher des emplois de faible qualité dans le secteur formel, dans les industries et les professions où les salaires et les taux de rétention sont faibles. Cette conclusion concorde également avec l'observation selon laquelle les mères ayant des enfants éligibles dépensent davantage dans les transports lorsqu'elles cherchent un emploi. Dans l'ensemble, ces résultats sont conformes aux principales prédictions de la littérature sur la recherche d'emploi, selon lesquelles une subvention exogène devrait sans ambiguïté augmenter la durée du chômage. Pour expliquer l'effet sur la qualité de l'emploi, j'ai mis au point un modèle de recherche stationnaire, dit de la "recherche orientée", qui montre à quel point l'impact sur la qualité de l'emploi

est ambigu : d'une part, les personnes plus riches gagnent moins à privilégier de meilleurs emplois; d'autre part, il est moins coûteux pour elles de privilégier de meilleurs emplois car elles ont plus de ressources pour ce faire et peuvent demeurer en situation de recherche d'emploi pendant plus longtemps. Intuitivement, plus les contraintes de liquidité seront fortes, plus ce second canal sera important.

Dans l'ensemble, la principale contribution de ce chapitre est de montrer que les transferts monétaires inconditionnels peuvent avoir des effets positifs persistants sur la qualité de l'emploi dans un marché du travail segmenté, ce qui est un résultat nouveau dans la littérature. Il n'existe, à ma connaissance, aucune autre étude montrant les effets à long terme des transferts monétaires sur les emplois de bénéficiaires adultes<sup>2</sup>, principalement en raison de la difficulté qu'il existe à observer des variations exogènes qui persistent sur le long terme. L'autre principale conclusion est que les travailleurs qui reçoivent ce choc sont plus susceptibles de se retrouver dans le secteur formel. Cela confirme une vision plus négative de l'informalité, où l'allocation sectorielle des travailleurs est déterminée par des contraintes exogènes.

Le deuxième chapitre de cette thèse (co-écrit avec P. Dutronc-Postel) explore le rôle des incitations dans la répartition sectorielle sur un marché du travail segmenté. Dans le même esprit que le premier chapitre, il exploite un choc dérivant d'une politique publique pour identifier les réponses des travailleurs et en tirer des conclusions sur leurs préférences. Une littérature récente souligne les distorsions que les programmes sociaux pourraient introduire sur un marché du travail où il existe un secteur informel important ([Azuara and Marinescu \(2013\)](#), [Bergolo and Cruces \(2014\)](#), [Gerard and Gonzaga \(2016\)](#)). Si les travailleurs se répartissent librement en fonction des revenus anticipés dans chaque secteur, les politiques sociales soumises à une condition de ressources peuvent entraîner un mouvement vers le secteur informel non réglementé. Pour aborder cette question, le chapitre analyse le plus grand programme social d'Afrique du Sud, la *Old Age Pension*, et examine s'il existe effectivement un effet d'entraînement vers le secteur informel. Comme les

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<sup>2</sup>La seule exception est une extension récente de [Haushofer and Shapiro \(2016\)](#), mais seulement à un horizon de deux ans.

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revenus d'un emploi informel *de facto* n'entrent pas dans le critère des ressources, les individus sont implicitement incités à cumuler la pension avec les salaires cachés du travail informel. La pertinence de ce canal dépendra de la raison pour lesquelles les travailleurs se trouvent en premier lieu dans chaque secteur, de leurs préférences et de leur salaire potentiel dans le secteur informel. Étant donné que le seuil du critère des ressources se situe dans une partie de la distribution des salaires où les salaires des secteurs formel et informel coexistent, il y a lieu de croire que cette préoccupation pourrait être pertinente.

Pour étudier cette question en détail, nous nous appuyons sur une réforme du système public de retraites sud-africain, qui a abaissé l'âge d'éligibilité à la retraite des hommes de 65 à 60 ans, tout en le laissant inchangé pour les femmes. Nous montrons que l'identification correcte des effets de ce programme sur le marché du travail nécessite la variation supplémentaire introduite par la réforme. Autrement, le simple examen de la discontinuité des comportements au seuil de 60 ans serait soumis au biais généré par l'existence d'autres régimes de retraite privés au même seuil, et mènerait à estimer des réponses deux fois plus importantes. Cela modifierait aussi radicalement l'interprétation qualitative dans la dimension formelle/informelle. Nos résultats montrent une forte réaction de l'emploi informel: il semble que les travailleurs quittent leur emploi informel lorsqu'ils deviennent éligibles à la pension. Contrairement à ce qu'un modèle standard d'arbitrage entre consommation et loisir prédirait, cette réponse est quantitativement similaire à tous les niveaux de la distribution des salaires informels. Cela suggère que ce ne sont pas seulement les travailleurs au niveau le plus bas des salaires qui abandonnent leur travail. Au contraire, la réponse de l'emploi formel est faible et non-significative : seuls les travailleurs formels gagnant des salaires équivalents au 1er quartile de la distribution salariale *informelle* quittent leur emploi, alors que ceux dont les salaires sont plus élevés ne réagissent pas. Cela signifie que les différences de réponses entre les secteurs formel et informel ne sont pas dues à des salaires plus bas dans le secteur informel. À niveau de salaire égal, les travailleurs informels quittent leur emploi, et les travailleurs formels ne le font pas. Nous interprétons cette asymétrie soit comme une indication

de préférences et de caractéristiques hétérogènes parmi les travailleurs de ces deux secteurs, soit comme une différence entre ces emplois dans d'autres dimensions. Dans l'ensemble, nous n'avons trouvé aucune indication d'une substitution importante entre formel et informel, ce qui donne à penser que les préoccupations concernant les distorsions introduites par les programmes imposant une condition du revenu pourraient être peu pertinentes.

Enfin, nous examinons si l'abaissement de l'âge de la retraite (publique) peut libérer des emplois pour les jeunes travailleurs. Comme cette motivation entre souvent dans le débat politique, même dans les pays en développement, il s'agit d'une question aux implications politiques importantes. Nos résultats suggèrent qu'il est peu probable que ce soit le cas. Bien que la réponse des personnes âgées soit assez importante au sein du groupe des personnes concernées par la réforme, ce groupe est trop petit pour faire une différence significative à l'échelle du marché du travail dans son ensemble. Nous estimons que la réforme a libéré tout au plus entre 20 000 et 30 000 emplois. En exploitant les différences de magnitude de l'effet de la réforme sur les différentes professions et les différents secteurs d'activité, nous sommes en mesure de rejeter l'hypothèse que la réforme ait substitué un travailleur aux caractéristiques très similaires aux bénéficiaires pour chaque travailleur qui quitte le marché du travail en conséquence de la réforme. Cependant, la magnitude de l'effet de la réforme, par rapport aux variations temporelles de l'emploi sud-africain, n'offre pas un pouvoir statistique suffisant pour juger si ces emplois ont été "perdus" ou s'ils ont été repris par d'autres travailleurs. En tout état de cause, nous concluons que, dans les pays ayant une structure démographique similaire à celle de l'Afrique du Sud, il est peu probable que ce type de réforme ait un impact significatif sur le stock d'emplois accessibles aux jeunes travailleurs.

Le troisième chapitre traite d'une question spécifique à l'Afrique du Sud, qui constitue sans doute la principale anomalie du marché du travail sud-africain: l'absence généralisée de travailleurs indépendants. Le taux des travailleurs indépendants en Afrique du Sud est remarquablement bas, une fraction seulement de ce que l'on observe dans des pays ayant un niveau de développement économique similaire.

L'absence de travail indépendant peut expliquer la quasi-totalité de l'écart avec le taux d'emploi moyen des pays ayant le même PIB par habitant. Cette particularité du marché du travail sud-africain est bien connue ([Kingdon and Knight \(2007\)](#), [Banerjee et al. \(2008\)](#), [Grabruker and Grimm \(2018\)](#)), mais les pistes d'explications restent limitées. Des explications communes, comme le taux de criminalité élevé ou la réglementation stricte du marché du travail, ne sont pas conciliables avec l'ampleur de ce phénomène ([Grabruker and Grimm \(2018\)](#), [Magruder \(2012\)](#)). Il est également peu probable qu'il s'agisse d'un problème de mesure, car le taux reste faible dans toutes les données de différentes enquêtes et à travers le temps, malgré les différentes mesures du travail indépendant et les efforts considérables de l'agence statistique sud-africaine pour obtenir un compte exact. En outre, l'absence de travail indépendant est limitée à la population noire native, alors que le taux de travail indépendant parmi les migrants d'autres pays africains est élevé. Cela montre qu'il n'est pas impossible d'être travailleur indépendant en Afrique du Sud ; la question est alors de savoir pourquoi la plupart des Sud-Africains n'envisagent pas ou ne peuvent pas exercer cette activité.

En m'appuyant sur les résultats des chapitres 1 et 2, je montre que les ménages ne travaillent pas plus à leur compte lorsqu'ils reçoivent des transferts monétaires substantiels et inconditionnels. Cette observation contraste avec un résultat établi dans la littérature de l'économie du développement, qui a montré que les transferts monétaires augmentent l'entrée dans le travail indépendant ([Bianchi and Bobba \(2013\)](#), [Blattman, Fiala and Martinez \(2013\)](#)). En théorie, un transfert monétaire pourrait favoriser le travail indépendant en assouplissant à la fois les contraintes de liquidité et d'assurance, car il procure un revenu supplémentaire tout en permettant de se prémunir contre la plus grande volatilité des revenus liés à la gestion d'une entreprise (par rapport au travail salarié). Cette réaction n'est pas observée en Afrique du Sud, où les bénéficiaires de transferts monétaires sont aussi susceptibles d'être des travailleurs indépendants que les non-bénéficiaires. Pour ces raisons, je soutiens que les contraintes de liquidité et l'aversion pour le risque ne sont probablement pas le principal obstacle au travail indépendant en Afrique du Sud. Ces résultats montrent



aussi que le système de protection sociale sud-africain n'a pas d'effet dissuasif fort sur le travail indépendant.

Dans ce chapitre, j'avance l'hypothèse que l'absence de travail indépendant a vraisemblablement des racines historiques. Pendant l'apartheid, les Sud-Africains noirs n'ont pas pu exercer une activité indépendante, que ce soit dans le secteur formel ou informel. La répression des activités de travail indépendant informel pendant cette période est abondamment documentée (Rogerson (2003)). Avec la fin de l'apartheid et de ces restrictions, les responsables politiques espéraient que le travail indépendant informel prospère et absorbe une grande partie de la main d'œuvre croissante (Rogerson (2000)). Dans les années qui ont suivi la fin de l'apartheid, la politique officielle du gouvernement s'est orientée vers la promotion des micro-entreprises, qui étaient considérées comme un élément clé pour la croissance économique en Afrique du Sud.

Cependant, cette évolution ne s'est pas produite. La proportion de travailleurs indépendants en Afrique du Sud est restée remarquablement basse depuis 1994. L'hypothèse selon laquelle le manque de travailleurs indépendants en Afrique du Sud serait dû à l'apartheid n'est pas nouvelle. Kingdon and Knight (2004) soulignait déjà les conséquences potentiellement durables de la répression durant l'apartheid, qui aurait empêché le développement du capital social nécessaire pour accéder à cette profession. Je m'appuie sur cet argument pour tirer des enseignements de la littérature socio-économique, qui met en lumière la forte composante intergénérationnelle de la transmission de la propension à exercer une activité indépendante. En effet, une observation empirique constante est que les enfants d'entrepreneurs sont beaucoup plus susceptibles d'être eux-mêmes des entrepreneurs. Cette corrélation est surtout attribuée à un effet d'imitation : en observant un parent entrepreneur, les enfants deviennent plus conscients de cette possibilité. Intuitivement, cela expliquerait pourquoi les jeunes générations, qui n'ont pas été directement touchées par les années de l'apartheid, ne se lancent toujours pas dans le travail indépendant. Tout simplement, les jeunes Sud-Africains d'aujourd'hui ne peuvent pas imiter leurs parents, car ceux-ci ne pouvaient pas exercer une activité indépendante. Dans le

chapitre, je présente quelques éléments qui vont dans ce sens, en m'intéressant aux migrants en Afrique du Sud et à leur descendance : ceux qui sont arrivés dans le pays après l'apartheid sont plus susceptibles d'être des travailleurs indépendants, tout comme leurs enfants.

Ces conclusions justifient en partie une action de politique publique, en suggérant qu'une intervention peut potentiellement compenser ce manque de transmission intergénérationnelle. Dans ce chapitre, je discute des pistes de recherche possibles et des mécanismes qui, selon moi, doivent encore être mis à l'essai. La question de savoir si les décideurs politiques devraient encourager l'entrée dans l'entrepreneuriat en Afrique du Sud, plutôt que d'essayer d'augmenter l'emploi salarié, n'est pas sans controverse. Cependant, le taux d'emploi salarié en Afrique du Sud n'est pas faible, il est même à peu près comparable à la moyenne des autres pays à revenu intermédiaire. Si la priorité est d'accroître l'emploi, il semble donc probable qu'il devra provenir, au moins en partie, du travail indépendant.

En conclusion, l'objectif de cette thèse de doctorat est de proposer une analyse des réponses aux programmes de transferts monétaires en Afrique du Sud pour tirer des leçons sur le fonctionnement des marchés du travail dans les pays à revenu intermédiaire. D'après ma lecture de ces résultats, les principales conclusions d'intérêt général sont les suivantes: i) les transferts monétaires non conditionnels peuvent avoir des conséquences durables sur les emplois des bénéficiaires adultes sur le marché du travail; en particulier, mes conclusions montrent qu'ils peuvent améliorer la qualité des emplois, grâce à une recherche prolongée qui mène à de meilleurs emplois; ii) en outre, les bénéficiaires des transferts monétaires ont une plus forte probabilité de travailler dans le secteur formel, ce qui révèle indirectement certains facteurs de segmentation du marché du travail dans un pays à revenu intermédiaire. En ce qui concerne les pensions publiques soumises à conditions de ressources, mes résultats montrent que iii) un transfert sous condition de revenu ne pousse pas les gens à se tourner vers l'emploi informel; iv) à salaire donné, les travailleurs informels quittent leur emploi, alors que les travailleurs formels ne le font pas, quand ils deviennent éligibles à la pension publique. De ces résultats, je tire également des conclusions

spécifiques à l'Afrique du Sud, à savoir que v) le taux d'emploi indépendant en Afrique du Sud n'augmente pas en conséquence des programmes de transferts monétaires, ce qui indique que les barrières à l'entrée sont de nature différente et vi) qu'il est peu probable que le taux d'activité indépendante en Afrique du Sud augmente sans intervention politique qui compenserait les restrictions passées qui ont pesé sur son marché du travail.

# Chapter 1

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

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## **Abstract**

Can cash with “no strings attached” have long-term benefits on the employment outcomes of adults in developing countries? This paper studies the impact of a large-scale, unconditional cash transfer in South Africa targeted 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 children were born one year apart to identify the short and long-term labor market effects of the grant. As a job search model would predict, in the short term, mothers are more likely to be unemployed and less likely to be working. Five years after the transfer was received, the employment rate is back to the same level as it is for ineligible mothers; the mothers who benefited for at least one year, however, are more likely to work in the formal sector. This appears to be the result of mothers targeting better jobs in the presence of high search costs.

**JEL Codes:** J46, J64, O17, I38

**Keywords:** unconditional cash transfers; job quality; informal sector; South Africa

## 1.1 Introduction

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

In order to shed more light on the topic, this paper analyzes the labor market effects of a child grant in a dynamic way. This form of cash transfer is very common in developing countries, yet the South African *Child Support Grant* (CSG) has some unique features that make its analysis particularly informative. 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, targeting, and the size of its income component are comparable to that of many similar policies across the developing world, which adds to the external validity of this analysis. For identification, I exploit exogenous variation in eligibility to the grant across children’s birth years. In the main specification, this allows me to pin down the labor market effects on mothers who have received roughly one year of grant<sup>1</sup>, both during the period of receipt and after the grant has stopped. As this specific shock occurs when the child is already quite old, I argue that direct effects on children are likely to be very limited. I find that recipient mothers are more likely to be unemployed, and less likely to take up low-quality, formal sector jobs, when receiving the transfer. Five years after the grant has stopped, the employment rate

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

is the same between mothers who have received the CSG and those who haven't. The employment composition, however, is not the same: "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. This appears to be the result of targeting better, more-stable jobs while the grant is received. Significantly longer periods of eligibility to the grant do not seem to cause further job quality gains, which suggests a non-linear effect. Lastly, the effect on overall income is positive but far from statistically significant. Due to data constraints, the minimum detectable effect is well above what could realistically be identified.

This paper connects with the debate in labor and development economics on the nature of segmented labor markets in developing countries. This literature has been characterized by a dichotomy of views, also referred to as the "segmentation" and the "comparative advantage" hypotheses ([Günther and Launov \(2012\)](#)).<sup>2</sup> The essence of the dispute is whether the segmentation observed in the labor market of developing countries is the result of frictions and barriers that are impeding an optimal allocation of workers, or the result of revealed preferences. The recent consensus is that each explanation holds for some portion of the informal workforce. 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<sup>3</sup> 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 Ranchhod \(2012\)](#), [Azuara and Marinescu \(2013\)](#), [Bergolo and Cruces \(2014\)](#), [Garganta and Gasparini \(2015\)](#), [Gerard and Gonzaga \(2016\)](#)).<sup>4</sup> The main conceptual difference from the *Child Support Grant* in

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<sup>2</sup>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: "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?"

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

<sup>4</sup>[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

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 have both an income and a substitution effect, I argue that with the CSG, one is able to capture whether a pure income shock has lasting effects on both job quantity and job quality. 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.<sup>5</sup>

The interaction of social programs and job search is at the heart of labor and public economics. Many studies have examined the impact of severance pay or unemployment benefits on unemployment duration, and the quality of the subsequent job ([Card, Chetty and Weber \(2007\)](#), [Nekoei and Weber \(2017\)](#)). In the context of developed countries, this has led to a variety of different findings.<sup>6</sup> 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 largely ignored the context of developing countries, where this question may be even more relevant because of the high segmentation within the labor market. This may be because the programs usually analyzed for this type of evaluation, such as unemployment benefits

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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 \(2014\)](#) look 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 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.

<sup>5</sup>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. 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.

<sup>6</sup>See [Nekoei and Weber \(2017\)](#) for a detailed summary of the empirical evidence.



or severance pay, are often less relevant in the context of developing countries.<sup>7</sup> Therefore, whether an exogenous cash transfer can raise future job quality is a key question with important policy implications. As [Card, Chetty and Weber \(2007\)](#) say, “*testing this prediction sheds light on whether improvements in future job outcomes provide a rationale for temporary income support programs.*”<sup>8</sup> 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, a simple adaptation of a directed job search model helps to frame the results of this paper.

Despite the large literature on cash transfers, 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, McKenzie and Özler \(2018\)](#) for summary). However, while the long-term effects of cash transfers on children have received significant attention (see [Molina Millán et al. \(forthcoming\)](#) for review), similar evidence for adults is rare ([Baird, McKenzie and Özler \(2018\)](#)).<sup>9</sup> 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 for a relatively small amount of money. This finding also helps to better understand the short-term responses in a dynamic way. In this study, the decrease in employment and longer search as a result of the cash grant leads recipients to better-quality jobs five years after. In this light, the negative, short-term employment effect is the result of 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, target jobs in better occupations, and who also, in the process, spend more money.

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<sup>7</sup>One notable exception is [Gerard and Gonzaga \(2016\)](#), who study how informality in Brazil impacts the efficiency cost of unemployment insurance, but do not look at subsequent job quality.

<sup>8</sup>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.

<sup>9</sup>[Bianchi and Bobba \(2013\)](#) find no long-term effects on the total population of a cash transfer program in Mexico on the probability of self-employment. In a recent extension to [Haushofer and Shapiro \(2016\)](#), the authors have shown that an unconditional cash transfer can have a persistent positive effect on assets at the two-year horizon.

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 in a directed-search framework. 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 possible channels in Section 6. Section 7 concludes.

## 1.2 The South African *Child Support Grant*

The *Child Support Grant* is the largest social program in South Africa in terms of participants, with a number that reached around 10 million children in 2010 (roughly 20% of the population); it is also the second largest program in terms of government spending (Gomersall (2013)).<sup>10</sup> First implemented in April 1998 in post-Apartheid South Africa with the aim of reducing poverty and inequality, it is generally considered to be the main anti-poverty policy of the South African government. The other two main social grants are the *Disability Grant* and the *Old Age Pension*, which cover either individuals who cannot work or who have reached pension age without a private pension.<sup>11</sup>

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, Kannemeyer and Woolard (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, to a lesser extent, White).<sup>12</sup> For both these reasons, overall coverage of the SMG in the early 1990s was lower than 1%.

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

<sup>11</sup>Coverage of these other grants at the household level is presented in Figure 1.A.1 in the Appendix, and does not vary significantly over time.

<sup>12</sup>“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, Kannemeyer and Woolard (2009))

Table 1.1: 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: a) having children of a certain age; and b) an income below a certain 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.<sup>13</sup> Obtaining access to the grant requires very few documents: an identity card, a birth certificate, and proof of earnings (but this last requirement is flexible, as discussed in the next paragraphs). The grant is paid to the “primary caregiver” of the child; hence, it is not exclusive to the parents (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.<sup>14</sup> In practice, the CSG is paid out almost exclusively to women,<sup>15</sup> and the biological mother of the child is the direct recipient a large majority of the time. The Black and Coloured population is disproportionately represented among CSG recipients, while less than

<sup>13</sup>This is true for biological children. For non-biological children, only up to 6 grants can be paid.

<sup>14</sup>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 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>)

<sup>15</sup>In only 4% of the cases, a man reports receiving the grant.

1% of recipients are White (*Source*: National Income Dynamics Survey (NIDS) - 2008).

Table 1.1 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 per month, and its 1998 amount is comparable to that of *Progresa* in Mexico for the same year (Bianchi and Bobba (2013)). In real terms, the amount of the transfer has increased by slightly less than 40% since its implementation.<sup>16</sup> 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 kept fixed for the entire period from 1998 to 2008.<sup>17</sup> Finally, in 2008, the means-test was harmonized – to 10 times the grant for individual incomes, and to 20 times the grant for the pooled income of the caregiver and his/her spouse – and it has not changed since.

While the age threshold is strictly applied through the birth certificate requirement, the means-test has not always been. Lund (2007), head of the committee behind the creation of this program, states clearly that since its inception the means-test was put in place to discourage applications from richer individuals, rather than as a strict threshold. Consistently, there is very little evidence of its application, in particular before 2008. Qualitative research has found “that the various elements of the means test are not generally enforced, understood or relevant.” 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, Rosa and Hall (2008)), which make this constraint *de facto* non-binding. In Figure 1.A.2, I show that in early 2008, before the means-test was reformed, take-up as a function

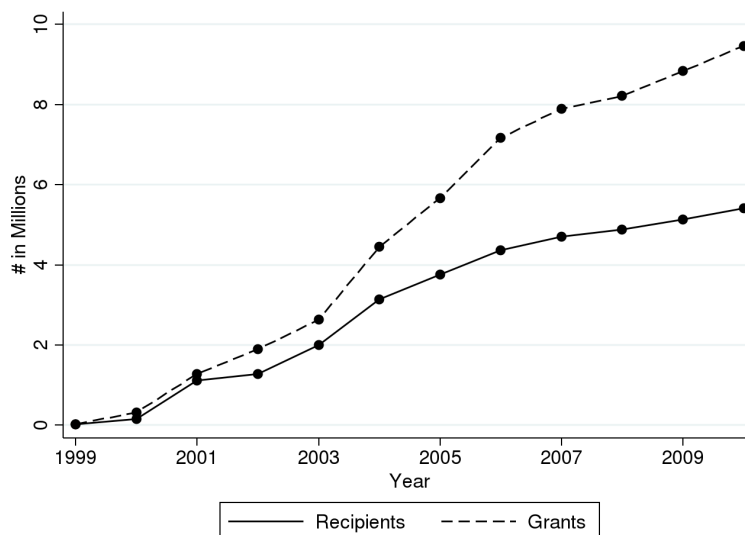
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<sup>16</sup>The amount of the grant in real terms is obtained by adjusting for inflation, measured by CPI at the national level. CPI data is obtained from OECD.stat.

<sup>17</sup>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)).

of earnings was similar for both formal wage, which should enter the means-test, and informal wage, which should not. This reinforces the argument that, at least before the reform, the means-test was not binding, nor perceived as such.

Figure 1.1: 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.

Figure 1.1 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 eligible children began increasing dramatically. The steepest increase in coverage occurs from 2003, when the age-eligibility threshold doubled in only three years from 7 to 14. These fast-paced reforms made more children eligible for the grant. The age threshold was stable from 2005 to 2009, and then gradually increased from 14 to 18 on January 1st of every year. Contrary to the previous increase, these reforms only increased duration for cohorts that were already receiving the grant. The massive means-test reform that occurred at the end of 2008 does not seem to have led to a discontinuous jump in take-up, which was increasing smoothly from 2006 and then stabilizes around 2010. This is consistent

with the view that the means-test was not strictly applied, and that age-eligibility was often the only binding criteria.<sup>18</sup>

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); for a full review, see Eyal and Woolard (2013)), who find 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, Scarlato and Napolitano (2018)). Only a few papers have begun to investigate the effects of this program on the labor market outcomes of the parents.<sup>19</sup> More attention has been given to the labor market effects of other cash transfer programs in South Africa, such as the *Old Age Pension* (Ardington, Case and Hosegood (2009), Abel (2013)), but evidence on working-age adults is mixed.<sup>20</sup>

### 1.3 Conceptual Framework

The job search literature provides a useful way to frame the results of this

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<sup>18</sup>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 couples, 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 couples, we might have expected to observe very different evolutions for these subgroups, but that is not the case (Figure 1.A.3). 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%).

<sup>19</sup>Eyal and Woolard (2011), OECD (2011) find some evidence that the CSG might increase employment by comparing mothers of children of a given age, before and after the reforms. On the contrary, Bengtsson (2012) compares the marginal effect on earnings of having a child in the household before and after the CSG was implemented. He finds that CSG receipt lowers the marginal propensity to earn (through lower labor supply) and increases consumption and expenditure. Berg (2013) looks at how households respond in terms of expenditure when the grant lapses, finding no decrease in expenditure when the child reaches the age eligibility threshold.

<sup>20</sup>The *Old Age Pension* is a public pension system in South Africa that provides an unconditional cash transfer, paid to individuals of a certain age regardless of previous pension contributions. Ardington, Case and Hosegood (2009) find that, contrary to what previous cross-sectional analyses had suggested, the positive income shock that occurs when an older member of the household reaches pension age leads to a significant increase in employment for working-age 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 result on employment of working-age adults.

paper. While this literature has mostly focused on unemployment benefits, these are often not relevant in the context of developing countries. Conceptually, while unemployment benefits only increase the value of unemployment, an unconditional cash transfer raises both the value of employment and unemployment. [Card, Chetty and Weber \(2007\)](#) show that in a job search model incorporating assets and effort decisions (a simplified version of [Lentz and Tranaes \(2005\)](#)), an exogenous cash grant should result in less search effort and longer unemployment, if individuals cannot perfectly smooth consumption between states. They find a null effect on subsequent wage of more cash-on-hand, in line with the empirical literature on unemployment benefits that has found impacts all across the range from positive to negative. [Nekoei and Weber \(2017\)](#) show that these diverse findings can be reconciled with a *directed* search model, where two opposing forces drive the impact 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. Intuitively, these channels make the sign of the effect of a positive shock in assets on subsequent job quality theoretically ambiguous, raising an empirically interesting question. From a policy standpoint, understanding whether cash transfers can lead to persistent job quality gains is key, both as an additional rationale for this type of policy, and, more generally, to know whether relaxing liquidity constraints can be a way to better labor market outcomes for recipients. This is not obvious. If there are barriers other than financial constraints (such as low human capital, for example) that prevent access to “good” jobs, then the labor market effects of such a policy, at least through the job search channel, should be null or small.

### **1.3.1 A Directed Search Model**

#### **1.3.1.1 Model Set-up**

In order to formalize this intuition, I propose a simple job search framework with the same set-up as [Card, Chetty and Weber \(2007\)](#), but following a *directed* search framework à la [Nekoei and Weber \(2017\)](#). This implies that individuals choose  $w$ , i.e.

what wage to target, which they then receive indefinitely when they get a job (no subsequent job destruction). In the model, for simplicity, wage is the only dimension of job quality. The job arrival rate,  $s_t(w)$ , is a decreasing function of the wage targeted, i.e. better jobs automatically take a longer search (effort is exogenous). Similarly, the monetary cost of the search,  $m_t(w)$ , is an increasing function of the target wage: better jobs require higher search costs.<sup>21</sup> Similarly to [Card, Chetty and Weber \(2007\)](#), individuals pay the cost of the search at the beginning of the period, and immediately begin their job if the search is successful, or stay unemployed otherwise.

### 1.3.1.2 Optimal Target Wage

Following similar notation to [Card, Chetty and Weber \(2007\)](#), one can write the value function of finding a job at the beginning of period  $t$  as:

$$V_t(A_t, w) = \max_{A_{t+1} \geq L} u(A_t - m_t(w) - A_{t+1}/(1+r) + w) + \frac{1}{1+\delta} V_{t+1}(A_{t+1}, w) \quad (1.1)$$

where  $A_t$  are assets at time  $t$ ,  $r$  is the interest rate in the economy,  $w$  is the wage, and  $\delta$  is the time discount factor.  $L$  is a potentially binding lower bound on assets. The value of being unemployed at the beginning of period  $t$  for those who do not find a job can be expressed as:

$$U_t(A_t, w) = \max_{A_{t+1} \geq L} u(A_t - m_t(w) - A_{t+1}/(1+r)) + \frac{1}{1+\delta} J_{t+1}(A_{t+1}) \quad (1.2)$$

Contrary to a standard job search model, unemployment benefits do not enter the unemployment value function, as only a small fraction of unemployed workers actually receives them; this better reflects the reality of the labor market of a developing economy.<sup>22</sup> I assume that  $U$  and  $V$  are concave.<sup>23</sup>

Again, keeping the same notation as in [Card et al. \(2007\)](#), but with the important

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<sup>21</sup>Better jobs take longer to search implies a negative first derivative with respect to wage for the job arrival rate ( $s'_t(w) < 0$ ), and a positive one for search costs ( $m'_t(w) > 0$ ).

<sup>22</sup>According to the 2007 *Labour Force Survey*, less than 1% of unemployed workers, irrespective of whether they were previously employed, receive support from the Unemployment Insurance Fund.

<sup>23</sup>For discussion of the possible convexity of  $U$ , see [Lentz and Tranaes \(2005\)](#).



difference that individuals maximize over  $w$ , one can write  $J_t$ , the expected value at the beginning of period  $t$  without a job and with assets  $A_t$ , as:

$$J_t(A_t) = \max_w H_t(A_t, w) = \max_w s_t(w)V_t(A_t, w) + (1 - s_t(w))U_t(A_t, w) \quad (1.3)$$

from which it is possible to derive the following first-order condition for the optimal target wage,  $w^*$ :

$$s_t(w^*) \frac{\partial V_t}{\partial w^*} + (1 - s_t(w^*)) \frac{\partial U_t}{\partial w^*} + s'_t(w^*)(V_t - U_t) = 0 \quad (1.4)$$

Intuitively, the optimal target wage,  $w^*$ , is the one where the marginal gains from targeting better jobs are equal to the marginal costs of depleting assets during search<sup>24</sup>, plus the cost from longer search ( $s'_t(w^*)$  is negative by assumption).

### 1.3.1.3 Effect of an Exogenous Cash Grant

The main prediction of interest is how the optimal target wage,  $w^*$ , changes with respect to a positive shock in assets, as the result of an exogenous cash transfer. The expression for the effect of a shock in assets on the optimal target wage is:

$$\frac{dw^*}{dA_t} = -\frac{\partial^2 H_t(A_t, w^*)}{\partial w^* \partial A_t} / \frac{\partial^2 H_t(A_t, w^*)}{\partial w^{*2}} \quad (1.5)$$

where the denominator is negative according to the second order condition of the maximization problem. The expression for the numerator is:

$$\frac{\partial^2 H_t(A_t, w^*)}{\partial w^* \partial A_t} = s(w^*) \frac{\partial V_t}{\partial w^* \partial A_t} + (1 - s_t(w^*)) \frac{\partial U_t}{\partial w^* \partial A_t} + s'_t(w^*) \left( \frac{\partial V_t}{\partial A_t} - \frac{\partial U_t}{\partial A_t} \right) \quad (1.6)$$

By applying the envelop theorem and writing  $c_t^e$  for consumption when employed and  $c_t^u$  when unemployed, one can derive the following expressions from Equations

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<sup>24</sup>As shown in Equations 1.1 and 1.2, these costs enter both  $U_t$  and  $V_t$ , so that  $\frac{\partial V_t}{\partial w}$  includes both the gains from a higher wage and the cost of higher search:  $\frac{\partial V_t}{\partial w} = (-m'_t(w) + 1)u'(A_t - m_t(w) - A_{t+1}/(1+r) + w) + \frac{1}{1+\delta} \frac{\partial V_{t+1}}{\partial w}$

1.1 and 1.2:

$$\frac{\partial V_t}{\partial w^* \partial A_t} = (-m'_t(w) + 1)u''(c_t^e) \quad (1.7)$$

and:

$$\frac{\partial U_t}{\partial w^* \partial A_t} = -m'_t(w)u''(c_t^u) \quad (1.8)$$

Plugging 1.7 and 1.8 into Equation 1.6, and rearranging:

$$\begin{aligned} \frac{\partial^2 H_t(A_t, w^*)}{\partial w^* \partial A_t} = & \underbrace{s_t(w^*)u''(c_t^e) - m'_t(w^*) \{s_t(w^*)u''(c_t^e) + (1 - s_t(w^*))u''(c_t^u)\}}_{(1), <0} + \underbrace{s'(w^*) \{u'(c_t^e) - u'(c_t^u)\}}_{(3), >0} \\ & \underbrace{m'_t(w^*)}_{(2), >0} \end{aligned} \quad (1.9)$$

Equation 1.9 puts forward three mechanisms at play. The first term captures that the positive shock in assets reduces the utility gain associated with a better-paying job, which lowers the target wage. On the contrary, the second term points out that a wealth effect reduces the utility loss deriving from depleting assets to pay for the monetary cost of search, which has a positive impact on the target wage. Similarly, the third term implies that a positive shock in assets decreases the utility loss associated with longer search, which again has a positive effect on the target wage. This channel will be at play if individuals cannot perfectly smooth consumption between employment and unemployment. Therefore, the sign of Equation 1.5 is ambiguous, and will be positive if second and third term dominate. The ambiguity of this prediction is intuitive. On the one hand, richer individuals gain less, in relative terms, from getting a better paid job. On the other hand, they also find it less costly in terms of utility to look longer for better jobs, as they have more resources to do so.

Moreover, one can also obtain the expressions for the effect of a positive wealth shock on search duration, and search costs, which are, respectively:

$$\frac{\partial s_t(w^*)}{\partial A_t} = \frac{\partial s_t(w^*)}{\partial w^*} \times \frac{\partial w^*}{\partial A_t} \quad (1.10)$$

$$\frac{\partial m_t(w^*)}{\partial A_t} = \frac{\partial m_t(w^*)}{\partial w^*} \times \frac{\partial w^*}{\partial A_t} \quad (1.11)$$

Therefore, given the sign of Equation 1.9, we can derive two subsequent predictions, which follow mechanically from the set-up of the model. If the effect of the positive shock in assets on the target wage is positive, we should observe lower job finding rates, and therefore, longer unemployment. Furthermore, as it will lead to target better jobs, and better jobs require higher search costs by assumption, this should also be associated with a higher cost of search.

In sum, the model’s prediction of the effect of an exogenous cash transfer is ambiguous. However, we can say that the positive effect on the target wage will be larger if individuals cannot perfectly smooth consumption between the employed and unemployed state, and, more generally, if consumption in unemployment is low. Hence, we can derive from this an indirect (and imperfect) test for the presence of liquidity constraints during job search.<sup>25</sup> If the liquidity effect dominates, we would expect cash transfer recipients to target better jobs, and search for longer. On the other hand, if the disincentive effect dominates, we would observe people searching for shorter periods and settling for lesser-quality jobs.

Anticipating the empirical results, we can examine how the effect should evolve as a function of the size of the positive wealth shock, as the treatment intensity  $dA_t$  varies with the duration of CSG receipt. Intuitively, under some assumptions about the concavity of the utility function, the model is consistent with a non-linearity of the effect of assets on the target wage.<sup>26</sup> The liquidity effect would increase less than proportionally as  $dA_t$  increases. The opposite is true for the negative impact on the target wage: the larger the positive asset shock, the greater the reduction in the utility gain associated with better-paying jobs.

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<sup>25</sup>This test is imperfect because it also depends on the magnitude of the first term of Equation 1.9, therefore whether the disincentive effect to look for better jobs as one gets richer dominates. Also, individuals might want to voluntarily keep consumption low when unemployed as in a buffer stock model (Card, Chetty and Weber (2007)).

<sup>26</sup>More precisely, this would be the case if the third-order derivative of the utility function is positive, as in a *Decreasing Absolute Risk Aversion* (DARA) utility function, such as  $u(c) = \ln(c)$ .

### 1.3.1.4 Alternative Channels

There are channels other than investment in job search that could lead to lasting labor market effects of an unconditional cash transfer. Baird, McKenzie and Özler (2018) mention three other potential channels: 1) a *health productivity* effect, where workers become more productive as a result of better nutrition. This specific channel is less pertinent in a middle-income country like South Africa. We could think, however, that any positive effect on human and/or physical capital could lead to benefits in labor market outcomes that outlast the grant; 2) a *self-employment* liquidity effect: in the presence of capital investments necessary to enter self-employment, a cash transfer could provide the positive income shock necessary to enter this sector; 3) an *insurance* effect: as some occupations and behaviors are more risky than others – such as self-employment and migration, for example – a cash transfer could increase willingness to bear risk (as shown by Bianchi and Bobba (2013)). Moreover, it has been shown that transfers targeted at women can have beneficial effects on their bargaining power within the household (Ambler (2016) in South Africa, Almas et al. (2018) for review). While plausible, I show that none of these channels match the evidence to the extent of job search. I provide a discussion of these alternative mechanisms in Section 6.

## 1.4 Data and Descriptive Statistics

### 1.4.1 Data

This paper combines several data sources to study in detail the labor market impact of the CSG. The main part of the analysis is conducted on Census data (2001 & 2011) and the Community Survey (2007). For simplicity, I refer to both as Census data, as they are highly comparable and have a large sample size.<sup>27</sup> Both also contain questions on the date of birth of the youngest child, which allows to have information

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<sup>27</sup>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. The publicly available Census waves are a 10% subsample of the overall Census in the years 2001 and 2011; the Community Survey is a large survey of roughly 2% of the population in 2007.

on CSG exposure regardless of whether or not the child is observed in the household. 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. Labor market outcomes in the Census are limited to information on informality, occupation, and sector of work. Wage information is lacking, and income is recorded as a categorical variable. In the Appendix, I outline how the informal sector is measured in the Census (non-registered businesses and private households), and how it compares to other surveys. Despite its limitations, Census data is the only source with information on the child’s birth year, which is key to i) compare the same population over time, and to ii) clearly identify which mothers are treated, and has a large enough sample to realistically capture the causal impact of the grant.

For more descriptive purposes, I take advantage of other sources of data: 1) for detailed information CSG receipt over time, and other social grants, I make use of the *General Household Survey* (GHS); 2) for detailed information on wages, and the composition of formal/informal jobs, I employ the South African *Labour Force Survey* (LFS) from 2002 to 2007.<sup>28</sup> Lastly, 3) to further study how the grant impacts job search, I use the *National Income Dynamics Survey* (NIDS). This panel dataset has a very detailed labor market section, a longitudinal dimension, but only covers the time period (2008–2014) and at the cost of a significantly smaller sample size. Importantly, this data source has information on aspects of job search such as transport expenses and reservation wages. In the Appendix, I outline how each outcome is measured in the different data sources.

## 1.4.2 Descriptive Statistics

In South Africa, employment in the informal sector accounts for around 30 percent of total employment, slightly less than 15 percent of working-age population (*Source*: LFS (2002–2007)). The informal sector remains where many employment opportunities are concentrated for Black and Coloured women. Indeed, despite a very

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<sup>28</sup>I exclude the two initial years of the LFS, 2000–2001, because of the problems of comparability in measurement of informal employment as pointed out in [Kerr and Wittenberg \(2015\)](#).

low employment rate, women account for more than half the employment in the informal sector. This often occurs as domestic workers in private households, the most common type of informal work in South Africa. In Figure 1.A.4, we observe that at the beginning of the period, informal sector employment made up almost half of women's total employment, with only marginal improvements over time. In terms of the composition of formal jobs, 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 (Table 1.A.1).<sup>29</sup>

Table 1.A.2 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 workers, but this gap is even larger for women. Similar magnitudes hold for hourly earnings, suggesting that this is not due to the higher prevalence of part-time in the informal sector. 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 indicates how the amount of the CSG cannot be considered small, in particular relative to returns to informal employment for women. However, it also points 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 one CSG grant, and less than 5% of workers in the informal sector do. These orders of magnitude are key to interpret the results that I present in the next section.

## 1.5 Empirical Analysis

### 1.5.1 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 important policy has received in the literature. However, the age

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<sup>29</sup>See Magruder (2012) for an interesting analysis of the employment effects of labor unions in South Africa.

eligibility criteria and its reforms provide a valuable source of variation to evaluate the effects of the CSG. Identification relies on the fact 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 1.2, 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.<sup>30</sup> As I show in Figure 1.A.5, 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. Instead, cohort 1993 is eligible for 30 months overall, and is the first one eligible for a full year. In this setting, what matters is that the large variation across adjacent cohorts is the result of exogenous changes in policy, which I argue is the case.

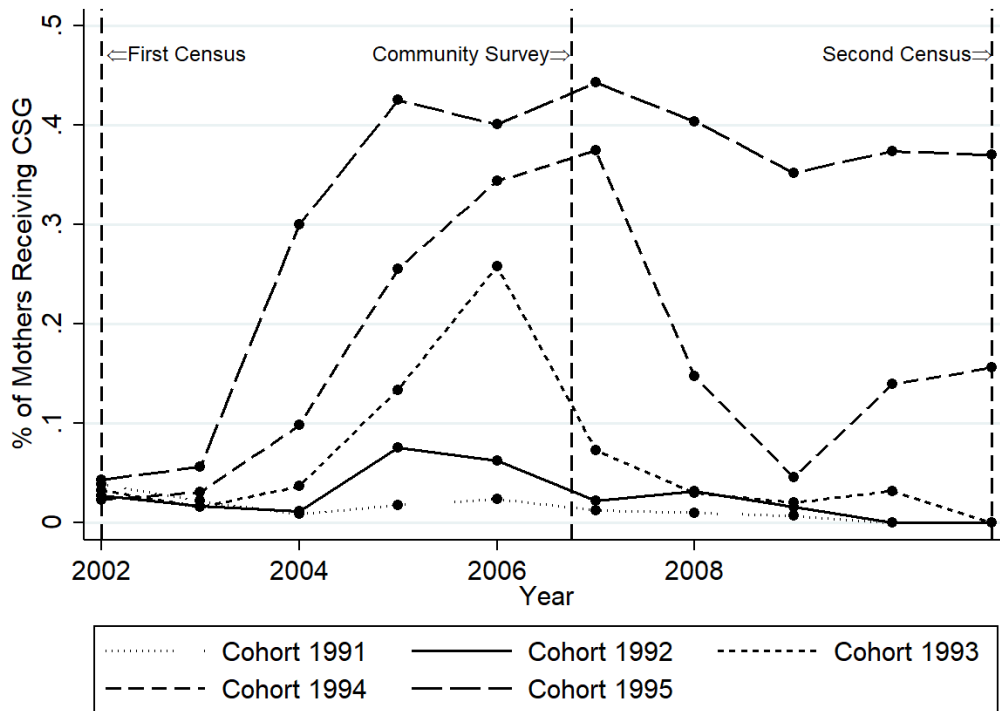
It is key to understand here that these differences in CSG exposure occur, on average, for cohorts in adjacent years, not adjacent months: individuals born one year apart can have large differences in the eligibility and take-up of the CSG, but probably not individuals born one month before or after. 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.

Given this setup, a first logical option would be to perform a “Difference-in-Differences” estimation, comparing the outcomes of mothers of exposed and unexposed cohorts before and after CSG receipt. However, a diff-in-diff with cohorts can

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<sup>30</sup>The GHS does not have a measure of year of birth, but only age. Therefore, information on the year of birth can only be obtained by taking  $year - age$ . Given that the GHS takes place in July, those born in the first half of the year have the correct year of birth,  $year - age = yob$ , while those born in the second half appear as born one year later,  $year - age = yob + 1$ . Therefore, in the GHS, each cohort is composed by those born before July of the correct year, and those born after July of the previous year. This implies that the median month in each cohort is January of the indicated year. So, these graphs should be interpreted as indicating take-up for mothers whose youngest child, on average, is born in January.

Figure 1.2: CSG Take-Up by Cohort of Birth, 2002–2011



*Note:* This graph gives the share of mothers receiving the CSG by year of birth of the youngest child for the period 2002–2011. The graph is representative of mothers whose child is born in January of each year; see footnote in text for details about measurement of the year of birth. Overall take-up of the CSG was very low before 2002 (see Figure 1.1). 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.

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 outlining this issue in Appendix 1.A.2.1.

To solve this problem, I exploit the large, discontinuous spike in take-up observed in Figure 1.2 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 year of birth of the child and the threshold is set at 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 sides of the threshold; hence, the “common trend” assumption



among treated and non-treated cohorts is not required. In this setting, identification relies on the fact that there is a last untreated cohort, 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.<sup>31</sup>

With this estimation, identification relies on the assumption that, had the CSG not been implemented, we should not observe any discontinuity at the threshold with respect to the labor market outcomes of the mother. This can be easily checked by using the three cross-sections at my disposal: one in late 2001, before any CSG; one in early 2007, just after the large spike; and one in 2011, after. By estimating the effect separately in 2007 and 2011, I am able to disentangle the short-term effect of receiving the CSG from its long-term effect five years after. Formally, I estimate the following equation, separately for 2001, 2007, and 2011:

$$Y_i = f(c_i - 1993) + \mathbb{1}\{c_i \geq 1993\} \times f(c_i - 1993) + \beta_1 \mathbb{1}\{c_i \geq 1993\} + X' + \varepsilon_i \quad (1.12)$$

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.  $\beta_1$  should capture in 2007 the short-term effect of the positive CSG shock, and in 2011 the persistent effect of having received the grant five years before.<sup>32</sup>  $X'$  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.<sup>33</sup>

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<sup>31</sup>One could, in theory, exclude some cohorts in order to increase this differential. For example, excluding cohort 1992 or excluding cohort 1993. 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.

<sup>32</sup>Children born in 1993 gradually lose eligibility in 2007, depending on the month of birth. Given that the Community Survey takes place in early 2007, most children are still receiving the transfer.

<sup>33</sup>Lee and Card (2008) initially suggested that in an RD design with a discrete forcing variable, clustering of the standard errors should occur over the discrete values of the running variable, while in a more recent development, Kolesár and Rothe (2018) strongly advise against this practice, in particular when the number of clusters is small. In my estimations, the standard errors always drop significantly when clustering by the running variable. For this reason, I only cluster at the

All estimations are performed on the sample of Black and Coloured mothers. As White mothers do not receive the CSG, this sub-population is well suited for a placebo test. Unless otherwise indicated, the forcing variable is always the youngest child born to a given mother, regardless of whether this child is observed in the household or not. One advantage of this approach, which is possible using fertility information in the Census, is that I do not need to observe a child in the household to know whether or not the mother was treated. This allows to define treatment status much more accurately than if only observing children in the household, which would be particularly problematic for the long-term results when the children at the threshold are already quite old. It also takes away the concern that the grant might lead to changes in household composition ([Ardington, Case and Hosegood \(2009\)](#), [Hamoudi and Thomas \(2014\)](#)), which could introduce selection bias. A disadvantage is that fertility information is only asked to women under 50 years of age, so there is no information for women older than this threshold.<sup>34</sup> Therefore, I run all estimations on the sub-sample of women born between 1960 and 1985, for whom I have both fertility and labor market information in the three Census waves.<sup>35</sup> In this way, the underlying population over time is roughly the same, unless for those mothers who have other children between 2007 and 2011, who drop out of the sample. Given the discreteness of the forcing variable, and the few support points near the threshold, I cannot run a local linear regression using the optimal bandwidth. Instead, I choose the preferred bandwidth according to Akaike Information Criterion (AIC) as the one that maximizes the window for a quadratic fit (in order to avoid the use of higher-order polynomials). Results are robust to both a linear and a quadratic functional form, and to different windows.

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household level and not at child cohort level.

<sup>34</sup>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 can be found later in the robustness checks.

<sup>35</sup>Labor market information is only collected in the Census for individuals older than 15 years.

## **1.5.2 Effects of the CSG on Employment and Sectoral Allocation**

Table 1.2 presents the main results with the preferred specification and window.<sup>36</sup> 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 on the effects in 2007, therefore as the CSG was received by cohort 1993, and in 2011, five years after. Graphical evidence of these effects is presented in Figures 1.A.6 to 1.A.9.

### **1.5.2.1 Short-Term Effects of Receiving the CSG**

The short-term results are presented in the middle panel of Table 1.2. For mothers, we observe a significant and large jump in unemployment at the threshold, matched by a symmetric drop in formal sector employment. The drop in overall employment is around 2 pp., although not statistically significant. Other household members, on average, do not respond to the CSG in terms of labor market outcomes, as shown in Table 1.A.4. The lack of any effect on other adult members may come as surprising, as it contrasts with a unitary household model where the grant is be pooled into household income, but it is in line with the fact that mothers are almost always the direct recipients of the grant.

These results indicate that receiving the CSG increases mothers' unemployment. While in theory, a higher unemployment rate can be the result of a lower flow out of unemployment or a higher flow into unemployment, I argue that it is most likely due to the former. Indeed, the increase is entirely explained by mothers who report not finding work/suitable work as the reason for unemployment, which is (more) consistent with longer search as an explanation.

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<sup>36</sup>This is a quadratic function on the 1981–2004 window, chosen according to the AIC criterion. Results are virtually identical with a linear window on the 1987–1998 window, chosen with the same criterion. See Table 1.A.3.

Table 1.2: 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 (0.0046)	-0.0029 (0.0051)	0.0011 (0.0051)	0.0016 (0.0040)	0.0002 (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 (0.0114)	0.0267** (0.0115)	-0.0190 (0.0133)	0.0122 (0.0110)	-0.0312*** (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 (0.0069)	-0.0053 (0.0056)	0.0027 (0.0071)	-0.0114** (0.0057)	0.0141** (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 1.12 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).

These results imply that those mothers who search more would have been formally employed in the absence of the CSG shock. This may appear counter-intuitive as, in the previous section, I have shown that formal jobs pay significantly more on average than informal jobs. Possibly, the higher flexibility of informal jobs allows mothers to search while also being informally employed.<sup>37</sup> Alternatively, this heterogeneity might reflect that workers in formal and informal jobs are inherently different across other dimensions.

Overall, these results are in line with predictions of a job search model, both in the directed search framework outlined before or in a more classic framework where workers choose effort. Without perfect consumption smoothing, an exogenous cash grant should have a positive effect on the level of unemployment, through longer duration, which is indeed what is observed.<sup>38</sup>

### **1.5.2.2 Persistent Effects of the CSG**

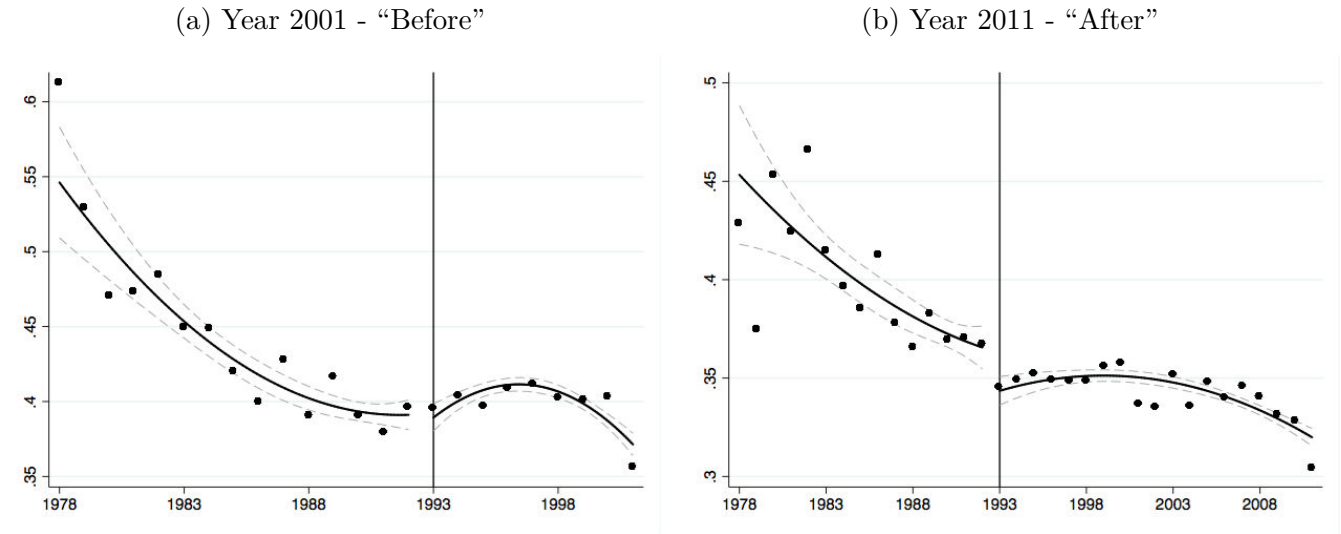
The main question of interest is whether the longer unemployment induced by the grant translates into job quality gains for recipients. The persistent results are presented in the bottom panel of Table 1.2. In 2011, mothers whose child was born in 1993 are as likely to be employed as mothers who did not have any access to the CSG. Any effect on total 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. A clear indication of the effect on the composition of employment appears already when looking at the share of informal employment (Figure 1.3). 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

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<sup>37</sup>I do not have information on working hours or on-the-job search, so I cannot test this hypothesis directly.

<sup>38</sup>This prediction is unambiguous in Card, Chetty and Weber (2007) in a job search model with assets where workers choose effort. In the model proposed in Section 3, this prediction is ambiguous, but is more likely to hold as liquidity constraints become more severe.

Figure 1.3: Share of Informal Employment by Birth Cohort of Youngest Child, 2001 & 2011



*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. A quadratic function is fitted on each side of the threshold with 95% confidence intervals. Mothers of children born as from 1993 have been eligible for at least one year to the CSG in-between the two Census waves; see text for explanation.

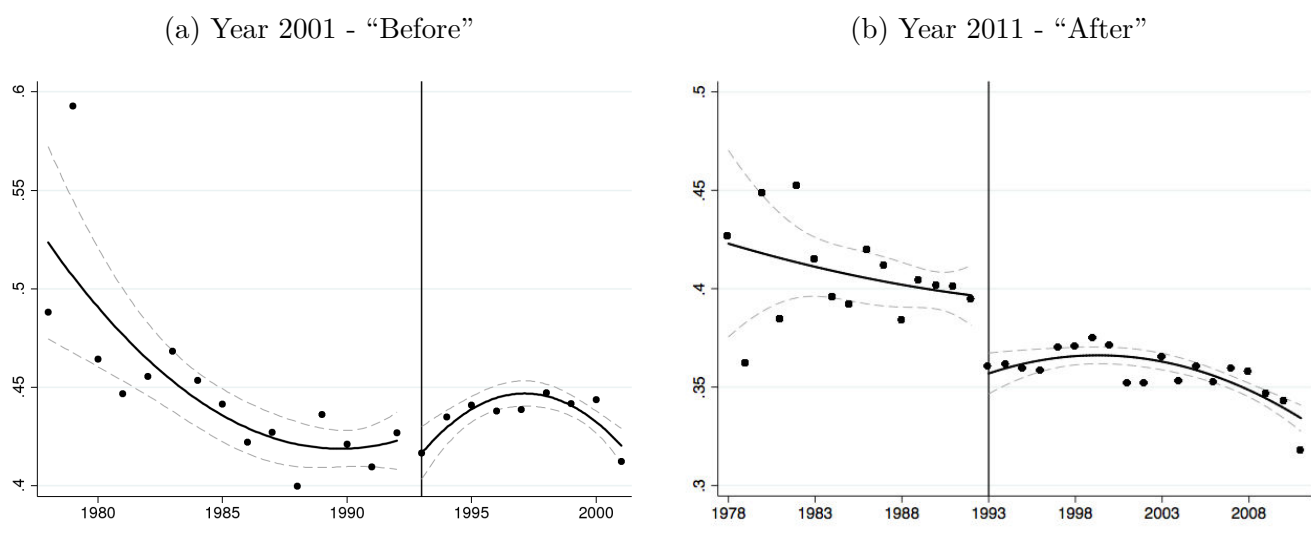
*Source:* Census (2001 & 2011).

the share of informal employment is significantly more marked for single mothers, as can be seen in Figure 1.4. For this subgroup, the decrease in informality is almost 10% at the threshold. This heterogeneity is not surprising. Single mothers are likely to be the most liquidity constrained and unable to stay unemployed, given that they cannot count on any income support from a spouse. Again, effects are concentrated on the direct recipients. Consistently with the previous results, there is no evidence of persistent effects on other household members.<sup>39</sup>

There are two main take-aways from these persistent results of the grant. The “glass half empty” 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 is not sufficient to overcome fixed cost to access employment, or that liquidity constraints are not so severe as to prevent entry into employment at the extensive margin, or that other barriers are binding. As overall employment for Black and Coloured women is

<sup>39</sup>As shown in Table 1.A.4, the issue with estimating effects on other household members is that this population, which is defined as the one sharing the household with a treated mother, changes significantly over time. This complicates the interpretation of the results on other household members in a dynamic way, but the absence of significant effects in any period suggests they are not affected.

Figure 1.4: Share of Informal Employment by Birth Cohort of Youngest Child, Single Mothers Only, 2001 & 2011



*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. Mothers of children born as from 1993 have been eligible for at least one year to the CSG in-between the two Census waves; see text for explanation.

*Source:* Census (2001 & 2011).

very low, one could argue that increasing participation at the extensive margin is the first order concern, but this largely depends on whether unemployment/inactivity is voluntary or not. On the contrary, the “glass half full” is that the grant appears to lead to persistent and sizable job quality improvements for employed mothers, as shown by the higher rate of formal sector employment among treated mothers. The fact that we observe these differences 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. The stronger effect among single mothers also suggests that these benefits might be concentrated on those who are more vulnerable.

In order to test to what extent the grant has led to an actual change in the types of jobs that mothers hold, in Tables 1.A.6 and 1.A.5, I decompose the effect by employer, occupational status, and sector. The long-term increase in formal wage employment comes in equal parts from informal wage- and self-employment. Contrary to Bianchi and Bobba (2013), I do not find any evidence that access to a cash transfer increases self-employment in South Africa. This is an indication that credit/liquidity constraints might not be the main reason behind the stunningly low rate

of self-employment in South Africa. Table 1.A.5 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 rest of the effect occurs instead within occupation. To summarize: the CSG makes mothers less likely to be employed by private households and more likely 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.

### 1.5.2.3 Heterogeneous Effects by Population Group

Table 1.3: Labor Market Effects of the CSG by Population Group, 2007 and 2011

	<i>Black Mothers</i>			<i>Coloured Mothers</i>		
	Unemployed in 2007 (1)	Informal in 2011 (2)	Formal in 2011 (3)	Unemployed in 2007 (4)	Informal in 2011 (5)	Formal in 2011 (6)
CSG	0.0224* (0.0128)	-0.0080 (0.0062)	0.0108 (0.0066)	0.0573** (0.0243)	-0.0287** (0.0142)	0.0306* (0.0184)
Observations	78,343	215,765	215,765	11,741	31,267	31,267
R-squared	0.0787	0.0407	0.1856	0.0923	0.0394	0.1432

*Note:* \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . This table gives the estimates of Equation 1.12 on mothers' labor market status, by population group. In the left panel, the sample is limited to Black mothers, and in the right panel, to Coloured mothers. The dependent variable in Columns (1) & (4) is the probability of being unemployed in 2007; in Columns (2) & (5), the probability of being informally employed in 2011; and in Columns (3) and (6), of being formally employed in 2011. 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. All estimations include controls for: age (cubic), education, marital status, municipality, and household size. Standard errors are clustered at the household level.

*Source:* Author's calculations on Community Survey (2007) and Census (2011).

One may wonder to what extent the 2007 and 2011 effects can be interpreted as linked, sequential responses. In Table 1.3, I explore heterogeneous effects by population groups, dividing between Black and Coloured mothers. The advantage of focusing on this heterogeneity is that this categorization, by definition, is not time-changing, as opposed to other observables. 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. Overall, it confirms the effect dynamics presented before: for both groups, we observe a large increase in unemployment; in 2011, 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, a small subgroup of the population, around 5% overall, with significantly better labor market outcomes on average. Indeed, the very large increase in unemployment for Coloureds in 2007 leads to a long-term effect three times larger than the one for the Black population. What these results show is 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.

#### 1.5.2.4 Heterogeneous Effects by Years of Eligibility

Having established the main results, I relax the identification strategy to exploit the large variation across treated cohorts in the length of their eligibility. 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 test the importance of treatment intensity. This allows me to check whether treatment effects vary with the length of exposure to the CSG. To do so, I now estimate the following equation:

$$Y_{it} = \alpha_0 + \alpha_1 \mathbb{1}\{c_i \geq c\} + \sum_{t=1}^2 \delta_t \mathbb{1}\{year = t\} + \mathbb{1}\{c_i \geq c\} \times \sum_{t=1}^2 \beta_t \mathbb{1}\{year = t\} + X' + \varepsilon_{it} \quad (1.13)$$

where  $\mathbb{1}\{c_i \geq c\}$  is a binary variable equal to one if the youngest child is born in or above a certain threshold value,  $c$ . I then interact this variable with the year to get the evolution of the relative difference over time. The list of covariates is the same as in the previous estimations. Intuitively, what Equation 1.13 does is to compare the relative evolution of mothers of treated and untreated cohorts over the three time

periods, or of cohorts treated for longer or shorter periods.

The results of this estimation, for different cohort windows, are presented in Table 1.A.7. For the smallest possible window, comparing the evolution of mothers with a youngest child born in 1992 vs. 1993 ( $c = 1993$ ), the results replicate for the most part those presented before. Treated mothers are more likely to be unemployed in 2007. 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 is a slightly “stronger” result, which would indicate that one year of CSG had a positive effect on the employment rate. However, for the reasons mentioned before, an RDD-like estimator is more trustworthy in this setting.<sup>40</sup>

Relaxing the constraints of the RDD estimation allows to further explore how mothers respond to longer exposure to the grant. I compare the relative evolution of cohort 1993 vs. cohort 1994 ( $c = 1994$ ). These cohorts experience dramatically different exposures. While those mothers whose child is born in 1993 can receive the grant for 30 months in total on average, those whose child is born just a year after experience on average 60 months of eligibility, twice as long (Figure 1.A.5). Despite this variation in treatment intensity, which also leads to 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 1.A.10: these findings hold true when comparing cohorts further away from the threshold, who experience even longer treatment duration. 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.

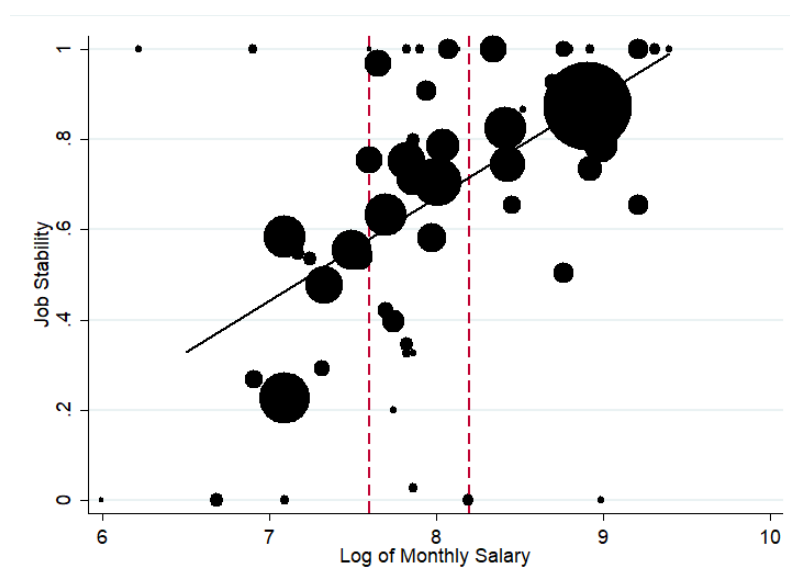
Another important take-away concerns the size of the treated population. In Figure 1.2, we see that longer eligibility also leads to greater take-up: a larger share of mothers is treated in later cohorts. The fact that the effects are not larger in later cohorts suggests that those “extra” treated mothers did not benefit from the grant

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<sup>40</sup>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 Figure 1.A.6, the child cohort function is much more “steep” in 2001 than 2011, because children are younger, and age effects are stronger.

in terms of labor market outcomes. In other words, this would indicate that those mothers who self-select into take-up with only one year of eligibility are also the ones with the higher potential gains from receiving the transfer. This conclusion is important for the interpretation of the effect as an average treatment effect (ATE). Although take-up is only 0.2–0.3 for the first treated cohort 1993, if the grant was received by the full population of mothers, the overall positive effect on job quality would likely remain of the same magnitude as those indicated in Table 1.2.

Figure 1.5: Job Quality Dimensions of Formal Jobs by Occupation and Industry, NIDS Data



*Note:* This graph plots retention rates over a two-year period over the log of median wages by occupation and industry cells, in the formal sector only, with a linear fit. Bigger dots indicate cells where a larger share of formal employment is concentrated (weights come from the 2007 Community Survey). The dashed, vertical lines indicate the thresholds used to identify low-, middle-, and high-quality formal sector jobs.

*Source:* Author's Calculations on NIDS data (2008–2014).

### 1.5.2.5 Effect on Type of Occupation Targeted

As shown in Section 3, in a directed search framework, a positive shock in assets may lead job seekers to target better-quality jobs. It is key to understand whether the targeting of better jobs lies behind to the long-term increase in job quality. To address this question, I exploit the rich information on industry and occupation available in the Census data, where employed respondents are asked their occupation

and sector of work.<sup>41</sup> To construct a measure of job quality by occupation and industry, I exploit wage information in the NIDS, and its longitudinal dimension. It is a well-established fact that job stability in South Africa is very low.<sup>42</sup> As a proxy, I use retention rates in formal jobs over a two-year period (the time span between two waves in the NIDS), calculated for the relevant sub-population of Black and Coloured mothers. I plot the correlation between the two main dimensions of job quality, wages and job stability, in Figure 1.5. Even within the formal sector, job quality varies widely across occupation and industries. These two measures are strongly positively correlated (between 0.7–0.8, depending on the year used to weigh employment in each sector), i.e., more stable occupations and industries also tend to have better wages. I arbitrarily split formal sector jobs in low, mid, and high quality, depending on the ranking of the occupation×industry cell in terms of wages. Given the strong relation with job stability, and presumably other job quality dimensions, this is meant to capture overall job quality rather than just wages. The split is done in order to have roughly equal thirds for the 2007 sample. Qualitatively similar results hold if formal sector jobs are split according to their retention rates, rather than wages.

Table 1.4 reproduces Table 1.2, splitting formal sector jobs by job quality. In 2007, mothers who receive the CSG are less likely to be in low-quality, formal sector jobs, meaning those with low retention rates and low wages. On the contrary, the persistent effect comes from higher employment in mid-quality, formal sector industries and occupations. The jobs that mothers do not take when receiving the grant are not the same jobs they have five years after. This is consistent with a different targeting as a result of the grant, where a cash transfer leads to looking for better jobs. The persistent decrease in informality suggests that, in the absence of the grant, some of these mothers would have ended up in the informal sector, which is not surprising, given the very low retention rates of low-quality formal sector jobs.

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<sup>41</sup>There are 10 occupations and 10 sectors, for 100 combinations. However, some of these are empty cells, or with too few people to be statistically relevant.

<sup>42</sup>The South African labor market is characterized by large worker flows and by high churning, meaning worker flows that are not justified by job reallocation (Banerjee et al. (2008)). This stylized fact holds true even when focusing only on formal jobs, as Kerr (2018) confirms using administrative data on the universe of formal jobs in South Africa.

Table 1.4: CSG & Job Quality, 2007 & 2011

	Year 2007 - “During”					
	(1)	(2)	(3)	(4)	(5)	(6)
	Unemployed	Informal	Formal	Formal Low Quality	Formal Mid-Quality	Formal High Quality
CSG	0.0242* (0.0124)	0.0145 (0.0113)	-0.0275** (0.0120)	-0.0184** (0.0079)	-0.0077 (0.0084)	-0.0014 (0.0083)
Mean Y at Threshold	0.2469	0.1908	0.2975	0.0826	0.0963	0.1185
Observations	82,634	82,634	82,634	82,634	82,634	82,634
R-squared	0.0893	0.0613	0.2018	0.0227	0.0501	0.2293
	Year 2011 - “After”					
	(1)	(2)	(3)	(4)	(5)	(6)
	Unemployed	Informal	Formal	Formal Low Quality	Formal Mid-Quality	Formal High Quality
CSG	-0.0054 (0.0056)	-0.0115** (0.0057)	0.0142** (0.0063)	-0.0006 (0.0037)	0.0116** (0.0051)	0.0032 (0.0041)
Mean Y at Threshold	0.1661	0.1820	0.3028	0.0661	0.1468	0.0897
Observations	246,522	246,522	246,522	246,522	246,522	246,522
R-squared	0.0421	0.0374	0.1831	0.0103	0.0744	0.1190

*Note:* \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . This table gives the OLS estimates of Equation 1.12 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). Low-, mid-, and high-quality formal sector jobs are defined according to their level of log-wages, as shown in Figure 1.5. The table is constructed such that the coefficients of Columns (4), (5), and (6) add up to Column (3). 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).

### 1.5.3 Persistent Effect on Income

Arguably, the most important dimension of job quality is the wage. Wages in the formal sector are significantly higher than in the informal sector. This is particularly true for Black and Coloured women in South Africa, who make three times more on average when working a formal job. Census data do not have a direct measure of wages. Only information on overall income, irrespective of the source and recorded in brackets, is collected.

If everybody was paid the median wage in each sector, I calculate that the observable wage increase associated with the increase in job quality should be in the order of magnitude of R 30 (6\$) over the total population, and around R 50 (11\$) for those employed.<sup>43</sup> In absolute terms, these numbers appear quite small. However, these “predicted” wage effects are sizable when compared to the amount of the grant (around 12% of the monthly value of one grant). Furthermore, as the take-up differential between the first and last unexposed cohort is relatively small (0.2–0.3), effects on the treated are potentially large.

Table 1.5: CSG & Income, 2011

	(1) <b>Predicted</b> $\Delta$ Wage	(2) Income	(3) <b>Predicted</b> $\Delta$ Wage	(4) Income
CSG	30.8480	16.9560 (53.3776)	52.5816	30.5555 (98.2905)
Sample Observations	All	All 238,322	Employed	Employed 107,782

*Note:* \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . This table gives the estimates of Equation 1.12 on mothers’ income in 2011. Columns (2) & (4) are estimated with a censored regression to account for the interval-coded nature of the data. Only Black and Coloured mothers born between 1960 and 1985 are included, and only those employed in Columns (3) & (4). *Predicted  $\Delta$  Wage* is predicted, observable change in wage given the median wage by formal and informal sector, calculated for the relevant sub-population in the NIDS. *Income* is a categorical variable for monthly income as asked in the Census questionnaire.

*Source:* Author’s calculations on Census 2011; wage data is from the NIDS.

Assuming the CSG has not affected other income generating activities, we may expect an effect of similar magnitude on income. In the absence of other adjustments, the effect on income should be a noisier estimate of the effect on wages. Indeed, the estimated coefficient on income is positive, and in line with the magnitudes presented before, but far from statistical significance. The interval-coded nature of the income data in the Census, with fairly large brackets, and the lack of precise wage data, is behind the low statistical power. Given these data constraints, the minimum detectable effect on monthly income would be in the order of 23\$ (or 43\$

<sup>43</sup>Dollar amounts are always in 2010 PPP. *Source:* OECD.stat

for the employed only). This would require the average wage gains as a result of the CSG to be significantly larger than the overall gap in median wages between the formal and informal sector, which seems unrealistic. With this in mind, the lack of a significant effect on income should not be surprising, and it is not necessarily an indication that there are no wage gains as a result of receiving the grant.

#### **1.5.4 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 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 the log density of cohorts around the threshold in the spirit of [McCrary \(2008\)](#). Graphical evidence, presented in [Figure 1.A.14](#), shows that no discontinuity exists at the threshold, neither before, 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 1.A.11 to 1.A.13](#) in the Appendix show 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, and have migrated, as well as 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: observable characteristics are comparable at the threshold for the sub-population that 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”.

Results are robust to changing 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 checks are presented in Figures 1.A.15 and 1.A.16. Moreover, I perform a placebo test, exploiting the fact that CSG take-up for Whites is virtually zero.<sup>44</sup> I estimate Equation 1.12 on White mothers only and check that their labor market outcomes are not significantly affected by the CSG eligibility (Figure 1.A.17).<sup>45</sup>

One concern could be that there are discontinuities in mothers’ employment (formal and/or informal) that are due to the age of the child. In Figure 1.A.19, I show the share of informal employment around a threshold set for cohort 1983 in 2001 (hence, exactly the same age as cohort 1993 in 2011)<sup>46</sup>. 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 the size of the jumps estimated at the “correct” threshold relative to other, “misplaced” thresholds. I present these in Figure 1.A.18 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.<sup>47</sup> This supports both the internal validity of the estimation, and the statistical significance of the results: the estimated coefficients with the correct threshold in the correct year are generally at the extremes of the distributions relative to all other years  $\times$  threshold

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<sup>44</sup>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.

<sup>45</sup>The validity of this placebo check is limited, as the smaller size of the White population would not allow in any case to significantly capture effects of the magnitude presented.

<sup>46</sup>Census 2001 and 2011 take place in the same month, October.

<sup>47</sup>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.



possible combinations.

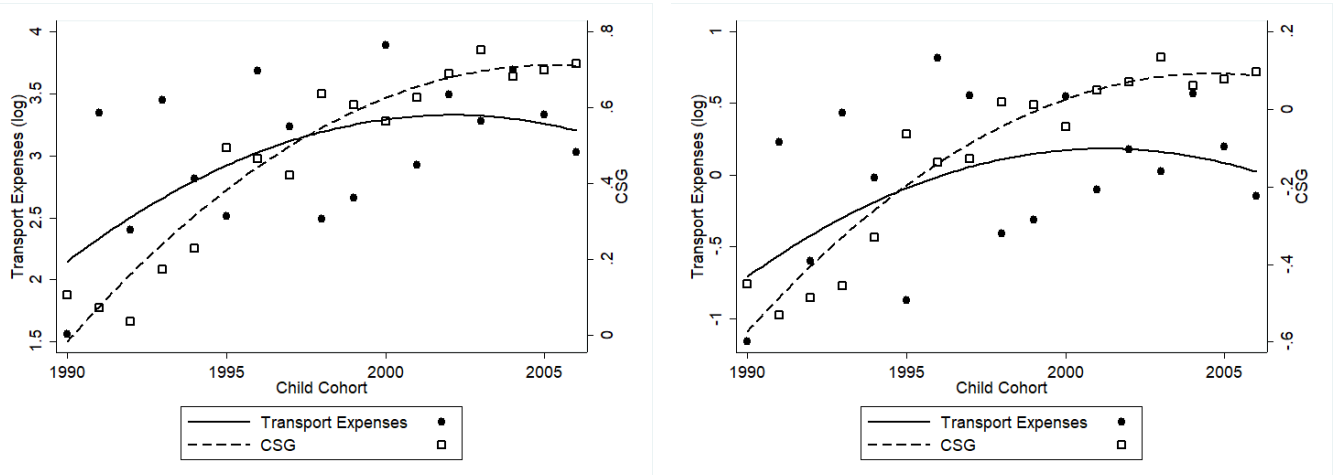
## 1.6 Mechanisms

This paper provides causal evidence that an unconditional cash transfer program can have lasting, positive effects on job quality for women in a strongly segmented labor market. We also observe that these improvements come from longer unemployment as a result of receiving the transfer. In the directed search framework presented in Section 3, I show that mothers might respond to this positive shock in assets by becoming more selective, and targeting better jobs. The NIDS data allows me to test, more in detail, the various mechanisms at play.

Figure 1.6: Correlation between Weekly Transport Cost to Look for a Job and CSG

a) Without Controls

b) With Controls



*Note:* These graphs plot the correlation between reported transport cost when looking for a job in the past week (in logs + 1), and CSG take-up by cohort of birth of the youngest child, both with a quadratic fit. The left panel plots the unconditional value, while the right panel plots the residuals controlling for a set of observable characteristics listed in Table 1.2. The sample is limited to unemployed mothers.

*Source:* NIDS (2010–2014).

Self-reported reservation wages, which are available in NIDS data, do not appear to be correlated with eligibility to the *Child Support Grant*. As shown in Figure 1.A.20, reservation wages are flat with respect to the year of birth of the youngest child, while CSG receipt is a strongly positive function. Between 2010 and 2014, unemployed mothers who receive the CSG report roughly the same reservation wages as those who do not. Target wages and reservation wages are not necessarily related,

and generally, there are concerns about the measurement of reservation wages, which are a “theoretical construct” (see [Nekoei and Weber \(2017\)](#) for discussion). NIDS data has no information on target wages. However, an indication that the grant changes how mothers search is the strong correlation with transport expenses when looking for a job. As can be observed in [Figure 1.6](#), over the same period, mothers whose child is eligible for the grant tend to spend significantly more on transport when unemployed. This correlation, which is robust to the introduction of several controls, suggests that CSG and non-CSG mothers search differently. This matches the prediction that targeting better jobs should be associated with higher transport expenses during job search. High search costs are a well-known issue in the labor market of developing countries; this is particularly true in South Africa, where, for historical reasons pertaining to Apartheid, people are often located far away from jobs and pay significant fees reaching them ([Kerr \(2017\)](#)). Importantly, the results of this paper are qualitatively similar to those of experimental studies on transport subsidies in other African labor markets ([Franklin \(2017\)](#), [Abebe et al. \(2017\)](#)), which find persistent positive effects on job quality for this type of intervention.<sup>48</sup>

As mentioned in Section 3, other mechanisms may explain labor market effects of cash transfers on adult recipients ([Baird, McKenzie and Özler \(2018\)](#)). A cash transfer could increase willingness to bear risk by providing insurance, and push workers towards self-employment or riskier behaviors, such as migration. I can easily exclude the self-employment channel: the grant does not make mothers, or other household members, more likely to be self-employed. Self-employment in South Africa is very low, and barriers other than liquidity constraints or risk are likely to be binding. Contrary to [Ardington, Case and Hosegood \(2009\)](#), who study how households respond to receipt of the *Old Age pension*, I do not find any evidence that the grant induces labor migration. Mothers who have received the transfer are as likely to have migrated as those who have never been eligible to the CSG. This

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<sup>48</sup>However, it should be underlined that, although they might play on the same channel, transport subsidies are conceptually very different from an unconditional cash transfer. A transport subsidy lowers the marginal cost of search and also provides an income effect, while an unconditional cash transfer only provides an income effect, and changes job search only in the presence of liquidity constraints.

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.

It is harder to tackle the “competitiveness” argument, i.e. whether the grant has changed these mothers or their environment in a way that could explain these long-term gains in job quality. In terms of what can be observed in the data, this does not seem to be the case. For example, the predicted share of informal employment, based on all of the mother, child, and household’s characteristics, is smooth around the threshold, suggesting that treated and non-treated mothers should be comparable in terms of their labor market outcomes and in terms of their observables. An advantage of the specific CSG shock used is that it is received when the child is quite old, so it is less likely that the grant has a direct effect on the child that would explain these results. Similarly, while it has been shown that cash transfers can have positive effects on women’s empowerment and bargaining power, this is hard to reconcile with the evidence that the effect is significantly stronger on single mothers.

## **1.7 Conclusion**

This paper has studied how an unconditional cash transfer, the South African *Child Support Grant*, impacts labor market outcomes for women, both in the short and long term. By exploiting eligibility across birth years of children, I find that mothers who are eligible for at least one year are more likely to look for a job when receiving the grant, and less likely to work in low-quality formal sector jobs. 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 better formal sector jobs, and less likely to be informal. This persistent effect on job quality is particularly large among single mothers, who seem to benefit the most in terms of gaining access to better employment. Similarly, the persistent effect on overall income is positive, in line with the magnitude of the job quality gains, but far from statistical significance.

This paper contributes to the growing empirical literature on search frictions in developing countries, showing that unconditional cash transfers can be a way to overcome them. The results are theoretically consistent with the key insights

from the job search literature, and with recent empirical evidence on the effects of transport subsidies. A cash transfer increases unemployment length and the quality of the subsequent jobs. The mechanism I put forward is that mothers search longer and for better jobs, increasing the chance of finding a more stable formal job and reducing the chance of falling into informality. In line with this mechanism, I also observe that those mothers who receive the grant tend to spend more on transport to look for a job. The positive effect on job quality is strongly non-linear in the number of years a mother is eligible to the grant, which reinforces the liquidity explanation. On the contrary, I do not find that the grant increases the long-term employment rate, which suggests that a cash transfer of this magnitude does not help to overcome barriers to entry into employment at the extensive margin, or that these barriers are not related to liquidity constraints and search frictions.

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 where self-employment is rare. Those who are slightly richer because of the transfer are more likely to end up in the formal sector, which indirectly reflects a preference for these types of jobs. One unique feature are the characteristics of the transfer: its complete lack of conditionalities and eligibility conditions other than age of the child. This allows a clean interpretation of its income effect, avoiding potentially confounding factors on behavior. A transfer as universal as the CSG is rare in developing countries, as reflected by its large coverage. For these reasons, these results on the short and long-term effects of an unconditional grant are more generally informative for the debate around transfers “no strings attached”.

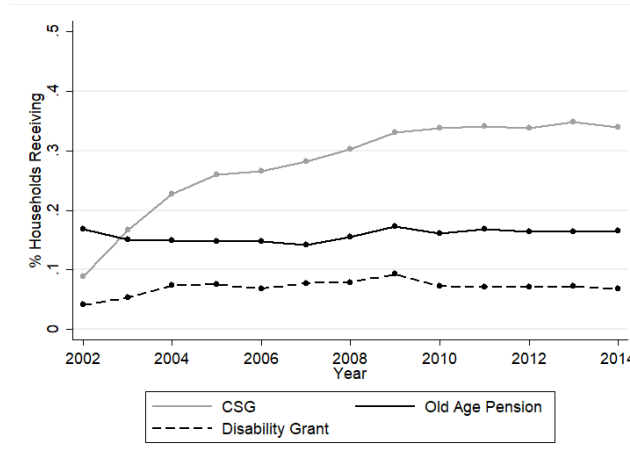
Lastly, an important question that remains unanswered is to what extent these results would carry through at the economy-wide level. Simple descriptive analysis shows that labor market outcomes for women have not improved significantly, despite dramatic growth in CSG coverage in the early 2000s. Naively, 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.

## 1.A Appendix

### 1.A.1 Descriptive Statistics

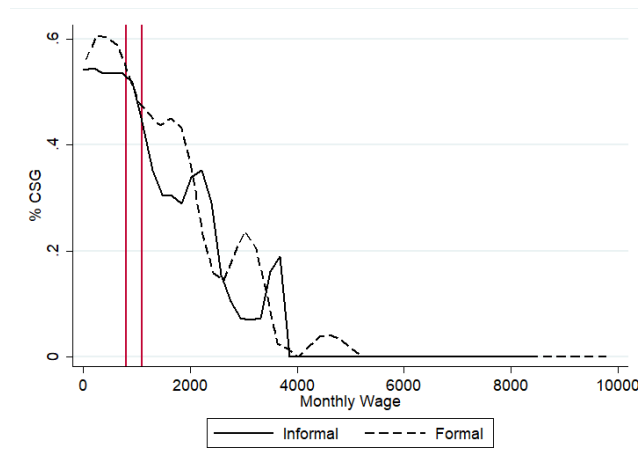
Figure 1.A.1: Share of Households Receiving Social Grants in South Africa, 2002–2014



*Note:* This graph draws the evolution of the three main social grants in South Africa over the period 2002 to 2014. Coverage refers to the percentage of households with at least one member receiving the grant.

*Source:* Author's calculations on GHS.

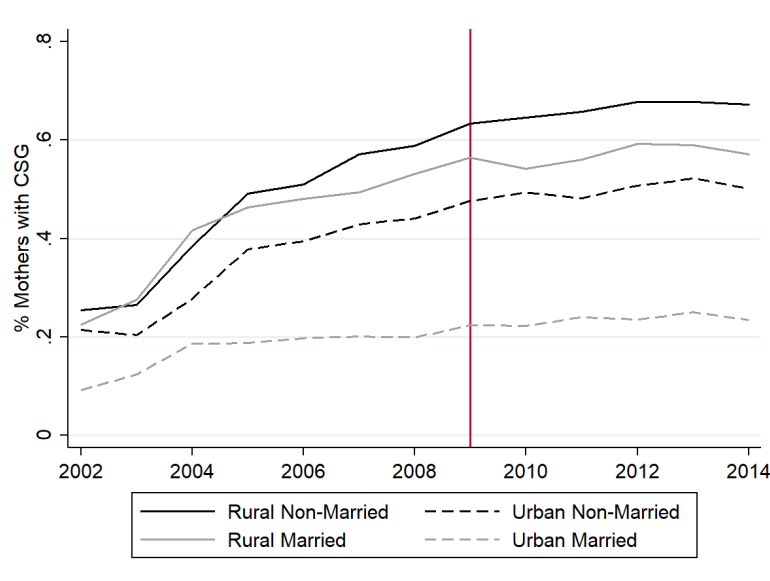
Figure 1.A.2: Take-Up by Wage in the Formal and Informal Sectors, Mothers, NIDS 2008



*Note:* This graph plots take-up of the CSG for mothers in formal and informal jobs in early 2008, before the reform of the means-test. The vertical lines indicates the means-test in urban areas (R 800), and in rural areas (R 1100).

*Source:* NIDS (Wave 1 - Interviews Before July).

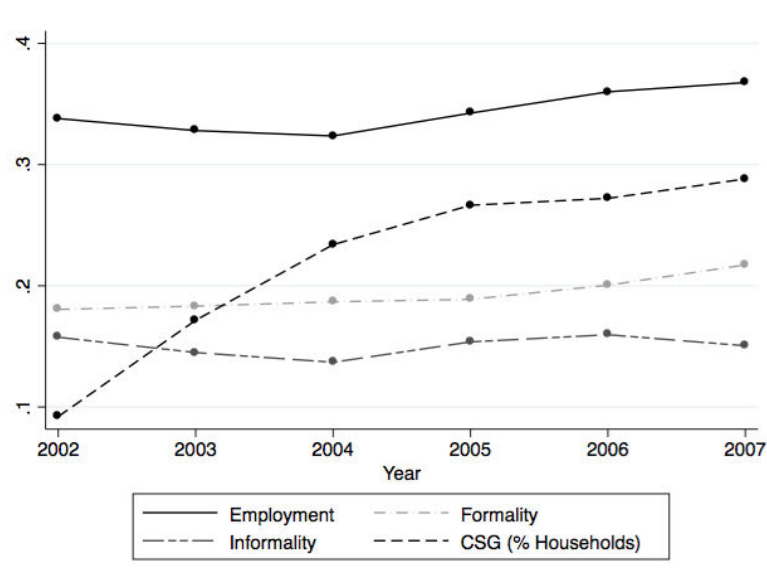
Figure 1.A.3: Share of Mothers Receiving the CSG by Urban/Rural and Married/Non-Married, GHS, 2002–2014



Note: This graph plots the % of mothers receiving at least one *Child Support Grant* for the four sub-groups that experience different increase in the nominal means-test. See text for explanation. The vertical line indicates the year after the means-test reform.

Source: Author's Calculations on GHS.

Figure 1.A.4: 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.

Table 1.A.1: Characteristics by Labor Market Status, 2002–2007

<i>Characteristics</i> (pop %)	Informal	Formal	Inactive	Unemployed
<i>Socio-Demographics</i>				
Black (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
<i>Job Characteristics</i>				
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 percent of informal workers are Black, while 59.96 percent of formal workers are Black. *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 an 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).



Table 1.A.2: Median Wages by Sector in South Africa, 2010 Rand

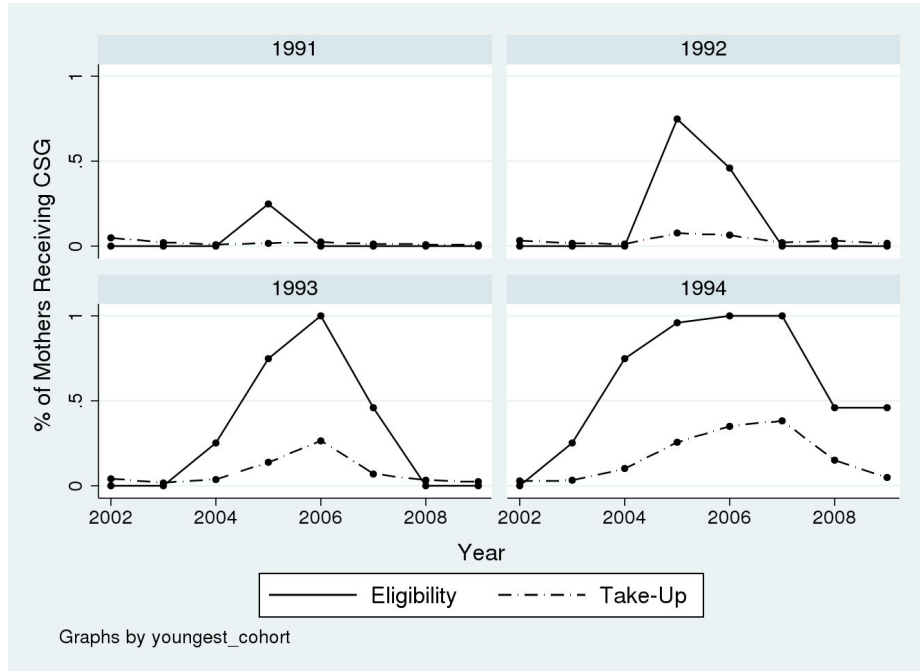
Median Monthly (Hourly) Wages	Formal Sector	Informal Sector	Gap
All	2660 R (13,3 R/h)	875 R (5,1 R/h)	1785 R (8,2 R/h)
Employees	2608 R (13,1 R/h)	875 R (5,0 R/h)	1733 R (8,1 R/h)
Self-Employed	6821 R (32,1 R/h)	893 R (5,2 R/h)	5928 R (26,9 R/h)
<i>Employers</i>	7823 R (38,8 R/h)	1412 R (7,9 R/h)	6411 R (30,9 R/h)
<i>Own-Account</i>	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).

## 1.A.2 Empirical Analysis

Figure 1.A.5: CSG Eligibility and Take-Up by Cohort of Birth, 2002–2011



*Note:* This graph gives the average yearly eligibility and the share of mothers receiving the CSG by year of birth of the youngest child for those born in 1991, 1992, 1993, or 1994 for the period 2002–2011. Take-up is representative of mothers whose child is born in January of each year; see footnote in text for details about measurement of the year of birth. Overall take-up of the CSG was very low before 2002 (see Figure 1.1). Eligibility is calculated using the age threshold information in Table 1.1 and is averaged over the full year. A value of 1 indicates that every child born in that year is eligible for the full year.

*Source:* Author’s calculations on GHS.

### 1.A.2.1 Bias in a 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 cohort with those whose youngest child was in an untreated cohort. If we take the smallest possible window, cohort 1992, the last unexposed cohort, and cohort 1993, the first exposed cohort, over a 10-year period, the DiD estimator equals:

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

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:

$$\begin{aligned} & [\mathbb{E}(y_0|c = 1993, year = 2011) - \mathbb{E}(y_0|c = 1993, year = 2001)] \\ &= [\mathbb{E}(y_0|c = 1992, year = 2011) - \mathbb{E}(y_0|c = 1992, year = 2001)] \end{aligned}$$

which means that the time evolution of labor market outcomes 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:

$$\begin{aligned} & [\mathbb{E}(y_0|age = 18, year = 2011) - \mathbb{E}(y_0|age = 8, year = 2001)] \\ &= [\mathbb{E}(y_0|age = 19, year = 2011) - \mathbb{E}(y_0|age = 9, year = 2001)] \end{aligned}$$

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:

$$\begin{aligned} & \sum_{j=0}^{10} [\mathbb{E}(y_0|age = 8 + j + 1, year = 2001 + j + 1) - \mathbb{E}(y_0|age = 8 + j, year = 2001 + j)] \\ &= \sum_{j=0}^{10} [\mathbb{E}(y_0|age = 9 + j + 1, year = 2001 + j + 1) - \mathbb{E}(y_0|age = 9 + j, year = 2001 + j)] \end{aligned}$$

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:

$$\begin{aligned}
 & \sum_{j=1}^{10} [\mathbb{E}(y_0 | \text{age} = 8 + j + 1, \text{year} = 2001 + j + 1) - \mathbb{E}(y_0 | \text{age} = 8 + j, \text{year} = 2001 + j)] \\
 & \quad + [\mathbb{E}(y_0 | \text{age} = 9, \text{year} = 2002) - \mathbb{E}(y_0 | \text{age} = 8, \text{year} = 2001)] \\
 = & \sum_{j=0}^9 [\mathbb{E}(y_0 | \text{age} = 9 + j + 1, \text{year} = 2001 + j + 1) - \mathbb{E}(y_0 | \text{age} = 9 + j, \text{year} = 2001 + j)] \\
 & \quad + [\mathbb{E}(y_0 | \text{age} = 19, \text{year} = 2011) - \mathbb{E}(y_0 | \text{age} = 18, \text{year} = 2010)]
 \end{aligned}$$

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  $\beta$  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  $\Delta_{18-19}$  denotes the age effect from 18 to 19 years, and  $\Delta_{8-9}$  denotes the age effect from 8 to 9 years.

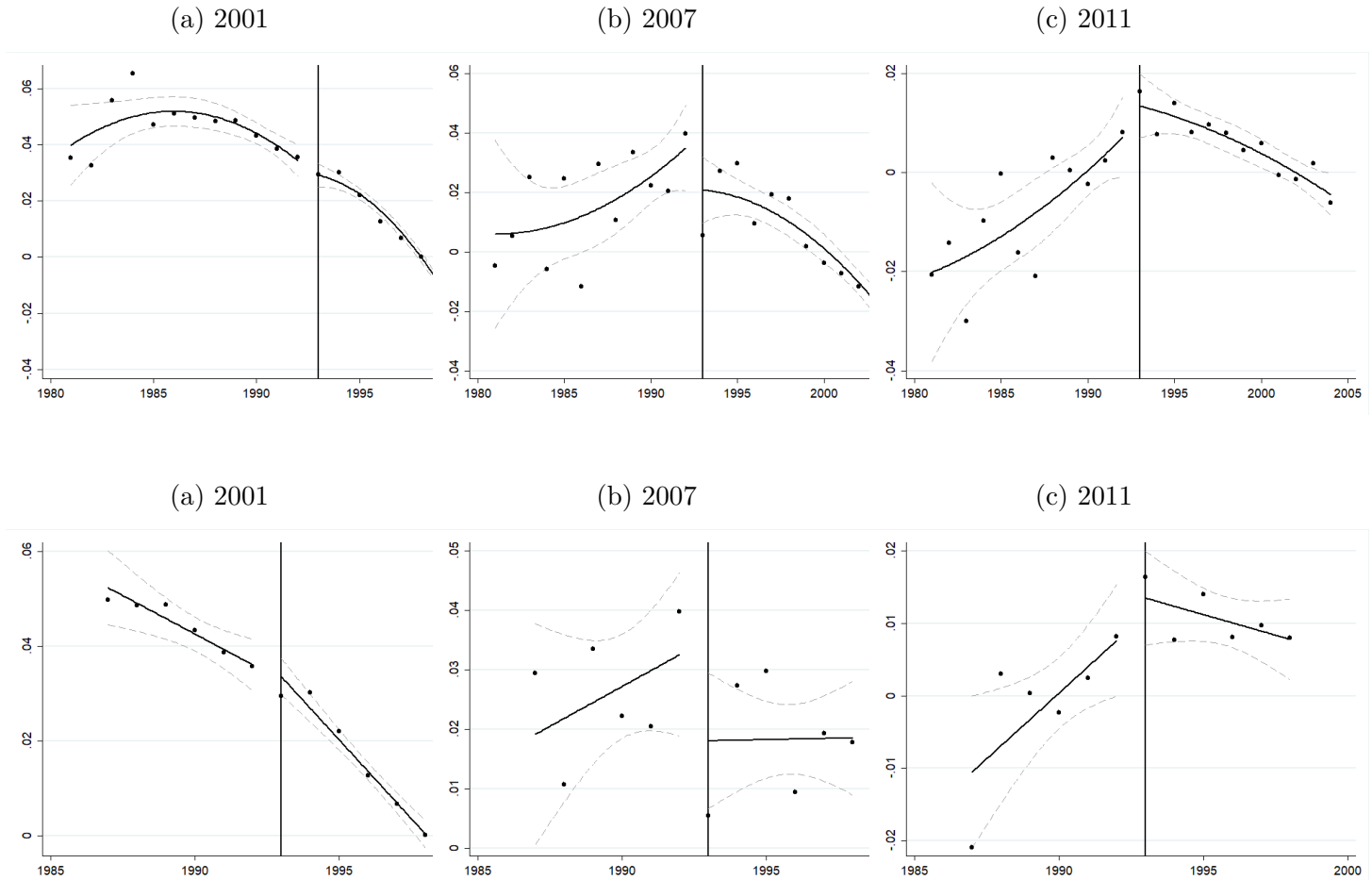
Table 1.A.3: Labor Market Effects of the CSG on Mothers, 2001, 2007, & 2011, Linear Fit, Window: 1987–1998

	Year 2001 - “Before”				
	(1) Active	(2) Unemployed	(3) Employed	(4) Informal	(5) Formal
CSG	0.0023 (0.0039)	0.0021 (0.0043)	0.0003 (0.0043)	0.0046 (0.0033)	-0.0049 (0.0035)
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	266,587	266,587	266,587	266,587	266,587
R-squared	0.1268	0.0682	0.1565	0.0784	0.2048
	Year 2007 - “During”				
	(1) Active	(2) Unemployed	(3) Employed	(4) Informal	(5) Formal
CSG	0.0094 (0.0100)	0.0266*** (0.0101)	-0.0172 (0.0117)	0.0125 (0.0097)	-0.0297*** (0.0103)
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	35,321	35,321	35,321	35,321	35,321
R-squared	0.1157	0.0771	0.1390	0.0631	0.2116
	Year 2011 - “After”				
	(1) Active	(2) Unemployed	(3) Employed	(4) Informal	(5) Formal
CSG	-0.0003 (0.0060)	-0.0024 (0.0049)	0.0021 (0.0062)	-0.0112** (0.0050)	0.0133** (0.0055)
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	106,255	106,255	106,255	106,255	106,255
R-squared	0.1302	0.0366	0.1493	0.0423	0.1904

*Note:* \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . This table gives the OLS estimates of Equation 1.12 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 linear for the window of cohorts born between 1987 to 1998. 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.

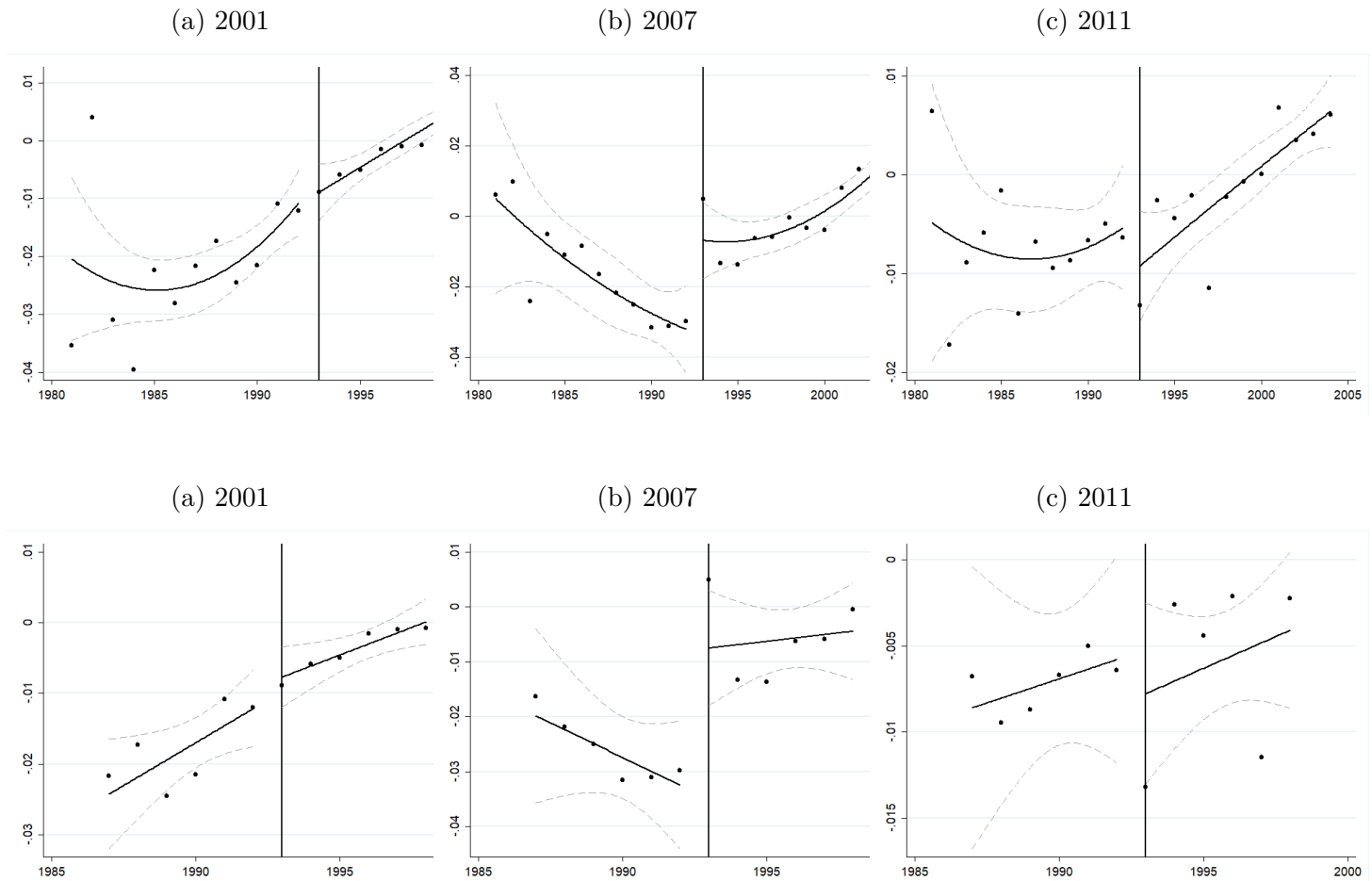
*Source:* Author’s calculations on Census 2001 & 2011, and Community Survey (2007).

Figure 1.A.6: CSG Effect on Employment Rate (Residuals)



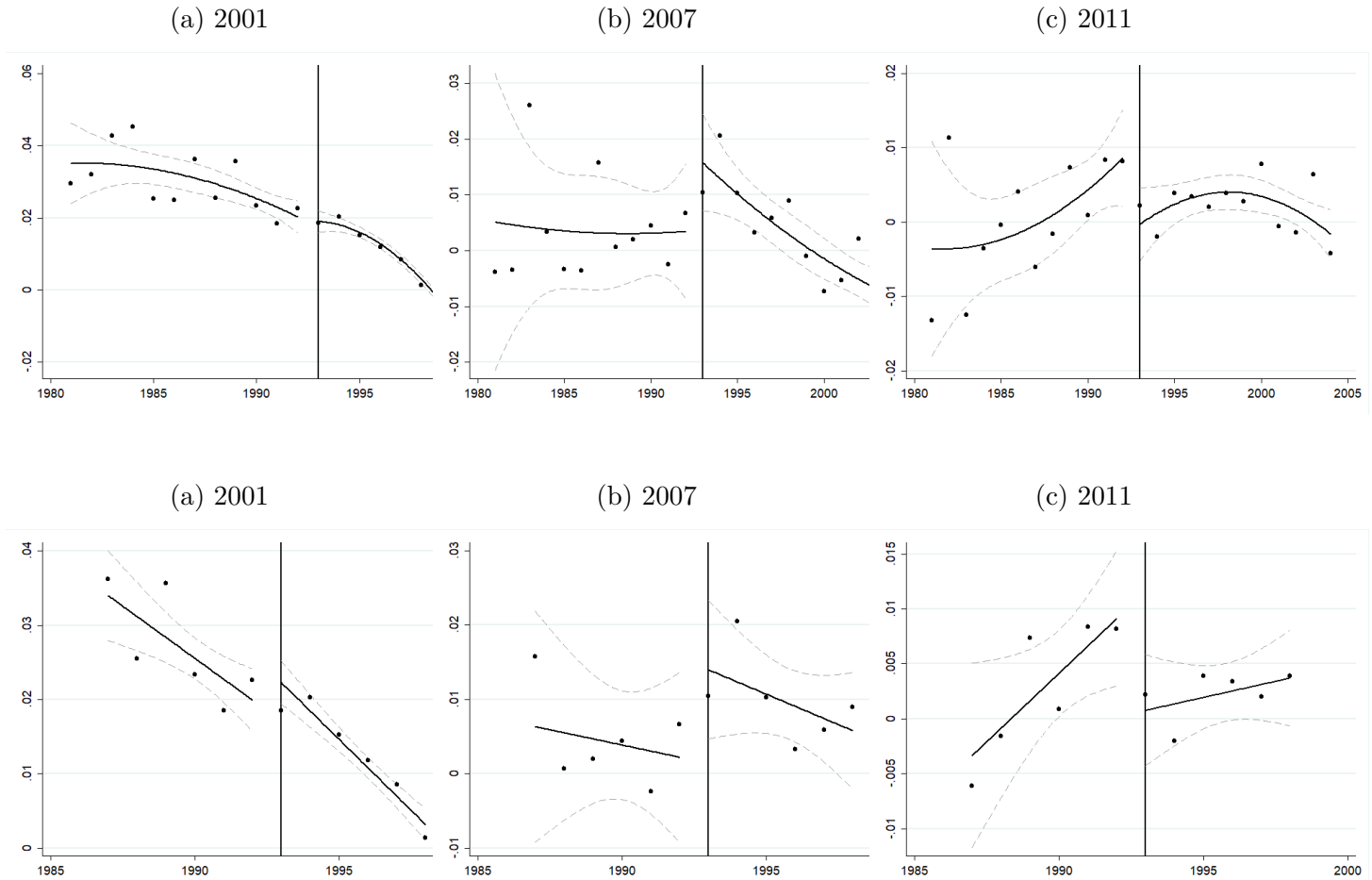
*Note:* These graphs report the estimations of Equation 1.12 in graphical form. The residuals are plotted after controlling for the same variables as listed in Table 1.2. In the upper panel, a quadratic function is fitted on both sides of the threshold for the three separate years. The window, 1981 to 2004, is chosen according to an Akaike Information Criterion (AIC) that maximizes the window for a quadratic function. In the lower panel, a linear function is fitted on both sides of the threshold for the three separate years. The window, 1987 to 1998, is chosen according to an Akaike Information Criterion (AIC) that maximizes the window for a linear function.

Figure 1.A.7: CSG Effect on Unemployment Rate (Residuals)



*Note:* These graphs report the estimations of Equation 1.12 in graphical form. The residuals are plotted after controlling for the same variables as listed in Table 1.2. In the upper panel, 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. In the lower panel, a linear function is fitted on both sides of the threshold for the three separate years. The window, 1987 to 1998, is chosen according to an Akaike Information Criterion (AIC) that maximizes the window for a linear function.

Figure 1.A.8: CSG Effect on Informal Sector Employment Rate (Residuals)



*Note:* These graphs report the estimations of Equation 1.12 in graphical form. The residuals are plotted after controlling for the same variables as listed in Table 1.2. In the upper panel, 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. In the lower panel, a linear function is fitted on both sides of the threshold for the three separate years. The window, 1987 to 1998, is chosen according to an Akaike Information Criterion (AIC) that maximizes the window for a linear function.

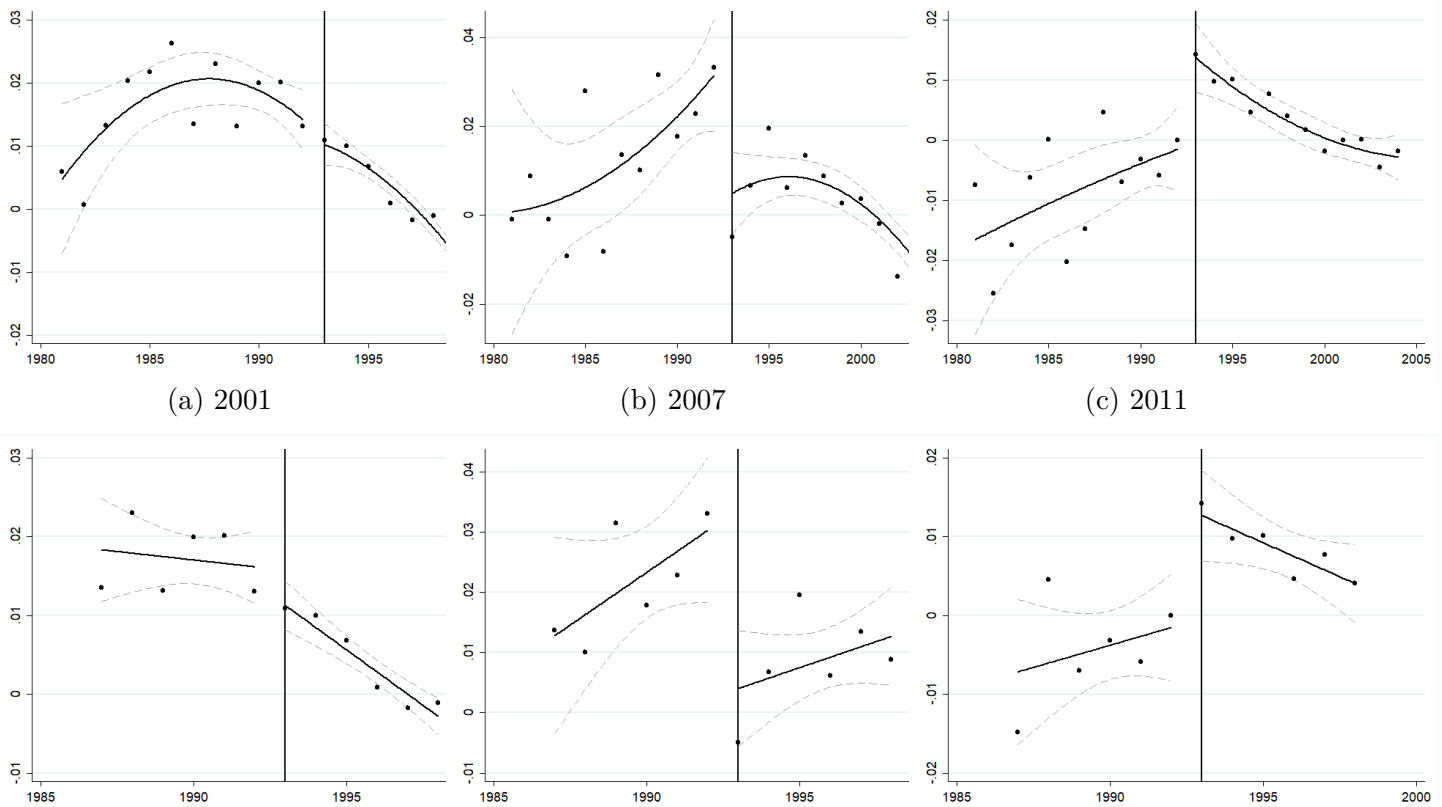


Figure 1.A.9: CSG Effect on Formal Sector Employment Rate (Residuals)

(a) 2001

(b) 2007

(c) 2011



*Note:* These graphs report the estimations of Equation 1.12 in graphical form. The residuals are plotted after controlling for the same variables as listed in Table 1.2. In the upper panel, 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. In the lower panel, a linear function is fitted on both sides of the threshold for the three separate years. The window, 1987 to 1998, is chosen according to an Akaike Information Criterion (AIC) that maximizes the window for a linear function.

Table 1.A.4: Labor Market Effects of the CSG on Other Adult Household Members, 2001, 2007, & 2011, Quadratic Fit, Window: 1981–2004

	<b>Year 2001 - “Before”</b>				
	(1) Active	(2) Unemployed	(3) Employed	(4) Informal	(5) Formal
CSG	-0.0033 (0.0053)	-0.0048 (0.0050)	0.0016 (0.0042)	0.0008 (0.0029)	0.0007 (0.0041)
Mean $Y$ at Threshold	0.4932	0.2461	0.2471	0.0654	0.1816
Weighted $n$ at Threshold	245,446	245,446	245,446	245,446	245,446
Observations	558,740	558,740	558,740	558,740	558,740
R-squared	0.4327	0.1121	0.3854	0.1365	0.3160
	<b>Year 2007 - “During”</b>				
	(1) Active	(2) Unemployed	(3) Employed	(4) Informal	(5) Formal
CSG	0.0038 (0.0156)	-0.0070 (0.0173)	0.0109 (0.0165)	0.0070 (0.0122)	0.0038 (0.0158)
Mean $Y$ at Threshold	0.7716	0.2991	0.4724	0.1311	0.3413
Weighted $n$ at Threshold	92,183	92,183	92,183	92,183	92,183
Observations	62,737	62,737	62,737	62,737	62,737
R-squared	0.1508	0.0958	0.2669	0.0772	0.2310
	<b>Year 2011 - “After”</b>				
	(1) Active	(2) Unemployed	(3) Employed	(4) Informal	(5) Formal
CSG	-0.0085 (0.0115)	0.0051 (0.0104)	-0.0136 (0.0120)	-0.0075 (0.0083)	-0.0061 (0.0115)
Mean $Y$ at Threshold	0.7240	0.1903	0.5336	0.1310	0.4026
Weighted $n$ at Threshold	38,942	38,942	38,942	38,942	38,942
Observations	114,687	114,687	114,687	114,687	114,687
R-squared	0.1367	0.0525	0.1947	0.0505	0.1756

*Note:* \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . This table gives the OLS estimates of Equation 1.12 on other adult household members’ labor market outcomes in 2001, 2007, and 2011, respectively (Black and Coloured population only, born between 1960 and 1985). The forcing variable is the cohort of birth of the youngest child ever born to a mother in the same household. 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, gender, and household size. Standard errors are clustered at the household level.

*Source:* Author’s calculations on Census 2001 & 2011, and Community Survey (2007).

Table 1.A.5: 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)
<i>2007 - “During”</i>						
CSG	-0.0210 (0.0133)	0.0021 (0.0064)	0.0053 (0.0057)	0.0069 (0.0100)	-0.0032 (0.0029)	-0.0280** (0.0117)
Mean <i>Y</i> 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	0.1510	0.0172	0.0190	0.0594	0.0083	0.1971
<i>2011 - “After”</i>						
CSG	0.0072 (0.0071)	-0.0045 (0.0038)	-0.0057** (0.0025)	-0.0056 (0.0053)	0.0012 (0.0028)	0.0129** (0.0061)
Mean <i>Y</i> 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	0.0340	0.0112	0.1666

*Note:* \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . This table gives the OLS estimates of Equation 1.12 on mothers’ occupational status (Columns (1) and (2)), also decomposed by sector (Columns (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 for the cohort at the threshold. Standard errors are clustered at the household level.

*Source:* Author’s calculations on Community Survey (2007) and Census (2011).

Table 1.A.6: 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) <b>Within</b> -Occupation Variation	(5) <b>Across</b> -Occupation Variation
CSG	-0.0251*** (0.0097)	-0.0147* (0.0087)	-0.0105 (0.0072)	-0.0111 (0.0072)	0.0006 (0.0009)
Observations	111,463	111,463	111,463	111,463	111,463
R-squared	0.1469	0.1294	0.0591	0.0534	0.0386

*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 decompose Column (3) in the following way:  $Informal_i = occupation_i + \epsilon_i$ , and then separately predict the fitted values and the residuals of this equation. I then estimate Equation 1.12 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 (cubic), education, race, marital status, province, household size, and municipality. Standard errors are clustered at the household level.

*Source:* Author's calculations on Census 2011.

Table 1.A.7: Difference-in-Differences Estimates of the Effect of the CSG on Mothers

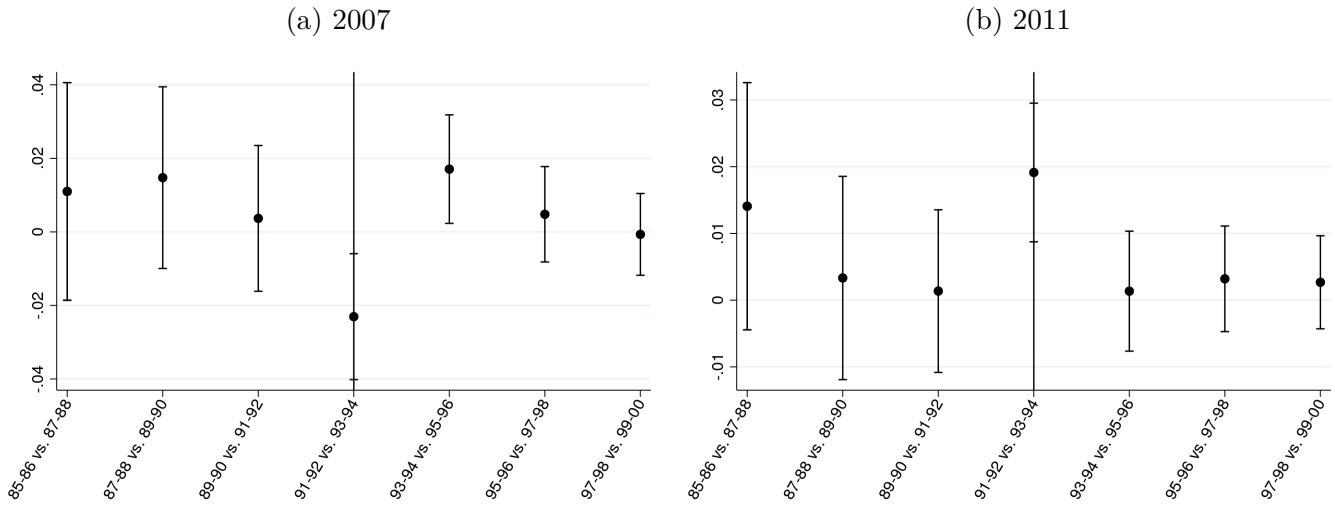
	(1) Active	(2) Unemployed	(3) Employed	(4) Informal	(5) Formal
<b>Window: 1992 vs. 1993</b>					
CSG×2007	0.0022 (0.0121)	0.0322** (0.0127)	-0.0300** (0.0142)	0.0080 (0.0116)	-0.0379*** (0.0124)
CSG×2011	0.0032 (0.0082)	-0.0091 (0.0074)	0.0123 (0.0086)	-0.0022 (0.0068)	0.0145* (0.0075)
Observations	59,614	59,614	59,614	59,614	59,614
R-squared	0.1203	0.0960	0.1480	0.0670	0.1982
<b>Window: 1993 vs. 1994</b>					
CSG×2007	0.0012 (0.0114)	-0.0185 (0.0122)	0.0196 (0.0133)	0.0075 (0.0109)	0.0122 (0.0114)
CSG×2011	0.0005 (0.0077)	0.0075 (0.0070)	-0.0070 (0.0080)	-0.0037 (0.0063)	-0.0033 (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
<b>Window: 1991–1992 vs. 1993–1994</b>					
CSG×2007	0.0130 (0.0088)	0.0213** (0.0090)	-0.0082 (0.0102)	0.0130 (0.0084)	-0.0212** (0.0089)
CSG×2011	0.0081 (0.0059)	-0.0077 (0.0054)	0.0158** (0.0062)	-0.0022 (0.0049)	0.0180*** (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 1.13 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.

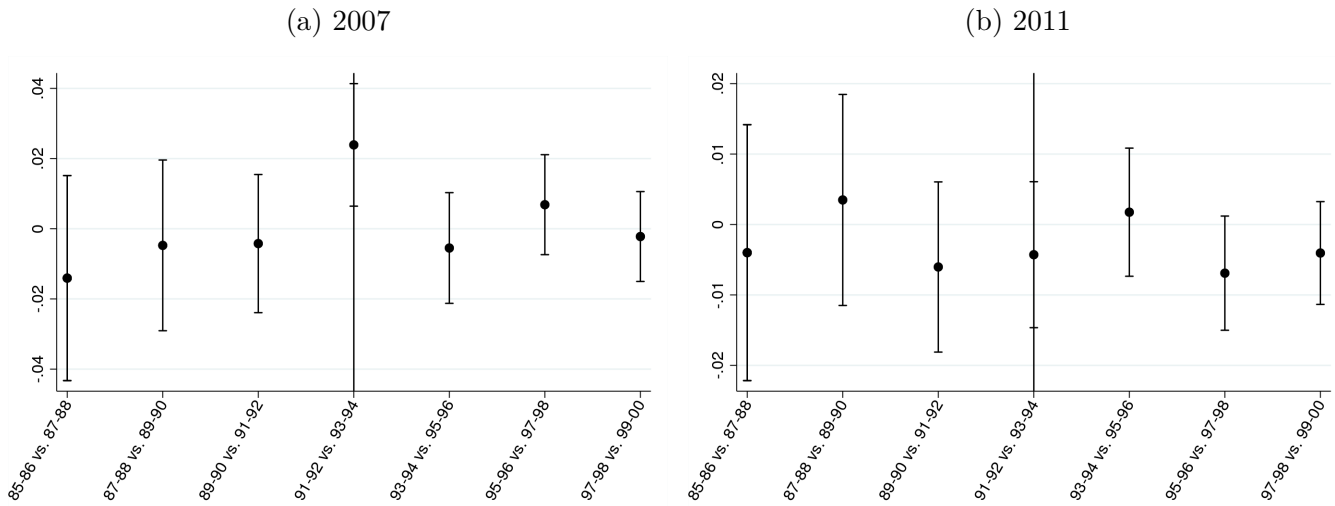
*Source:* Author's calculations on Census 2001 & 2011, and Community Survey (2007).

Figure 1.A.10: Difference-in-Differences Estimates with Varying Windows

**Formal Employment**



**Unemployment**

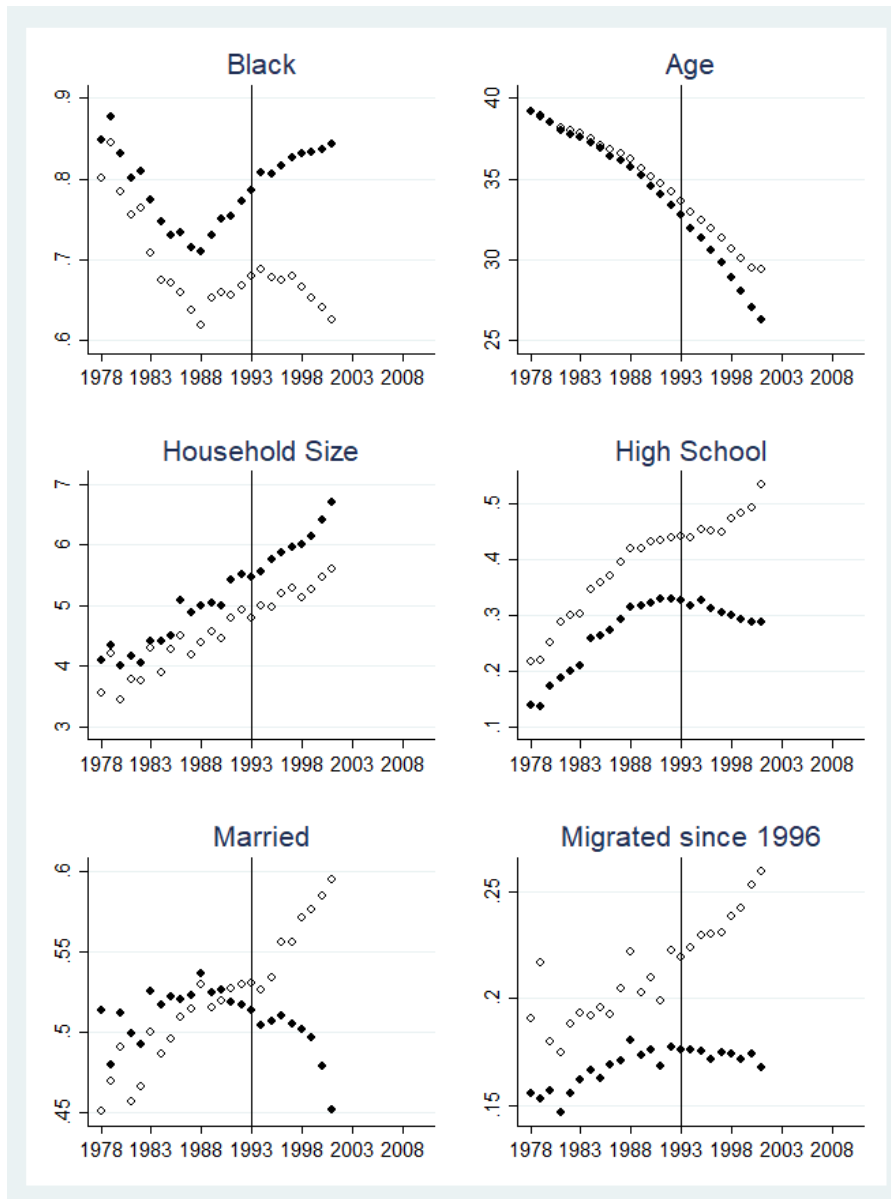


*Note:* This graph gives the coefficients of Equation 1.13, the difference-in-differences estimation, with varying cohort windows, on both formal sector employment and unemployment, for both 2007 and 2011. The first two cohorts indicate the control group, while the last two the treated group (for example, 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).

### 1.A.3 Robustness Checks

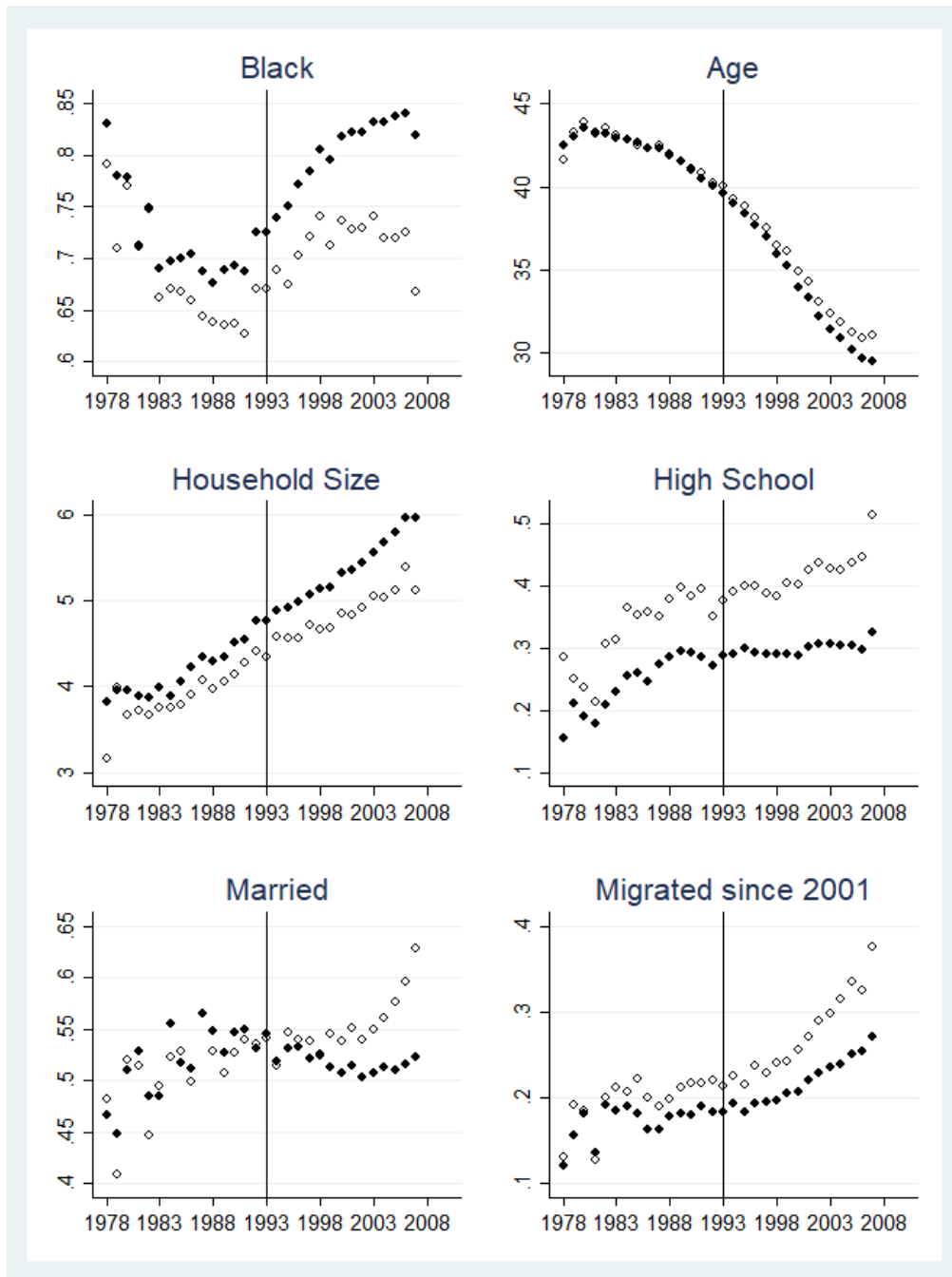
Figure 1.A.11: Observable Characteristics (All Mothers and Employed only), 2001



*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 a 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 1.A.12: Observable Characteristics (All Mothers and Employed only), 2007

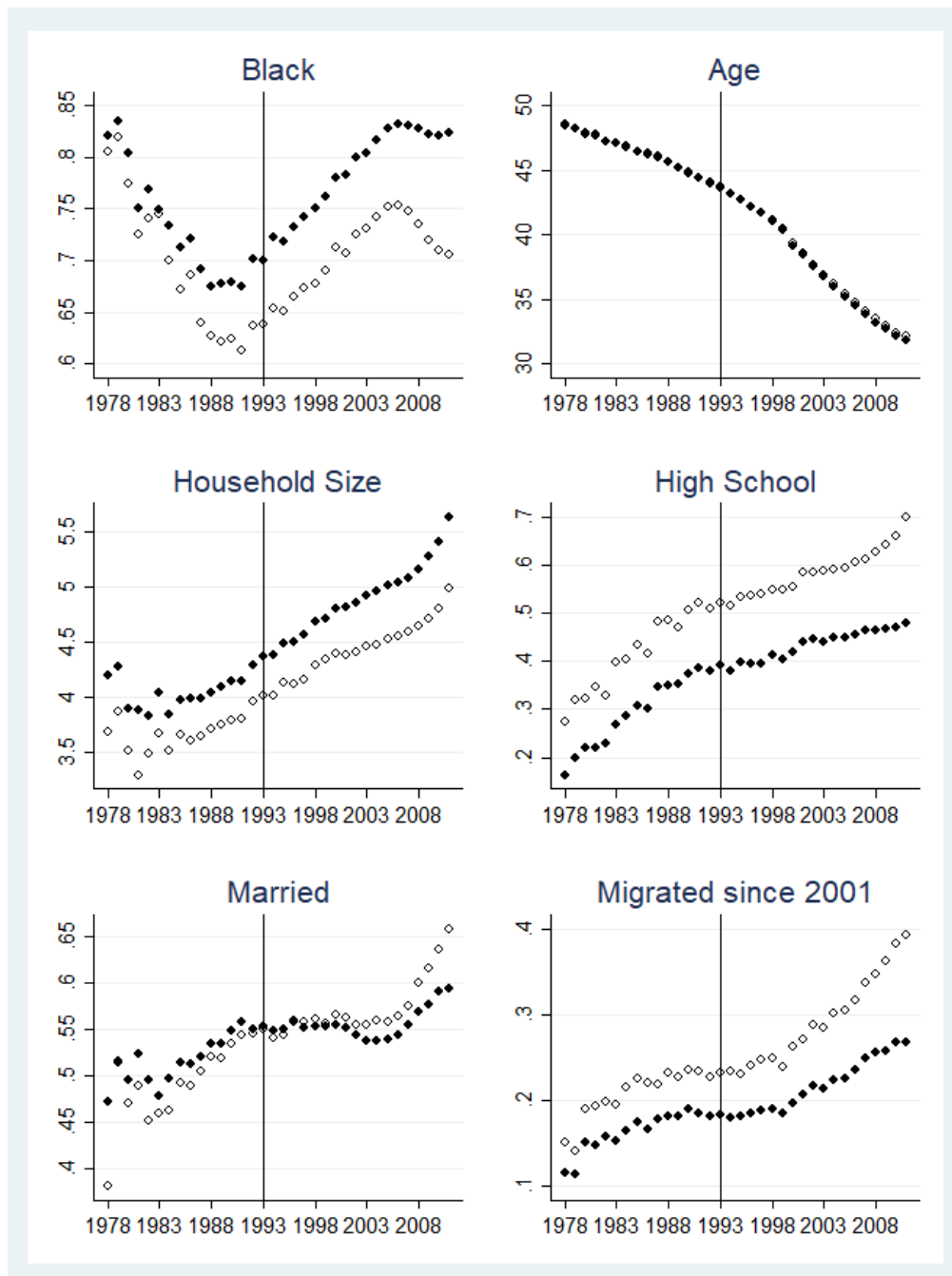


*Note:* These graphs show the distribution of observable characteristics by birth cohort of the youngest child, for all mothers (black dots) and employed mothers only (hollow dots). *Black* refers to the share of mothers who are neither Coloured, nor Indian, nor White. *Age* is the age of the mother, not of the child. *Household size* refers to the number of individuals in the household. *High School* refers to the share who has obtained a 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 1.A.13: Observable Characteristics (All Mothers and Employed only), 2011

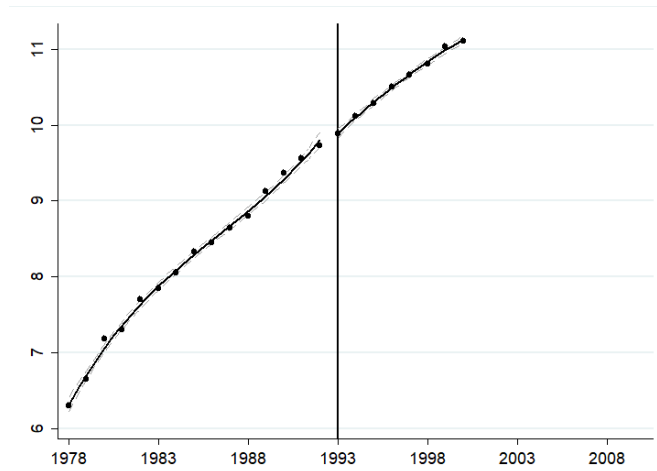


*Note:* These graphs show the distribution of observable characteristics by birth cohort of the youngest child, for all mothers (black dots) and employed mothers only (hollow dots). *Black* refers to the share of mothers who are neither Coloured, nor Indian, nor White. *Age* is the age of the mother, not of the child. *Household size* refers to the number of individuals in the household. *High School* refers to the share who has obtained a 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.

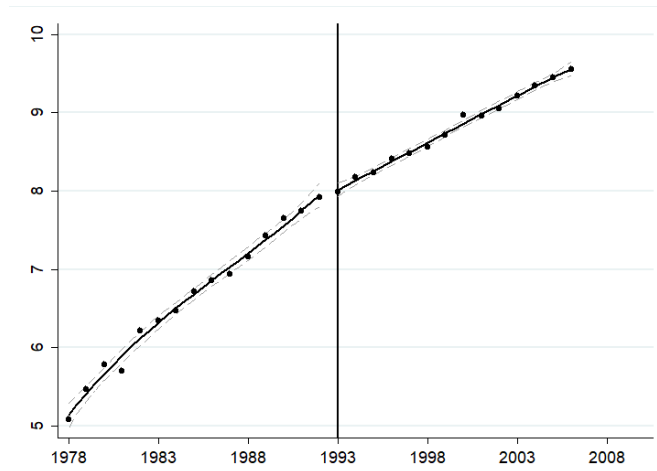
*Source:* Census (2011).

Figure 1.A.14: Density, 2001, 2007, & 2011

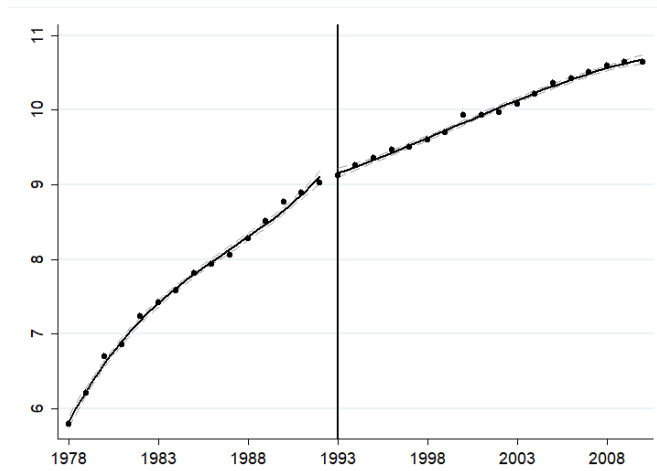
(a) 2001



(b) 2007



(c) 2011



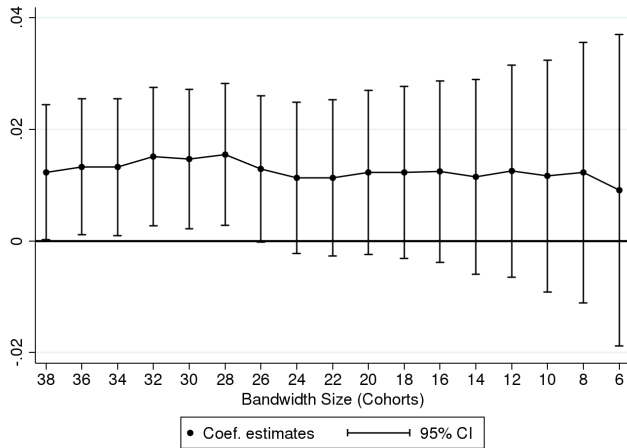
*Note:* These graphs give the log-density of mothers by year of birth of their youngest child. In order to compare the same population over time, the sample is limited to mothers born between 1960 and 1985, due to the questionnaire design of the census, where only women under 50 are asked for fertility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

*Source:* Census (2001 & 2011) and Community Survey (2007).

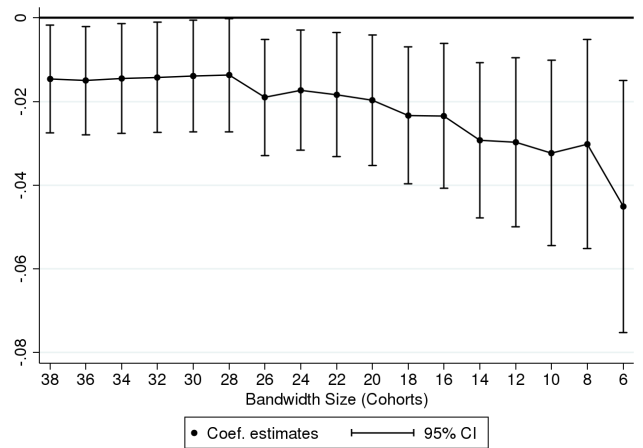
Figure 1.A.15: Bandwidth Sensitivity, 2007

**Linear**

(a) Informal

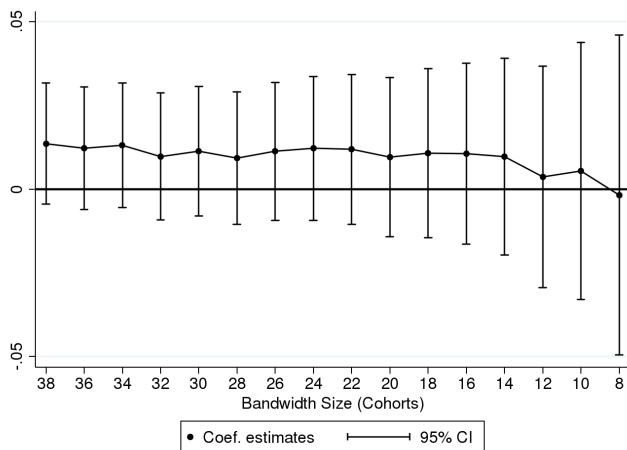


(b) Formal

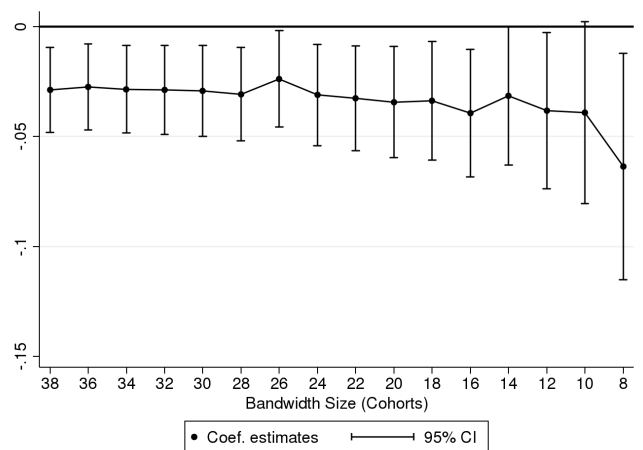


**Quadratic**

(a) Informal



(b) Formal



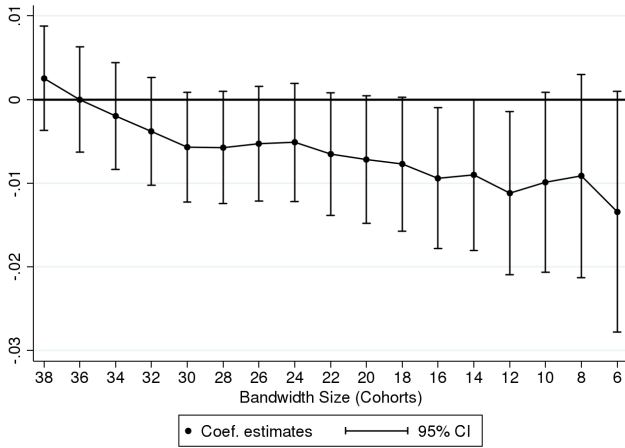
Note: Panel (a) tests the sensitivity of the estimates of Equation 1.12 on informal employment to the size of the bandwidth with a linear fit (upper panel) and a quadratic fit (lower panel). Panel (b) performs the same exercise for formal employment. The boundaries around the coefficients are 95% confidence intervals.

Source: Author's calculations on Community Survey (2007).

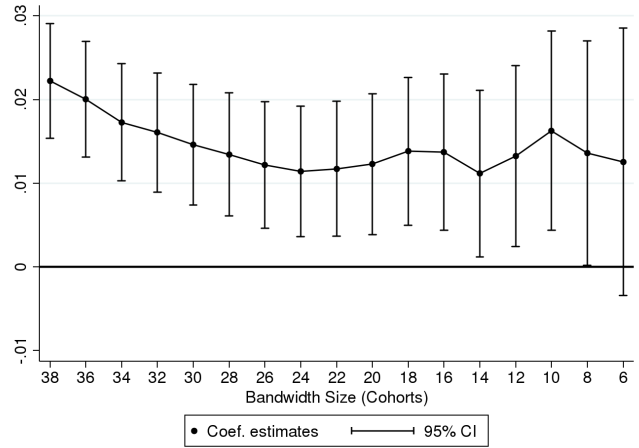
Figure 1.A.16: Bandwidth Sensitivity, 2011

**Linear**

(a) Informal

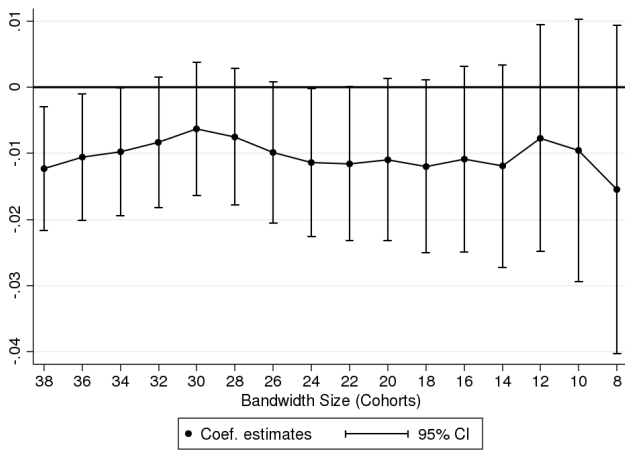


(b) Formal

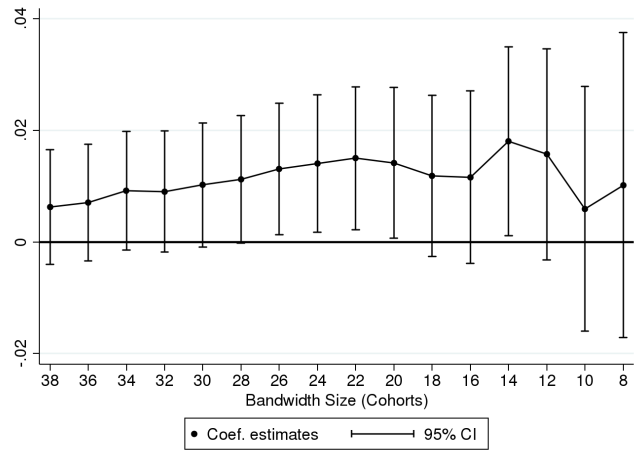


**Quadratic**

(a) Informal



(b) Formal



Note: Panel (a) tests the sensitivity of the estimates of Equation 1.12 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.

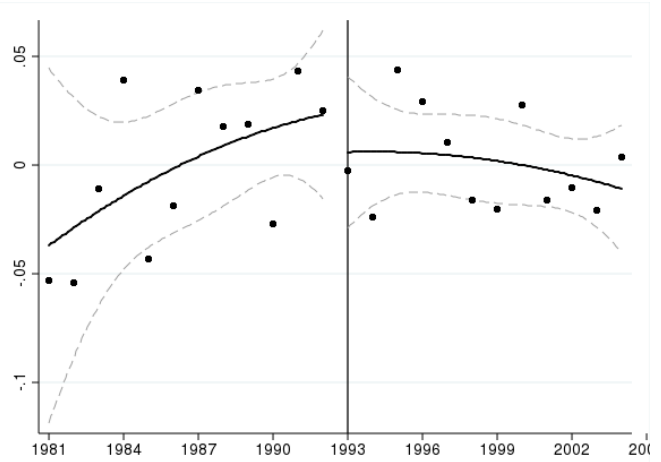
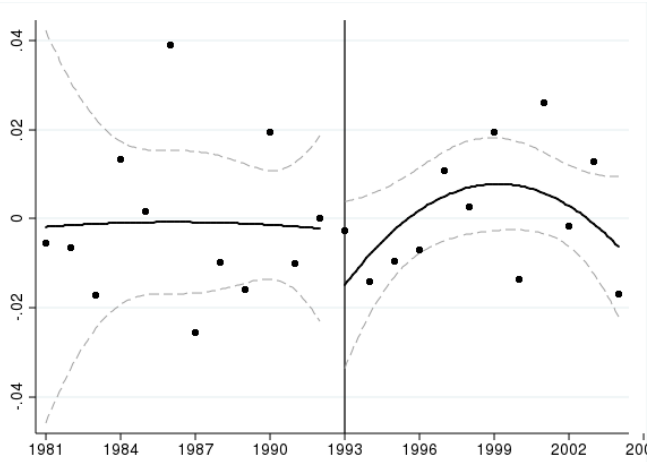
Source: Author's calculations on Census (2011).

Figure 1.A.17: Placebo Test - White Mothers Only, 2007 & 2011

2007

Informal

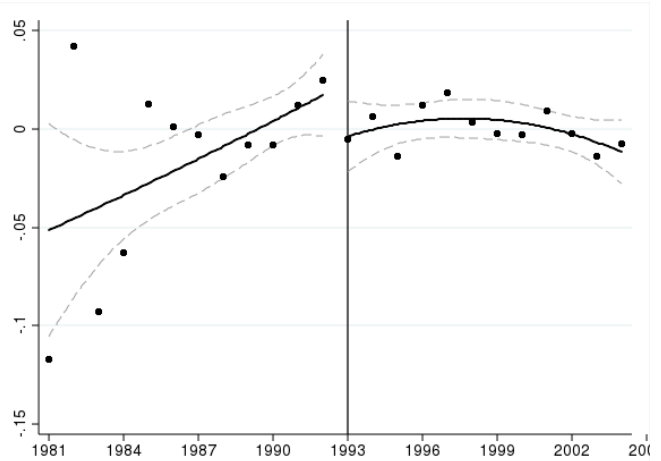
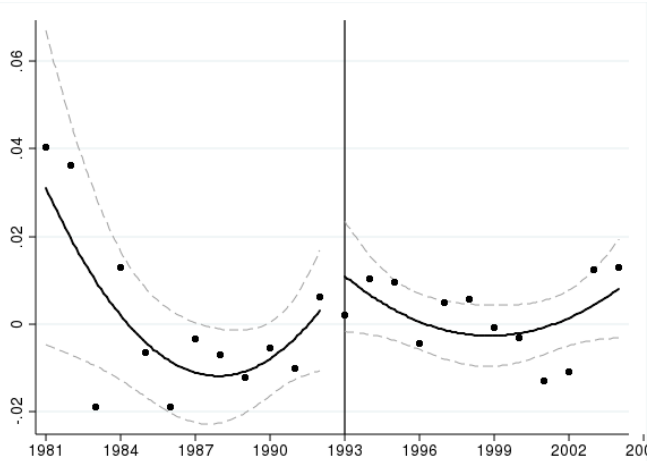
Formal



2011

Informal

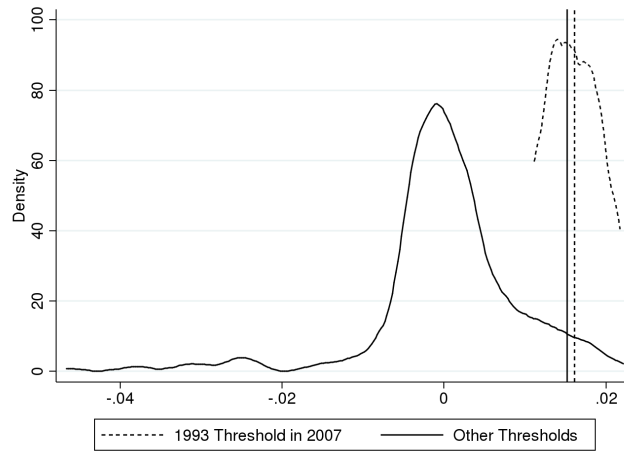
Formal



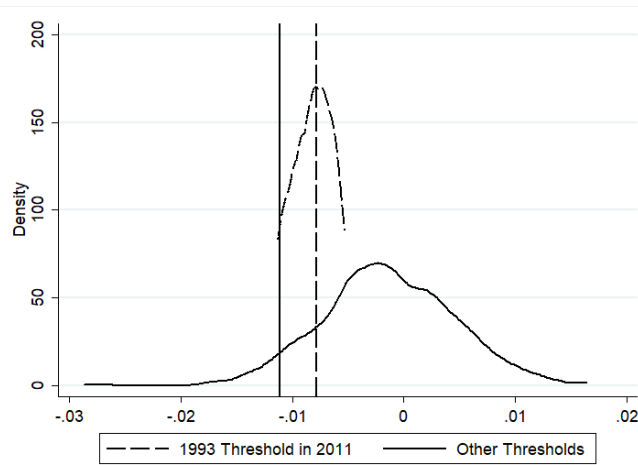
*Note:* These graphs give the estimates of Equation 1.12 on informal and formal employment for White mothers, who do not receive the CSG, in 2007 (upper panel) and 2011 (lower panel). Results are similar with a linear window. For details on the specification, see the note on Table 1.2.  
*Source:* Author's calculations on Census (2011) and Community Survey (2007).

Figure 1.A.18: Placebo Thresholds

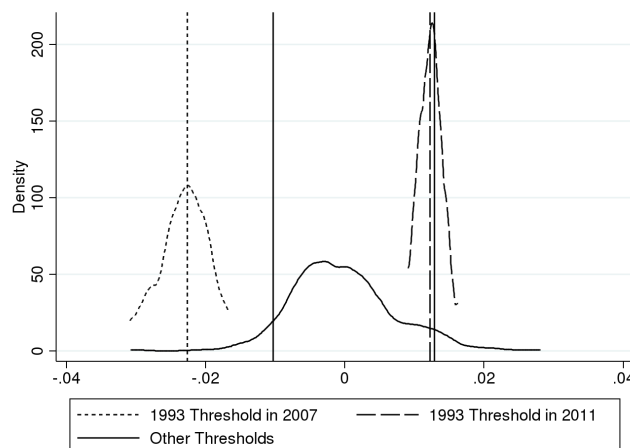
(a) Unemployment



(b) Informal Employment

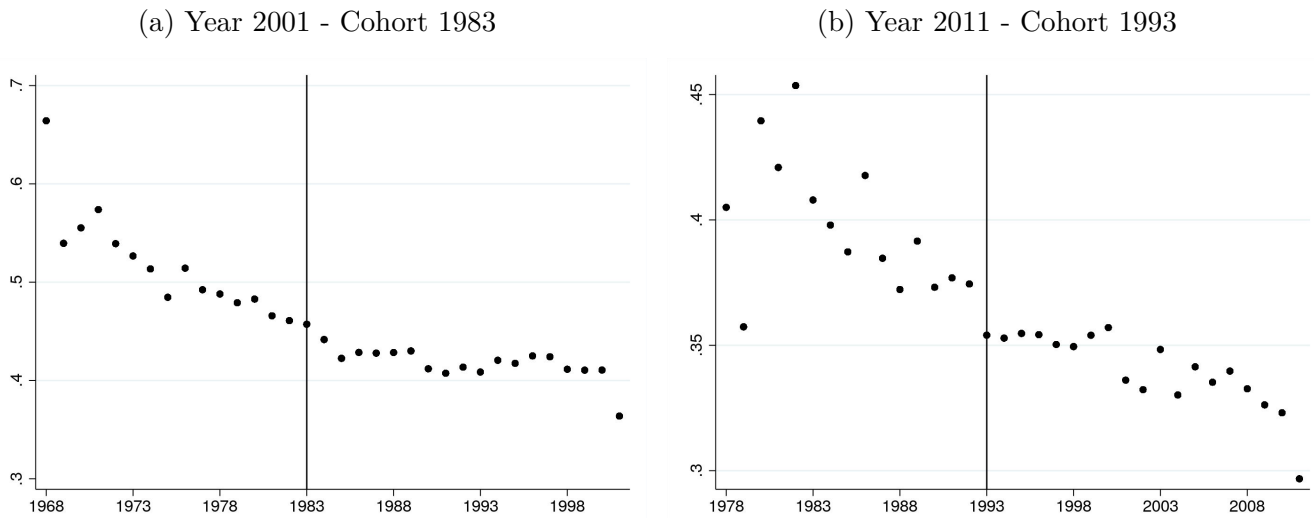


(c) Formal Employment



*Note:* These graphs give the distribution of the CSG coefficient of Equation 1.12, 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  $\pm 14$  to  $\pm 5$ ) 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  $\pm 14$  to  $\pm 5$ ). The solid vertical lines give the 5th and 95th percentiles of the distributions. *Source:* Census (2001 & 2011) and Community Survey (2007).

Figure 1.A.19: Share of Informal Employment by Birth Cohort of Youngest Child (Cohorts of the Same Age), 2001 & 2011



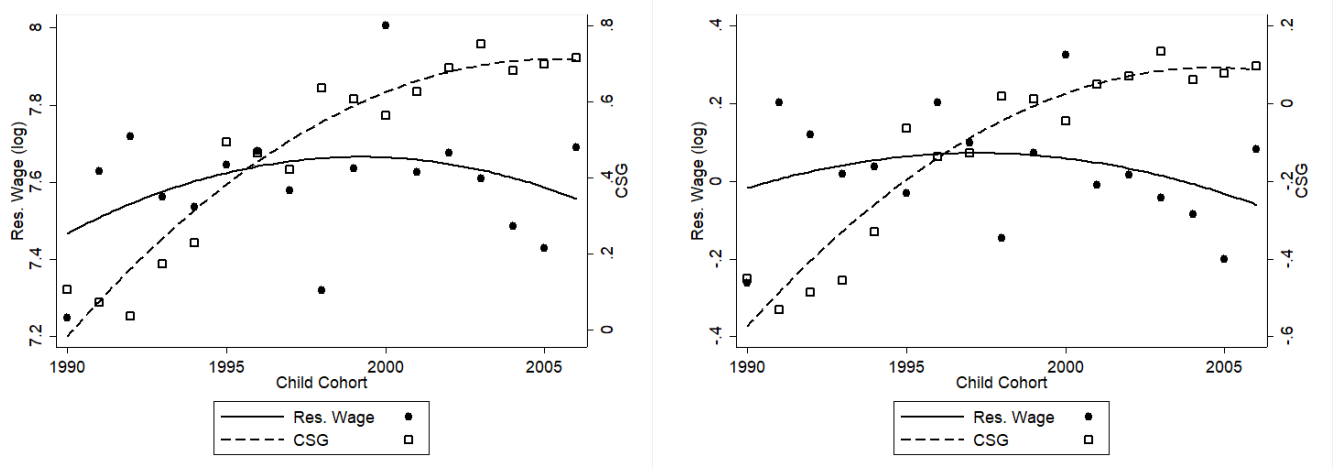
*Note:* These graphs give the probability of being employed in the informal sector, conditional on being employed, for mothers by cohort of birth of the youngest child ever born, in 2001 and 2011, respectively. In the left panel, the threshold is set at cohort 1983, who is exactly the same age as cohort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

*Source:* Census (2001 & 2011).

Figure 1.A.20: Correlation between Reported Reservation Wages and CSG

a) Without Controls

b) With Controls



*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, both with a quadratic fit. The left panel plots the unconditional value, while the right panel plots the residuals controlling for a set of observable characteristics listed in Table 1.2. The sample is limited to unemployed mothers.

*Source:* NIDS (2010–2014).

## 1.A.4 Data Appendix

### Census 2001

*Employment/Informality Status:* In the SEVEN DAYS before 10 October did (the person) do any work for PAY (in cash or in kind) PROFIT or FAMILY GAIN, for one hour or more?

1 =Yes: formal registered (non-farming), 2 =Yes: informal unregistered (non-farming), 3 =Yes: farming, 4 =Yes: has work but was temporarily absent, 5 =No: did not have work

*Unemployment Status:* In the PAST FOUR WEEKS before 10 October has (the person) taken active steps to find employment?

*and* If offered work, how soon could (the person) start?

1 = Within one week, 2 = More than 1 week, up to 2 weeks, 3 = More than 2 weeks, up to 4 weeks, 4 = Some time after 4 weeks, 5 = Does not choose to work

*Work Status:* How can one best describe (the person's) main activity or work status?

1 = Paid employee, 2 = Paid family worker, 3 = Self-employed, 4 = Employer, 5 = Unpaid family worker, 6 = Other (specify)

*Industry:* What does the business do (main economic activity)?

*Occupation:* What is the main occupation of (the person) in this workplace?

*Fertility:* When was (the person's) last child born? (women aged 12 to 50 years)

*Personal Income (Categorical):* What is the income category that best describes the gross income of (this person) before tax?

### Community Survey 2007

*Employment Status:* In the last 7 days, did (the person) run or do any kind of business, big or small, for himself/herself or with one or more partners even for only one hour?

*and* In the last 7 days, did (the person) do any work for a wage, salary, commission



or payment in kind (excluding domestic worker) even for only one hour?

*and* In the last 7 days, did (the person) do any work as a domestic worker for a wage, salary or payment in kind even for only one hour?

*and* In the last 7 days, did (the person) help unpaid in a household business of any kind even for only one hour?

*and* In the last 7 days, did (the person) do any work on his/her own or the household's plot, farm, food garden, cattle post or kraal, or help in growing farm produce or in looking after animals for the household even for only one hour?

*and* In the last 7 days, did (the person) do any construction or major repair work on his/her own home, plot, cattle post or business even for only one hour?

*and* In the last 7 days, did (the person) catch any fish, prawns, shell fish, wild animals either as food for sale or for household use, even for only one hour?

*and* Even though (the person) did not do any of these activities in the last seven days, does he/she have a job, business, or other economic or farming activity that he/she will definitely return to?

*Unemployment Status:* During the past four weeks, has (the person) taken any action to look for any kind of work?

*and* During the past four weeks, has (the person) taken any action to start any kind of business?

*and* If a suitable job is offered, how soon can (the person) start work?

1 = Within one week, 2 = More than 1 week, up to 2 weeks, 3 = More than 2 weeks, up to 4 weeks, 4 = Some time after 4 weeks, 5 = Does not choose to work

*Informality Status:* Is the organisation / company / business / enterprise/branch where (the person) works, in the formal or informal sector?

1=In the formal sector, 2=In the informal sector (including domestic work), 3=Do not know

*Work Status:* How can one describe (the person)'s main activity or work status best?

1 = Paid employee, 2 = Paid family worker, 3 = Self-employed, 4 = Employer, 5 = Unpaid family worker

*Industry:* What is the main business/function of the company that (the person) works (main economic activity) for?

*Occupation:* What is the main occupation of (the person) in this workplace?

*Fertility:* When was (the person)'s last child born, even if the child died soon after birth? (women aged 12 to 50 years)

*Personal Income (Categorical):* What is the income category that best describes the gross monthly or annual income of (the person) before deductions and including all sources of income?

## **Census 2011**

*Employment Status:* In the SEVEN DAYS before 10 October ...Did (name) work for a wage, salary, commission or any payment in kind (including paid domestic work), even if it was for only one hour?

*and* In the SEVEN DAYS before 10 October ...Did (name) run or do any kind of business, big or small, for herself/himself or with one or more partners, even if it was for only one hour?

*and* In the SEVEN DAYS before 10 October ...Did (name) help without being paid in any kind of business run by her/his household, even if it was for only one hour?

*and* Even though (name) did not do any work for pay, profit or did not help without pay in a household business in the SEVEN DAYS before 10 October, did he/she have a paid job or business that he/she would definitely return to?

*Unemployment Status:* In the four weeks before 10 October was (name) looking for any kind of job or trying to start any kind of business?

*and* Would (name) have liked to work in the SEVEN DAYS before 10 October?

*and* If a suitable job had been offered or circumstances had allowed, would (name) have been able to start work or a business in the SEVEN DAYS before 10 October?

*Informality Status:* Is (name's) place of work .....

1=In the formal sector, 2=In the informal sector, 3=Private household, 4= Do not

know

*Industry:* What are the main goods or services produced at (name's) place of work or its main functions?

*Occupation:* What kind of work does (name) usually do in his/her main job/business?

*Fertility:* When was (name's) last child born, even if the child died soon after birth? (women aged 12 to 50 years)

*Personal Income (Categorical):* What is the income category that best describes the gross monthly or annual income of (name) before deductions and including all sources of income?

### **Labour Force Surveys, 2002–2007**

Employment status is measured in a similar way to the Census; the reference period is the last seven days for employment and past four weeks for unemployment. Measurement of the informal sector is identical to the Census.

*Informality Status:* Is the organisation / business / enterprise/branch where.... works, in the formal or informal sector?

1=In the formal sector, 2=In the informal sector (including domestic work), 3=Don't know

Alternatively, this data also allows to measure informal employment with questions on the presence of a written contract or whether the business is registered for VAT.

### **General Household Surveys, 2002–2010**

The General Household Survey has information on which household members receive the *Child Support Grant*. In 2002, this information was linked to the child. As from 2002, it is linked to the person actually receiving the grant.

*Child Support Grant:* Does ... receive any of the following Welfare grants? c) Child support grant

### **National Income Dynamics Survey, 2008, 2010–11, 2012, 2014**

The reference period in the NIDS is the same to define employment (4 weeks) and unemployment. Information on informal employment is available through whether the worker has a written contract or not, affiliation to social security (pension, unemployment, health contributions), or whether the business is registered for VAT (self-employed only). The NIDS also has information on the full birth history of every woman in the household.

*Reservation Wages:* What is the absolute lowest take-home wage that you would accept for any permanent, full-time work (per month)? (unemployed only)

*Transport Costs for Job Search:* How much did you spend on travel costs associated with looking for work last week? (unemployed only)

### **1.A.5 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 or not the business is registered.

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.<sup>49</sup> 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 1.A.8. 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

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<sup>49</sup>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 their industry and occupation.

in Agriculture<sup>50</sup>

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

Table 1.A.8: Overlap between Informal Sector and Informal Employment

<i>Employees</i>	Informal Sector	Informal Employment	
		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
<i>Self-employed</i>	Informal Sector	Informal Employment	
		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).

<sup>50</sup>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.



## Chapter 2

# Large Means-Tested Pensions with Informal Labor Markets: Evidence from South Africa\*

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## **Abstract**

Do means-tested social programs push workers into informality? This paper investigates the impact of a reform of the old-age, non-contributory and means-tested pension system in South Africa, which lowered the age of retirement from 65 to 60 for men only. By employing a difference-in-discontinuities (“diff-in-disc”) approach, we show that this reform triggered a large drop in elderly male employment. This response at the extensive margin comes largely from informal workers, who drop out of the labor force, while formal employment is mostly unaffected. This heterogeneity is not the result of lower wages in the informal sector; at the same level of wages, informal workers drop out, while formal workers do not. This occurs despite the implicit incentive to draw benefits and work informal jobs at the same time, and even if the means-test is located where formal and informal wages largely overlap. In total, the pension reform has driven about 25,000 individuals aged 60 to 64 out of the labor market. By exploiting the differential effects this reform has had across sectors, we can reject one-to-one substitution with the most similar workers.

**JEL Codes:** J26, J46, H55, O17

**Keywords:** old-age pensions; difference-in-discontinuities; informal employment; South Africa

## 2.1 Introduction

Understanding and quantifying labor supply responses to income shocks is a central question for economic research. It is also a key parameter for policy-makers, essential for the “calibration” of social programs, such as those non-contributory, public pensions schemes that are prevalent across developing countries. What is unique and relevant in these contexts is the concern that social assistance might distort incentives due to the segmented nature of the labor market, and create inefficiencies ([Gerard and Gonzaga \(2016\)](#)). Indeed, when a large portion of the workforce escapes labor market regulation, social assistance programs providing large income benefits conditional on means may discourage work, but also incentivize entry into the informal sector.

This paper exploits a reform of the public, non-contributory pension system of South Africa, which significantly lowered the age threshold necessary to access the pension, for men only. The focus of this paper is to document in detail the labor market effects on direct recipients, i.e. the elderly. By making use of this reform, we are able to precisely identify the labor market effects of the public pension, while avoiding potential confounders from other pension schemes, which we show to be sizable. We provide evidence that the disincentive effects of the OAP on its direct recipients are large, but almost exclusively concentrated on informal employment, as informal workers drop out of the labor force regardless of the level of hourly wages. Instead, formal workers are affected only at very low level of hourly wages. We believe these results have implications for both economic research and policy design. These results speak (indirectly) to the potentially large welfare effects of a public pension in a developing country: providing individuals with an external source of income results in large, informal labor supply adjustments at the extensive margin, suggesting that this type of program might relieve subsistence-level constraints. Moreover, in our results, we find no evidence that the pension gives the incentive to combine earnings from informal labor, which do not enter the means-test, with the pension transfer. This has important implication for policy design, as it indicates that this potentially concerning “perverse” effect is small, despite the means-test being located in an area where the two distributions largely overlap. Lastly, we also argue that,

in the aggregate, the jobs freed up by the elderly are unlikely to make a difference for the young. While employment responses of the elderly are relatively large, the affected group is too small to significantly affect the stock of available jobs. Although with limited statistical power, by exploiting differential effects of the reform across industries and occupations, we are able to reject a one-to-one substitution with those workers with most similar characteristics to the affected population.

Non-contributory, old-age pension programs have a long history in developing countries, and have been generally found to have negative, yet relatively small effects on employment of the elderly (Juárez and Pfitze (2015), de Carvalho Filho (2008), Kaushal (2014)), but these programs are usually less universal and provide lower amounts than the South African *Old Age Pension*. Contrary to Jung and Tran (2012), who model the general equilibrium effects of extending social security programs to informal workers, our analysis is focused on the direct effects on recipients.

The results of this paper are informative for the broader literature about informality in the labor market of developing countries. The main debate remains whether workers choose to work informally rather than in the formal sector, or whether informal employment provides subsistence-level jobs when better employment is lacking. These diverging views have led to the development of different hypotheses on the nature of the informal sector, also known as the “comparative advantage” and the “segmentation” hypotheses (summarized in Günther and Launov (2012)). Until recently, less attention has been paid to the interaction between social security programs and informality, more specifically on how different social policies shape the size and composition of the informal sector, and on how the presence of a significant informal sector might distort incentives within the labor market (Azuara and Marinescu (2013) and Bergolo and Cruces (2014) for non-contributory health insurance to workers and/or relatives; Garganta and Gasparini (2015) for cash transfers to those not in formal employment; Gerard and Gonzaga (2016) for how informality plays a role in unemployment insurance). We contribute to this growing literature by studying the effects of a strong income effect for workers near retirement, interacted with the “perverse” incentive to switch to informal work. Within the context of

older workers in the South African labor market, our results support the view of informality as mostly out of necessity.

There is a large literature on the South African *Old Age Pension*, which can be categorized in two main branches. The first branch of the literature relates to the impact of the OAP on children's outcomes and, more generally, to the intra-household allocation of resources (Duflo (2000, 2003), Jensen (2003), Ambler (2016)).<sup>1</sup> The labor market effects of the OAP have also been at the center of significant empirical research. Ranchhod (2006) estimates the discontinuity in labor supply and employment for individuals at the age cut-off point, finding large disincentive effects for both men and women. We are able to significantly improve on this identification by taking advantage of the latest reforms in age eligibility.<sup>2</sup> Evidence on the labor market impacts on other household members is mixed and significantly more complex, mostly because of the issue of selection when household composition changes as a result of pension receipt (Hamoudi and Thomas (2014)). Cross-sectional evidence from Bertrand et al. (2003) revealed the presence of disincentive effects for other members of the household who were not the direct recipients, but migration is also impacted by the grant, as members of OAP recipient households are more likely to migrate (Posel, Fairburn and Lund (2006)). To solve this selection issue, Ardington, Case and Hosegood (2009) make use of panel data to show that households that receive the OAP actually experience an increase in employment, which "occurs primarily through labor migration." However, recent evidence by Abel (2013), who also uses panel data but at the country level, has challenged these results. Thus, evidence on the employment effects for member of the same household is still unclear.

Incorporating the reform within our empirical strategy allows us to better identify the effects of the *Old Age Pension*. The main issue with the existing literature is that, in the cross-section, it is difficult to disentangle the effect of the OAP from

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<sup>1</sup>Duflo (2000, 2003) finds that the extension of the OAP to the African population in the early 90s has led to higher health and nutrition outcomes for girls who live in the same household as their grandmother. Ambler (2016) expands on this argument to show that this is the result of a change in bargaining power within the household upon pension receipt. Jensen (2003) shows that the public pension partly crowds out private transfers from other household members, such as remittances.

<sup>2</sup>To our knowledge, the only other paper to use these reforms for an analysis of the effects of public pension is Matsuda (2016).

other private schemes that might have the same age threshold, and that create discontinuities in the age profile regardless of the social pension. Indeed, we show that ignoring other pension scheme with similar age threshold leads to overestimate the employment response of the elderly by a factor of two, which could potentially confound the impact on other outcomes and affect the computation of income elasticities (Berg (2013), Ambler (2016)).

The outline of the paper is the following: Section 2 describes the South African *Old Age Pension*, and the latest reforms. Section 3 provides a simple conceptual framework to better frame the results of the paper. Section 4 presents the empirical strategy, showing the bias from not accounting for other pension schemes, and the results. Section 6 discusses the market level results. Section 7 concludes.

## **2.2 The South African *Old Age Pension***

The *Old Age Pension* (OAP) is a non-contributory pension system in South Africa, originally put in place in the 1920s to provide a minimal level of income to those who were not covered by a retirement plan (Duflo (2000), Woolard and Leibbrandt (2010)). During the Apartheid period, Black South Africans were consistently excluded from most social transfers, and, to a large extent, from public pensions. This occurred in several different ways: the means-test was set at different levels for different races, and was significantly lower for Black and Coloured people. Moreover, the benefits paid when actually eligible only made up one tenth of the amount paid to Whites (Duflo (2003)). Several other administrative loopholes were exploited to keep groups other than White to fully access the grant (for a full account of the history of the OAP in Apartheid South Africa refer to Lund (1993) and Woolard and Leibbrandt (2010)). The means-tests were equalised in 1992, and full “legal” equality was achieved as from 1993, about one year before the first democratic election (Duflo (2003), Woolard and Leibbrandt (2010)).

The *Old age Pension* scheme is the largest social program in South Africa in terms of spending (National Budget Reviews (2007-2013)). Access to the grant is

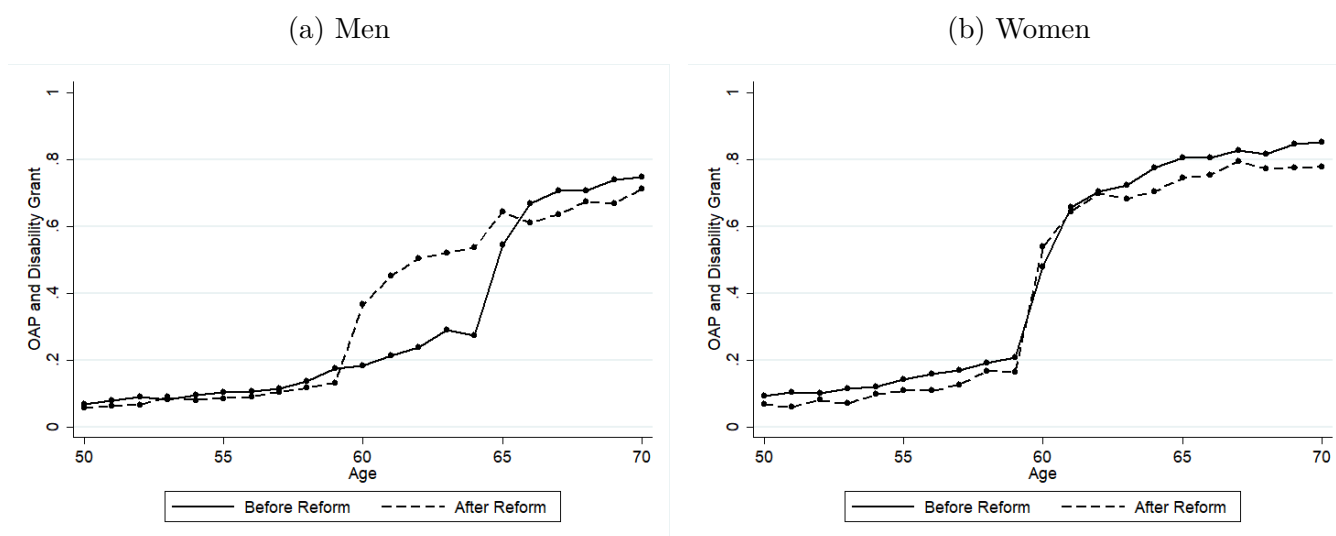
subject to two criteria: (1) an age threshold, where the recipient has to be older than a certain age; and (2) a means-test, where the recipient's earnings (and overall wealth) have to be lower than a certain threshold. There are no requirements with respect to past contributions or past employment, so it can be characterized as a non-contributory, means-tested pension scheme. Since the extension of the grant to the Black population, the age criterion for women has always remained fixed at 60 years old. Instead, the threshold for men has been set at 65 until July 2008, and then gradually lowered to 60 in the two following years. We present the reforms in the age threshold and the amount paid by the pension over time in Table 2.A.1. The amount of the pension in nominal terms has been constantly increased since 1993 to keep up with inflation. The real amount of the grant is fairly constant, around R 1000 (2010), which is equivalent to approximately 200 \$ PPP *per month*. This is a remarkably high transfer when compared to median income (Woolard and Leibbrandt (2010)), but also to wages, as we will show in the following sections.

The means-test was always set at very high levels. Initially, the proportion of the grant paid was supposed to be a decreasing function of the recipient's income, which would reach zero at a given threshold. However, it was always understood in a "binary fashion" (i.e. an individual either makes the means-test or he/she does not), hence with a 100% marginal tax rate (Ranchhod (2006)). Since the reform, the means-test has been relaxed further. In the next Section, we show how it compares to monthly earnings in the formal and informal sector. It is not clear to what extent the administration can verify applicants' income, and probably relies for the most part on what is self-reported at the application stage (Case and Deaton (1998), Ranchhod (2006)). Importantly, private occupational pensions enter the computation for the means-test, and this is likely to be an important discriminating factor. Together with proof of income, individuals have to provide proof of their private pension (if any) and assets.<sup>3</sup> As informal jobs, by definition, do not provide any proof of income, we consider informal labor supply decision (for workers already in the informal sector) to be independent from the means-test.

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<sup>3</sup>This is clearly stated in the guidelines published by the South African Government: <http://www.gov.za/services/social-benefits-retirement-and-old-age/old-age-pension>

Figure 2.1: Share of People Receiving the *Old Age Pension* or *Disability Grant* by Age, **Before and After** Pension Reform



*Note:* These graphs plot the share of individuals receiving the *Old Age Pension* or the *Disability Grant* within each age bin, for men and women separately. The solid line is for the years before the reform (2002–2007), while the dotted line is for the years after the reform (2010–2015).  
*Source:* Authors’ calculations on GHS.

An important, complementary feature of the *Old Age Pension* is the *Disability Grant* (DG). This transfer is received by individuals who have not yet reached the age threshold for the public pension, and carry some sort of physical and/or mental disability.<sup>4</sup> The *Disability Grant* has exactly the same means-test as the OAP, and pays the same amount. Moreover, it is automatically converted into the pension once the appropriate age is reached (Abel (2013)). The two transfers cannot be received simultaneously, as one condition to get the *Disability Grant* is to be younger than the pension age threshold. Therefore, they have to be thought of as complementary, in that the DG provides some form of income support to people who cannot work before they reach pension age. Figure 2.A.1 gives the number of beneficiaries of the OAP and *Disability Grant*, and their sum, over time.<sup>5</sup>

In Figure 2.1, we plot the share receiving the *Disability Grant* and the *Old Age*

<sup>4</sup>Information about the *Disability Grant* can be found here: <http://www.gov.za/services/social-benefits/disability-grant>

<sup>5</sup>This figure is calculated on administrative data from the Social Security administration (SOCPEN database) found in the yearly National Budget Reviews. As from 2008 when retirement age is lowered, there is a large increase in the number of OAP recipients. This positive trend is partly offset by a decrease in the number of disability grants, because the two transfers are not cumulative. The total number of beneficiaries increases during the period of the reform but not at a faster (nor slower) pace than the previous years, because the faster growth in the number of pensions paid is offset by a decline in the number of disability grants.

*Pension* by age in the years before and after the reforms in age eligibility (*Source*: General Household Survey). The percentage of men aged 60 to 64 receiving the DG before 2008 was already fairly high before the reform. However, we do observe a large spike between 60 and 64 for the post-reform years. Consistently, this only occurs for men and not for women, for whom everything remains virtually unchanged. Overall, the share of people receiving the OAP is very high, reaching peaks of over 80% for women older than 65, and around 70% for men of a similar age. These rates are even higher when excluding the White and Indian population, who are generally less covered by social grants (Figure 2.A.2.) We also observe that, in proportion, men receive the pension less than for women, which is true both before and after the equalization of the age threshold. The explanation presumably lies in the higher prevalence of formal employment among men, who are therefore more likely to be covered by occupational pension schemes rather than the public pension.

## 2.3 Data and Descriptive Statistics

### 2.3.1 Data

Our analysis relies on the use of two datasets: the *General Household Survey* (GHS) and the *Post-Apartheid Labour Market Series* (PALMS). The GHS is an annual, nationally representative survey that covers the period 2002 to 2015, run by the South African statistical agency (STATSA). It includes information on the *Old Age Pension*, the *Disability grant*, but only some basic information about employment and salary. Its labor market section is not detailed enough for the purpose of our analysis, so we only use this dataset to document the effect of the reforms on the share of people receiving the *Old Age Pension*.

The PALMS consists of several appended cross-sections from 1994 to 2015. Over our period of interest (2002–2015), the PALMS is made up of two similar surveys: the *Labour Force Survey* (2002–2007) and the *Quarterly Labour Force Survey* (2008–2015). These surveys are generally considered to be of high quality, and are the main source of labor market information in South Africa. They are also run by STATSA, but



the PALMS dataset, which combines them in a coherent way, is put together by researchers at *Data First* of the University of Cape Town.<sup>6</sup> From 2000 to 2007, the LFS was bi-annual, i.e. interviews were carried out in March and September. As from 2008 to now, together with a change in the sampling framework, the QLFS began to be run each quarter. Overall, both the LFS and the QLFS have much more detailed information on employment, informality and wages (except for the period 2008-2009, where salary information was not asked). The sampling frame is the same as the GHS. The disadvantage of the PALMS is that information about social transfers (including the public pension) is asked only to individuals who are either inactive or unemployed. This makes it impossible to calculate accurately the share of people receiving the OAP on the PALMS, given that employed people can in theory also access it. The advantage of using the PALMS, rather than the original waves from the LFS and the QLFS, is that variables are coded consistently over the whole period, and sampling weights are adjusted to be coherent over time, but the underlying data is the same. To ensure the maximal comparability across the GHS and the PALMS, we exclude the initial waves of the PALMS and focus only on the period 2002–2015.<sup>7</sup>

### **2.3.2 Informality Definition**

Given its detailed labor market section, PALMS data allows for an accurate measurement of informal employment. Throughout the paper, we employ the most objective, and most conservative, measurement of informality. For employees, we consider informal workers those who work without a written contract. The advantage of this measure is its lack of ambiguity, as it is easily known to the worker who answers the survey questions.<sup>8</sup> For self-employed workers, who, by definition, do not have a work contract, we use information about business registration. These are both standard ways to measure informality in the literature. Throughout the paper, we

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<sup>6</sup>Detailed information about how the PALMS 3.2 was put together can be found here: <https://www.datafirst.uct.ac.za/dataportal/index.php/catalog/434>

<sup>7</sup>Moreover, it has been reported that the initial waves of the LFS significantly over-measured informal employment. See [Kerr and Wittenberg \(2015\)](#) for discussion.

<sup>8</sup>Questions about whether the employer pays social contributions may be more difficult to re-collect, or might not be known to the worker.

use the terms informal employment and informal sector interchangeably, but the definition is always based on the presence/absence of work contracts (employees) and business registration (self-employed). Importantly, earnings are measured in gross amounts, before tax and deductions. This is irrelevant for informal workers (for whom net wage=gross wage), and low-paid formal workers, but it certainly enhances the differences between formal and informal wages, in particular at the top of the wage distribution where taxes and social contributions are more relevant.

### **2.3.3 Descriptive Statistics**

Men aged 60–64 make-up between 1.5%-2% of the working-age population (15–65)<sup>9</sup>, and account for a similar share of working-age employment. Around half of employed elderly male were in the informal sector between 2002 and 2007, while this share decreased to one fourth after the reform. The importance of private pension schemes is shown in Figure 2.A.3, where we plot the joint probability to be employed and have an occupational pension in the years before the reform. A significant share of people contributes to some type of pension fund (through the employer). More importantly, this probability drops discontinuously at age 60, suggesting that workers tend to retire at that age. In the next sections, we show that not accounting for these private pension schemes leads to a significant over-estimation of the effects of the *Old Age Pension*, and to drastically different results about the heterogeneous impacts on formal and informal employment.

Table 2.A.2 gives average characteristics by labor market status. The median monthly salary for men aged 60–64 before the reform was R 4462 (R 23 per hour) in the formal sector, and R 1373 ( R 7 per hour) in the informal sector. After the reform, it was R 5613 (R 30 per hour) in the formal sector, and R 2004 (R 12 per hour) in the informal sector (in 2010 Rand). The amount of the OAP is around R 1000, and roughly stable over time in real terms (Table 2.A.1). Thus, it can be considered a large amount when compared to median monthly salaries in the informal sector (slightly below the median in the pre-reform period). These orders

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<sup>9</sup>This share grows slightly during the period of our study (2002–2015), as life expectancy, which was well below 60 over the period in South Africa, increased.

of magnitude are important to interpret the results of the next sections.

Working hours, on average, are similar between the formal and informal sector, where people usually work around 45 hours, with a slight decline over the period. However, this masks a large heterogeneity in the informal sector, where many workers are employed part-time (15-20%) at less than 30 hours a week. On the contrary, part-time is very rare in the formal sector, where less than 5% work less than 30 hours. This suggests that one of the advantages of informal work might be its higher flexibility. In Figure 2.A.4, we show the distribution of occupations in informal employment by categories of age, for men and women respectively. Elementary occupations make up the most of the informal employment of elderly men. Overall, informality among the elderly appears to be relatively similar to that of their younger counterparts, in the sense that is similarly distributed across occupations and sectors.

The position of the means-test, after the reform, with respect to earnings in the formal and informal sectors is given in Figure 2.A.5. We focus on the period after the reform as this is the relevant one to our analysis, and for which we have better information. Moreover, qualitative evidence suggests that the means-test is likely to be more strictly applied in later years, than in the earlier stages of the program. As we can see from Figure 2.A.5, the monthly value of the means-test is located around the median of the distribution of formal monthly salaries, and this remains true across years, despite changes in the nominal level. Importantly, the means-test is located in an area where there is a large overlap between formal and informal wages, suggesting that, at that level of wages, there are (potentially) corresponding jobs in the informal sector that pay a similar amount.

## **2.4 Conceptual Framework**

We use a simple model of sectoral choice and labor supply to help formalize the potential effects of an unconditional pension reform on labor market outcomes. The purpose of this simple framework is to elaborate a few predictions on the behavior of individuals faced with an exogenous non-labor income shock. The set-up is a classic

leisure-consumption trade-off, where individual  $i$  chooses how much to consume ( $C$ ), how much to work ( $l = T - L$ ) and in which sector to work ( $k \in \{F, I\}$ ). For simplicity, utility is given by  $u_i(C, L, k) = \alpha_i \ln(C) + (1 - \alpha_i) \ln(L) - \mu_i(k)$ ; people are all endowed with the same amount of time,  $T$ , and are faced with wages that may differ for each individual and across sectors,  $w_{i,k}$ . These wages are fixed, which at least in the short to medium term, is a reasonable assumption. In the absence of pension, non-labor income,  $m$ , is zero. The reform consists in offering  $m > 0$  to all individuals whose labor earnings ( $w(T - L(w, m))$ ) are below a certain threshold  $\bar{W}$ . We further assume working in the formal sector implies a negative utility component ( $\mu_i(F) = \mu_i$ ) which is not encountered when working in the informal sector ( $\mu_i(I) = 0$ ). This is meant to reflect some type of fixed cost to enter the formal sector, while we model the informal sector as “free entry”. The introduction of this parameter does not qualitatively change the predictions of interest of this simple framework, and it is an analytically simple way to model the constraints vs. choice debate in the literature on informality. A more detailed treatment of this framework is given in Appendix [2.A.1](#).

**Individual labor supply** Prior to  $m > 0$  being implemented, agents allocate in the formal or in the informal sector. These are people for whom, respectively,  $\mu_i < \alpha_i \ln \frac{w_{i,F}}{w_{i,I}}$  (formal-sector workers), and  $\mu_i > \alpha_i \ln \frac{w_{i,F}}{w_{i,I}}$  (informal-sector workers). In words, people chose to work in the formal sector when the relative monetary benefits of doing so (as opposed to working in the informal sector), weighted by their preference for consumption (the utility associated with this gap in expected earnings), is larger than the (utility) costs associated with it. The difference in the choice of sector is driven by (i) differences in  $\frac{w_{i,F}}{w_{i,I}}$ , (ii) differences in  $\mu_i$ , or (iii) differences in  $\alpha_i$  (of course, several mechanisms might be simultaneously at play).

As a result of the pension reform, the non-labor income  $m$  is given to everyone choosing to work in the informal sector  $I$ , and to people earning less than  $\bar{W}$  in the formal sector. The condition for choosing to work in the formal sector rather than the informal one becomes more demanding as the pension reform makes the informal sector more attractive as compared to the formal one (since the means-test does not

concern revenue from informal work).

Individuals who would have chosen to work in the informal sector absent the pension either keep working in the informal sector and decrease their hours, or drop out of the labor force when receiving the pension. In other words, the effect on the labor supply of those individuals will be that of an income effect. The magnitude of the drop in the labor supply, relative to its baseline level, is  $\Delta_\alpha l(m) = -\frac{1-\alpha_i}{\alpha_i} \frac{m}{w_{i,I}T}$ . This expression implies that workers with lower hourly wages will react more to the income shock.

Individuals who would have chosen to work in the formal sector absent the pension split in two subcategories that might react differently to the reform. The first are individuals whose optimal labor income in the absence of the pension,  $w_{i,F}l^*(0)$ , is smaller than the means-test threshold. Because  $m$  is alleviating the budget constraint, it is making the formal sector's wage premium relatively less important, thereby potentially inducing switching from the formal sector to the informal sector; in other words, the magnitude of  $\frac{w_{i,F}}{w_{i,I}}$  might not be enough to overturn the costs of working in the formal sector anymore. If individuals do not switch, their formal labor supply will nonetheless decrease, as a result of the income effect created by the pension. Second, individuals whose optimal labor supply in the absence of the pension is located above the means-test will either (i) be left unaffected by the reform, or (ii) decrease their formal labor supply to locate below the means-test, or (iii) switch to the informal sector (and decrease their labor supply). High levels of  $w_{i,F}$  will make  $m$  negligible enough that labor supply (and sector choice) is unaffected, with no take-up of the pension. For lower levels of  $w_{i,F}$ , it is the ratio between  $w_{i,F}$  and  $w_{i,I}$  that determines whether individuals will decrease their formal labor supply, or switch to the informal sector and decrease their labor supply. This implies that the share of individuals who would switch to the informal sector will largely depend on the counterfactual wage that formal workers would have in the informal sector.

**Aggregate labor supply** In summary, we expect that the formal labor supply curve should move unambiguously downward. Whether it should be affected at all, remains to be investigated. Even with no change at the extensive margin, we

would still expect a response in terms of hours worked. Because of the means-test threshold, it could be that some individuals work a few hours less, so as to qualify for the means-test. For workers at the lower-end of the formal wage distribution, where agents are anyway eligible to the pension as they are under the means-test, we would also expect a decrease in the number of hours worked. Failure to observe either response could be because the pension amount is not sufficient to make people drop out of the labor force, and because hours would not be able to adjust, say if contracts cannot easily be changed.

Instead, the direction in which the aggregate informal supply curve will react is ambiguous. If there is no switching from formal employment, then any drop in the informal employment rate is entirely attributable to the income effect for informal workers. If there is switching from formal employment, any drop in the informal employment rate observed at 60 would be the sum of a drop in the (informal) labor supply of individuals who would have worked informally in the absence of the pension, and of an increase in the informal labor supply of individuals who would have worked formally in the absence of the pension, but switch to informal work as a consequence of it.

**Disentangling the effect of wages** The distributions of wages in formal employment and in informal employment are very different (see Figures 2.A.6 and 2.A.7). Absent any substitution from formal to informal employment, differences between the labor supply reactions of both sectors are likely to be driven by wage differences: for smaller expected hourly wages, the non-labor income  $m$  that the pension represents has a relatively larger income effect. However, to control for these differences, we can compare the formal and informal labor supply responses at similar level of hourly wages. The labor supply response of informal workers (A), and formal workers (B) are respectively equal to  $\Delta_{\alpha_A} l(m) = -\frac{1-\alpha_A}{\alpha_A} \frac{m}{w_{A,I}}$  and  $\Delta_{\alpha_B} l(m) = -\frac{1-\alpha_B}{\alpha_B} \frac{m}{w_{B,F}}$ . At the same level of wages (i.e. for  $w_{A,I} = w_{B,F}$ ), the differences in labor supply responses are thus attributable to differences between informal and formal labor markets; either with regards to the characteristics of workers who select into them (the preference parameter  $\alpha$  and  $\mu$  in our framework), or with regards to intrinsic

characteristics of the jobs in question. We are able to take this prediction to the data as the distribution of hourly wages in the informal and formal employment overlap, although only partly. We go back to the empirical challenges of this heterogeneity analysis in the next section.

## 2.5 Empirical Analysis

### 2.5.1 Identification Strategy

In order to capture the labor market effects of the *Old Age Pension*, we make use of the latest reform in eligibility, which only directly affected men. In our estimations, we employ a “modified” Regression Discontinuity Design (RDD), conceptually similar to the “diff-in-disc” estimator proposed in [Grembi, Nannicini and Troiano \(2016\)](#). In this context, the main concern is to avoid bias from other pension schemes with a similar age threshold, which we show to be sizable. With this in mind, we extend the RDD framework to incorporate the time variation in the age threshold introduced by the reform. Therefore, rather than simply estimating the jump at a given threshold as in a traditional RDD setting, we estimate the difference in the jump before and after the reform. This relaxes considerably the “classic” RDD assumption that, in the absence of the treatment, there should be no discontinuity at the cut-off point. Instead, by performing this estimation, the requirement for identification is that the discontinuity at the threshold would have remained the same in the absence of the reform. This design can be thought of as combining both a “Difference-in-Differences” (DID) and an RDD, but requires significantly weaker assumptions than any of the two methodologies applied independently.<sup>10</sup> All our estimations are run on the subsample of Black and Coloured individuals, during the years 2002 to 2007 and 2010 to 2015. We exclude the reform years (2008–2009), and only focus on the before/after period.

As we also want to derive the magnitude of the bias in the simpler, cross-sectional RD estimator, we also compare the estimates in both strategies. This also allows us to have some results for women, for whom the threshold is not reformed. Formally,

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<sup>10</sup>The assumption of the DID is that the affected and unaffected age-groups would have evolved in the same way over the period in the absence of the reform.

we begin by estimating the following equations, separately for men and women, before and after the reform:

$$Y_{i,t} = \delta_t + f(\text{age}_i) + f(\text{age}_i) \times \text{Age}_{(60+)} + \beta_{RD} \text{Age}_{(60+)} + \varepsilon_{i,t} \quad (2.1)$$

where  $Y_{i,t}$  is the outcome of interest.  $\delta_t$  indicates year effects.  $f(\text{Age}_i)$  is a function of age, and we test the sensitivity of our results to both a linear and quadratic function. Following [Gelman and Imbens \(2017\)](#), we avoid the use of higher-order polynomials.  $\text{Age}_{(60+)}$  is a dummy variable indicating whether the individual is older than 60, which is the cut-off point after the reform.  $\beta_{RD}$  is the discontinuity estimated with the classic RD framework, of which we obtain four values: before and after the reform, for men and for women.

To incorporate the reform in the age threshold into the RD strategy, we modify Equation 2.1 in the following form:

$$Y_{i,t} = \delta_t + f(\text{age}_i) + f(\text{age}_i) \times \text{Age}_{(60+)} + f(\text{age}_i) \times \text{Post}_t + f(\text{age}_i) \times \text{Age}_{(60+)} \times \text{Post}_t + \beta_1 \text{Age}_{(60+)} + \beta_{DiDRD} \text{Post}_t \times \text{Age}_{(60+)} + \varepsilon_{i,t} \quad (2.2)$$

where the difference is that we interact the function of age with the  $\text{Post}_t$  and  $\text{Age}_{(60+)}$  variables.  $\text{Post}_t$  is a binary variable equal to 1 for the years after the reform (2010–2015) and equal to 0 for the years before (2002–2007). In this way, we allow for four different functions of age, on both sides of the threshold and before/after. We also allow for two different discontinuities:  $\text{Age}_{(60+)}$ , and  $\text{Post}_t \times \text{Age}_{(60+)}$ .  $\beta_{DiDRD}$  captures the before/after difference in the discontinuity at age 60. It is important to underline that we observe age as a discrete variable, so our design suffers from the limitations of an RD with a discrete forcing variable in terms of inference ([Lee and Card \(2008\)](#), [Lee and Lemieux \(2010\)](#), [Kolesár and Rothe \(2018\)](#)). As suggested by [Kolesár and Rothe \(2018\)](#), we do not cluster by the running variable (i.e. age). Instead, when running Equation 2.2, we obtain robust standard errors by clustering at the race-cohort level. In this way, we want to account for the serial correlation arising from observing some of the same cohorts over time at different points of the



age profile.

One of the advantages of this estimation is that it offers a practical solution to “age heaping”, meaning the tendency among survey respondents to round age to the closest multiple of 5 or 10, as already pointed out by [Ranchhod \(2006\)](#). In our setting, this does not pose a problem unless heaping is more or less severe before or after the reform, which can be checked easily by looking at the change in density. More generally, under the assumptions stated before, comparing our estimate for  $\beta_{RD}$  and  $\beta_{DiDRD}$  will give us the effect of the pension “purged” of the bias from other pension schemes, and age heaping. In the results Section, we discuss the sign and magnitude of this bias.

Another concern is the possible presence of anticipation effects. This is a common problem when dealing with age as a forcing variable. Individuals are aware of the age threshold, and can anticipate or postpone their retirement decision before reaching the age threshold. Given that we focus on the discontinuity at the threshold, our estimation ignores changes that may occur before the threshold as a response to the pension reform. With this in mind, our estimates have to be understood as capturing the anticipated income effect of receiving the pension (relative to not receiving it) rather than as the “absolute” effect of the pension reform. In theory, the effect of anticipation could go either way: if individuals younger than 60 anticipate their retirement decision because of the lower age threshold, then our estimates will be a lower bound. Instead, if individuals postpone their retirement to reach the public pension age, then we would overestimate the effect of the pension. However, the gender dimension of the reform allows to tackle this openly. As women are not affected by the change, we can compare men and women at unaffected age values close to the threshold to measure anticipation. Overall, we do not find any evidence of employment responses occurring before eligibility kicks in.

## **2.5.2 Results**

The results of the RD estimation are presented in [Table 2.1](#), and those of the “diff-in-disc” in [Table 2.2](#). Graphical evidence of the drop in total employment and by

sector is presented in Figure 2.2. Before the reform, we estimate a significant and large drop in formal employment at age 60, while informal employment is smooth around the threshold. As men were only receiving the pension from age 65 before the reform, these estimates cannot be the result of the *Old Age Pension*. After the reform, when men are “treated” as from age 60, the drop in employment is more than double that of the pre-reform period, coming in equal parts from formal and informal employment. We observe qualitatively similar evidence for women, who also experience a sizable drop in employment at 60, but constant over the period, again split in roughly equal parts from formal and informal employment. This is in line with the fact that they are not affected by the reform, and are always eligible to the pension from the same age.

Table 2.1: Old Age Pension and Employment, RDD Results, Quadratic Fit 50–70, PALMS

	Before Reform			After Reform		
	(1) Employed	(2) Informal	(3) Formal	(4) Employed	(5) Informal	(6) Formal
<b>a. Men</b>						
<i>Age</i> <sub>(60+)</sub>	-0.0278 (0.0195)	0.0083 (0.0151)	-0.0361** (0.0170)	-0.0907*** (0.0115)	-0.0427*** (0.0084)	-0.0480*** (0.0108)
Mean <i>Y</i> at Age 59	0.41	0.18	0.23	0.46	0.15	0.31
Observations	47,034	47,034	47,034	93,030	93,030	93,030
R-squared	0.1037	0.0214	0.0668	0.1521	0.0242	0.1009
<b>b. Women</b>						
<i>Age</i> <sub>(60+)</sub>	-0.0720*** (0.0136)	-0.0427*** (0.0114)	-0.0293*** (0.0096)	-0.0536*** (0.0090)	-0.0360*** (0.0067)	-0.0176** (0.0074)
Mean <i>Y</i> at Age 59	0.25	0.15	0.09	0.31	0.14	0.17
Observations	67,803	67,803	67,803	137,631	137,631	137,631
R-squared	0.1071	0.0539	0.0460	0.1251	0.0419	0.0689

*Note:* \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. This table gives the results of Equation 2.1 with a **quadratic function**, for men (upper panel), and women (lower panel), on the age window 50–70. The sample is limited to Black and Coloured men and women. We only report the coefficient of interest,  $\beta_{RD}$ . The dependent variables are binary variables for: (1) employed, (2) informally employed, (3) formally employed, such that (1)=(2)+(3), and (4)=(5)+(6). *Mean Y at Age 59* refers to the value of the dependent variable at age 59 in the years after the reform. Robust standard errors in parentheses.

*Source:* Authors’ calculations on PALMS 3.2 (2002–2007 and 2010–2015).

Table 2.2: Old Age Pension and Employment, Quadratic Fit 50–70, Diff-in-Disc, PALMS

	Employed	Informal	Formal	
	(1)	(2)	Extensive (3)	Intensive (4)
<b>a. Men</b>				
$Post \times Age_{(60+)}$	-0.0639** (0.0260)	-0.0511** (0.0195)	-0.0128 (0.0261)	-0.4621 (1.2691)
Mean $Y$ at Age 59	0.52	0.14	0.38	13.9
Observations	140,064	140,064	140,064	140,064
R-squared	0.1365	0.0264	0.0956	0.0843
<b>b. Women</b>				
$Post \times Age_{(60+)}$	0.0183 (0.0131)	0.0067 (0.0134)	0.0116 (0.0137)	0.4686 (0.5951)
Mean $Y$ at Age 59	0.34	0.12	0.21	6.5
Observations	205,434	205,434	205,434	205,434
R-squared	0.1242	0.0463	0.0743	0.0656

*Note:* \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . This table gives the results of Equation 2.2 with a **quadratic function**, for men (upper panel) and women (lower panel) on the age window 50–70. The sample is limited to Black and Coloured men and women. We only report the coefficient of interest,  $\beta_{DiDRD}$ . The dependent variables are binary variables for: (1) employed, (2) informally employed, (3) formally employed, such that (1)=(2)+(3), and (4) hours in formal employment. *Mean  $Y$  at Age 59* refers to the value of the dependent variable at age 59 in the years after the reform. Robust standard errors clustered by race-cohort group.

*Source:* Authors’ calculations on PALMS 3.2 (2002–2007 and 2010–2015).

Importantly, these results show that the “simple” RD results suffer from a significant bias. This bias is concentrated on formal employment because this is where the jobs with private pension schemes are. Ignoring the confounding effects from other pension schemes leads to mistakenly attribute a negative effect of the *Old Age Pension* on formal employment. Overall, this brings to an over-estimation of the total employment effect of the pension by a factor between 1.5 and 2.<sup>11</sup> Intuitively, the coefficients suggest that the size of the bias is roughly the same for women, but

<sup>11</sup>The RD overestimates the employment effect by a factor of 2 when using a linear function on the 55-64 window, see Table 2.A.3.

this cannot be tested directly as the age threshold for women is left unchanged.<sup>12</sup>

When we correct for this bias, by employing the estimator of Equation 2.2, the point estimate on total employment decreases significantly (Table 2.2). Not surprisingly, our estimate of the employment effects of the pension on direct recipients is more conservative than previous studies, which found effects around twice as large (Ranchhod (2006), Ambler (2016)). The effect on informal employment is unchanged from the previous estimation, between -4 and -5pp., which is relatively large drop: more than a third of informal workers leave their jobs as they become age-eligible to the pension. Looking at Table 2.1, we can speculate that the effect is of similar magnitude for women, although the lack of a reform does not allow us to control for potential confounders.

The effect on formal employment is much smaller, and insignificant, but the point estimate is not zero. We show in the next section that some formal workers at the very bottom of the wage distribution stop working. Formals workers also do not seem to significantly respond at the intensive margin, as average working hours in the formal sector stay stable when men become eligible to the *Old Age Pension* (Table 2.2, Column 4).<sup>13</sup> This partly contradicts the prediction of our conceptual framework in Section 4, which suggested some kind of adjustment by formal workers. However, this prediction was based on the assumption that formal workers can freely adjust their hours, which is unlikely to be the case. If formal jobs are not flexible in terms of working hours, as the absence of part-time jobs would suggest, this could explain the lack of an adjustment at both the extensive and intensive margin.

Overall, the results match the prediction that there should be a net decrease in employment. This decrease comes almost entirely from informal employment, while formal workers do not significantly respond. Importantly, we do not observe a “perverse” effect at play where workers reallocate from formal to informal employment. This would suggest that those in formal employment strictly prefer it to informal

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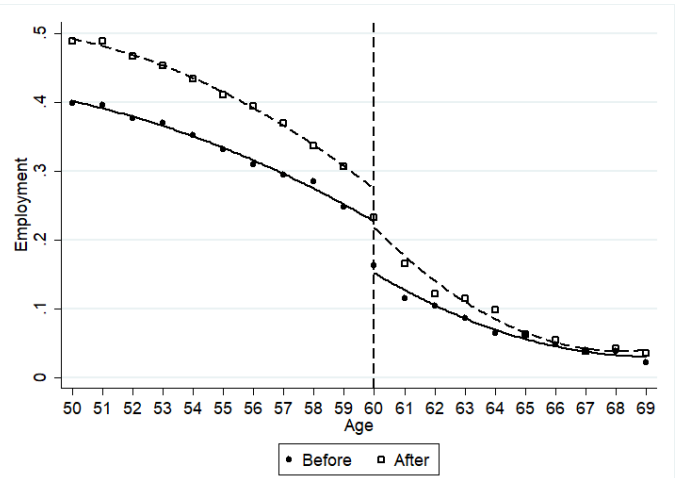
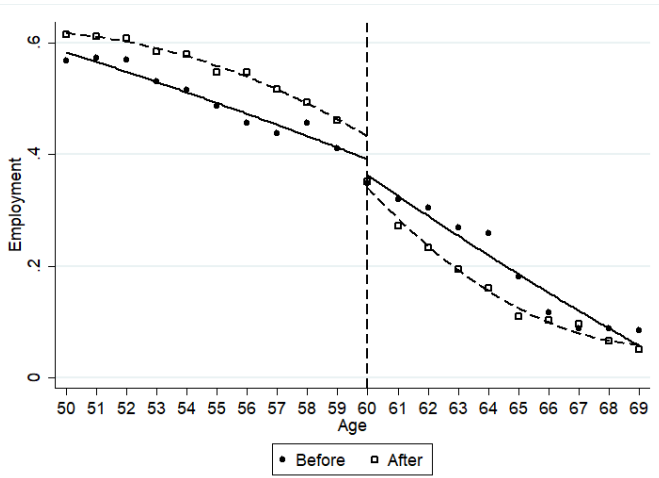
<sup>12</sup>The drop in total employment is roughly of the same magnitude as for men, and split in equal parts between formal and informal. If, as for men, there is little or no effect of the pension on formal employment, we can also speculate that the RD estimation over-estimate the effect on employed by a similar magnitude.

<sup>13</sup>Estimating an effect on the intensive margin of informal employment is more complicated because of the large extensive margin response.

Figure 2.2: Total, Formal and Informal Employment, Before and After, Men & Women

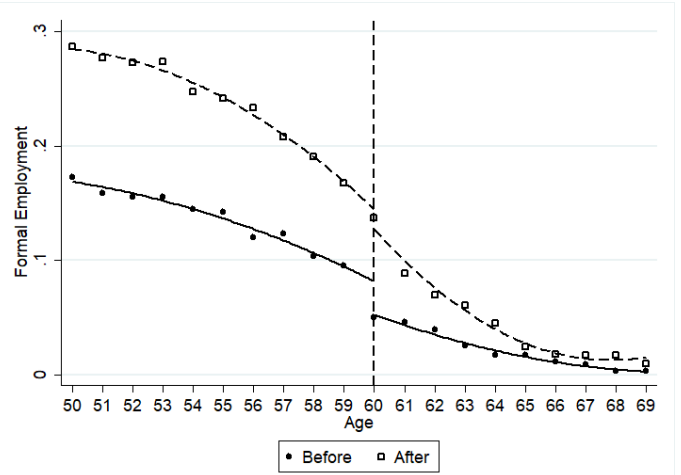
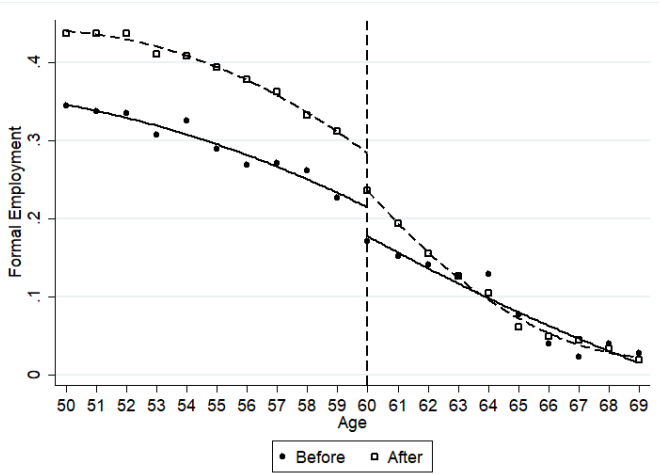
(a) Men: Employment

(b) Women: Employment



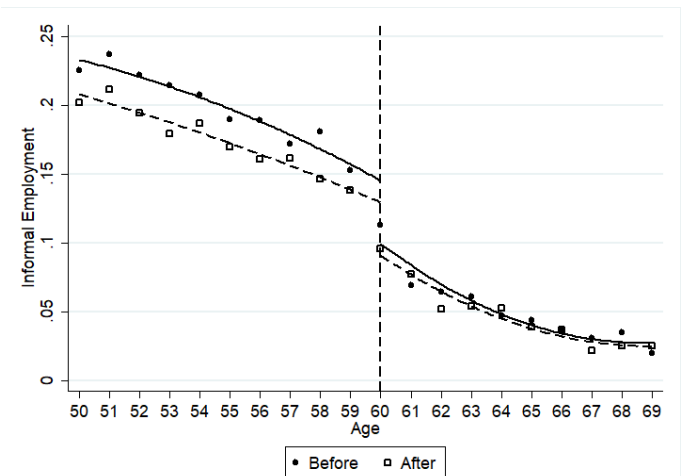
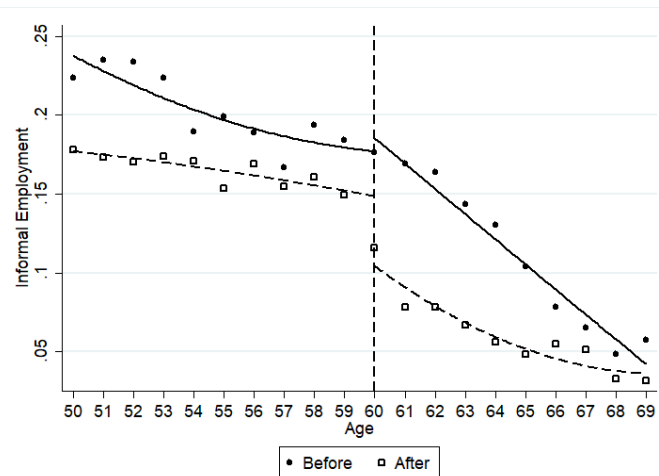
(a) Men: Formal Employment

(b) Women: Formal Employment



(a) Men: Informal Employment

(b) Women: Informal Employment



Note: These graphs plot the total employment rate, formal employment rate, and informal employment rate by age for men, in panel (a), and women, in panel (b). Formal and informal employment add up to total employment. The black dots give the mean at each age value before the reform, while the hollow squares after the reform that made men eligible as from age 60. Women have always been eligible as from age 60. A quadratic function is fitted on both sides of the threshold. Source: Authors' calculations on PALMS 3.2 (2002–2007 and 2010–2015).

work, for example because their counterfactual wages in informal jobs would be significantly lower, so that this change in payoffs across sectors is not large enough to generate any switching. Alternatively, one could posit that there may be barriers that impede this re-allocation across sector. We have assumed that workers can simply enter the informal sector, by modelling it as a “free-entry” sector as is standard in the literature, but this might not be the case.

### **2.5.2.1 Heterogeneous Effects by Wage**

We now look at how eligibility to the pension has affected workers at different points of the wage distribution. The goal is to test the prediction that the labor supply response of workers should be larger at lower levels of hourly wages. Moreover, we also want to examine differential responses in formal and informal employment *at the same level of wages*. As wages in the informal sector are, on average, lower than in the formal sector, this observed heterogeneity may simply derive from the fact that the OAP is a relatively larger income shock for informal workers.

Ideally, we would estimate the discontinuity at 60 of the *conditional* labor supply at various levels of potential wages. However, only realized wages are observed, i.e. for people who are employed. Alternatively, as outcome variables, we construct different indicator variables equal to 1 if the individual is observed in employment at given levels of wages, and 0 otherwise. The “diff-in-disc” estimator is not well suited for the purpose of this analysis, as the distribution of real wages changes over time.<sup>14</sup> Differences in the wage distribution over time, weighted by the discontinuity at baseline, would bias our estimates. However, under the assumption that the distribution of wages around the threshold is continuous, and in the absence of any discontinuity prior to the reform, the RD estimator causally identifies the discontinuity in the conditional labor supply (scaled by the density of wages). We show this formally in Appendix 2.A.3.<sup>15</sup> As there was no discontinuity pre-reform in

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<sup>14</sup>Because of growth in real wages over time, the identification assumption that the magnitude of the discontinuity would have stayed the same over time *at each level of wages* is unlikely to hold. For more details see Appendix 2.A.3.

<sup>15</sup>The intuition is that the joint density of labor supply and wages is the product of two continuous densities: the conditional labor supply and the distribution of wages; if the latter is continuous, the discontinuity in the joint density identifies the discontinuity in the conditional labor supply.

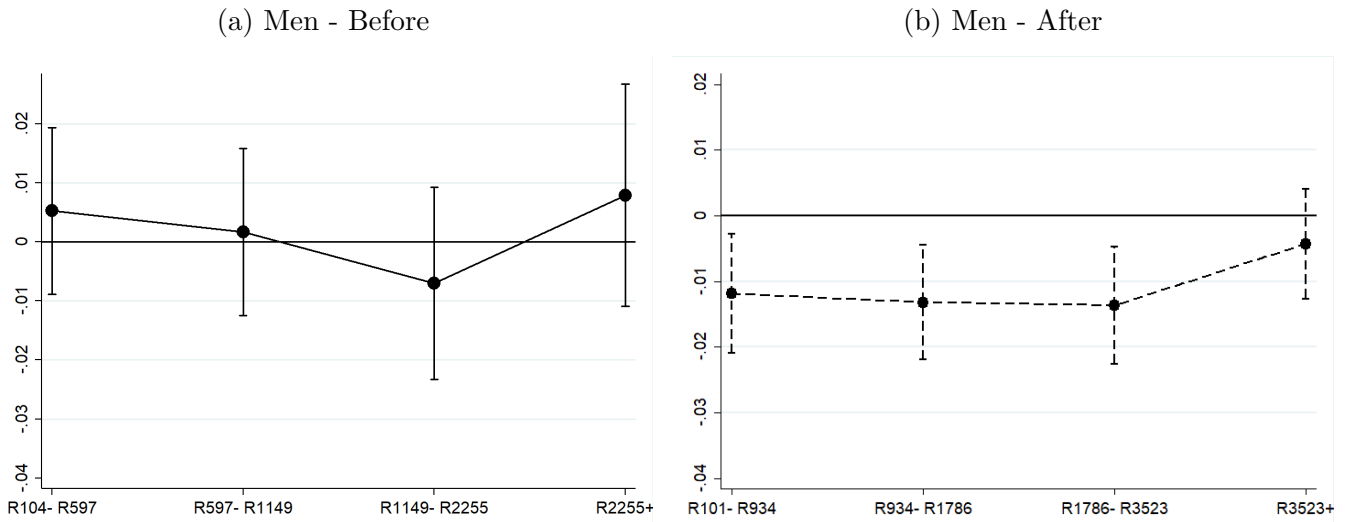
informal employment, informal labor supply responses by wage can be interpreted directly; with some caveats, we argue one can also interpret the effects on formal employment by wage.

To obtain a counter-factual of the distribution of wages absent the pension reform, we focus on the closest unaffected age group. The wage distribution of 55–59 years old is virtually identical to that of 60–64 years old before the reform (Figures 2.A.6 and 2.A.7). Therefore, we use the distribution of *informal wages* for 55–59 years old to construct quartiles, separately before and after the reform, and estimate heterogeneous effects by wages.

The extensive margin response of informal employment by wage quartile is plotted in Figure 2.3 for men, and in Figure 2.4 for women. The intuitive prediction of our simple framework is that the effect should be stronger for lower levels of wages, but this is not what we observe. For men, the negative effect on informal employment is constant up to the third quartile of the distribution. Only in the top quartile of the informal wage distribution the effect seems to converge to zero. We observe very similar patterns for women, keeping in mind that they are treated at 60 in both periods. Indeed, extensive margin response by wage is quantitatively and qualitatively similar, with an effect of similar size across the wage distribution, both before and after.

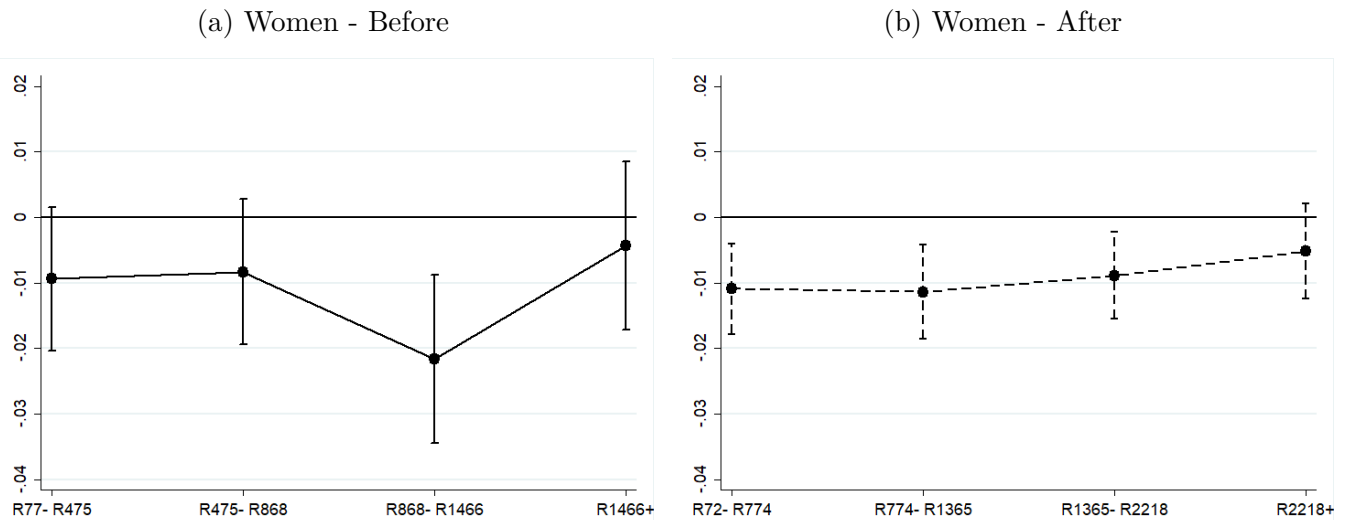
Importantly, the magnitude of the response is the same even for those workers whose full-time equivalent monthly salary, i.e. what they would make in a month given their hourly wage and 43 hours working week, is larger than the amount of the pension (R 1000). In other words, some informal workers are willing to give up more earnings than what they get with the pension. One interpretation of the stability of the effect with respect to hourly wage is that the pension might relieve a subsistence-level constraint. A given share of workers at each point of the informal wage distribution cannot afford not to work. Once this constraint is lifted, they stop working entirely. This would have potentially important implications in terms of welfare, as it suggests that any positive effect on utility might be greater than just the income effect.

Figure 2.3: Effect on Probability to be **Informally** Employed by Quartile of **Informal** Hourly Wage, Quadratic Fit 50–70, RDD, Before and After, Men



*Note:* These graphs plot the coefficients of Equation 2.1 on the probability to be informally employed within each quartile of the informal hourly wage distribution before and after the OAP reform. Quartiles are defined according the distribution of informal hourly wages for the 55–59 years old population in each period. The x-axis is labelled with the bounds of the quartiles for the monthly salary equivalent at a 43 hours working week. The sample includes Black and Coloured men only. *Source:* Authors’ calculations on PALMS 3.2 (2002–2007 and 2010–2015).

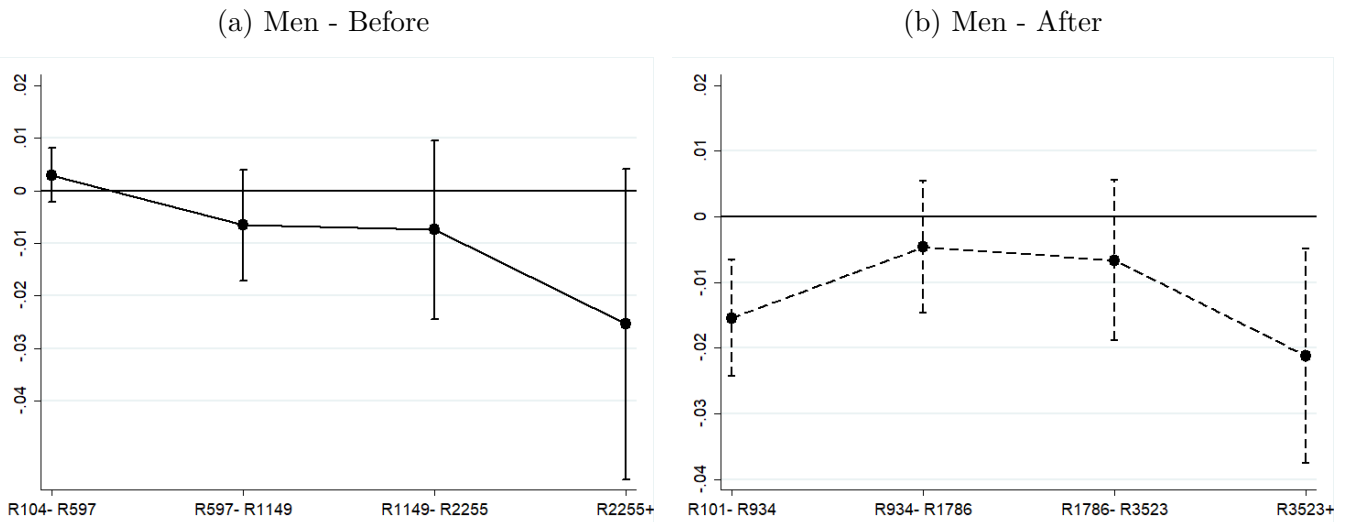
Figure 2.4: Effect on Probability to be **Informally** Employed by Quartile of **Informal** Hourly Wage, RDD, Before and After, Women



*Note:* These graphs plot the coefficients of Equation 2.1 on the probability to be informally employed within each quartile of the informal hourly wage distribution before and after the OAP reform. Quartiles are defined according the distribution of informal hourly wages for the 55–59 years old population in each period. The x-axis is labelled with the bounds of the quartiles for the monthly salary equivalent at a 43 hours working week. The sample includes Black and Coloured women only. *Source:* Authors’ calculations on PALMS 3.2 (2002–2007 and 2010–2015).



Figure 2.5: Effect on Probability to be **Formally** Employed by Quartile of **Informal** Hourly Wage, RDD, Before and After, Men



*Note:* These graphs plot the coefficients of Equation 2.1 on the probability to be formally employed within each quartile of the informal hourly wage distribution before and after the OAP reform. Quartiles are defined according the distribution of informal hourly wages for the 55–59 years old population in each period. The x-axis is labelled with the bounds of the quartiles for the monthly salary equivalent at a 43 hours working week. The sample includes Black and Coloured men only. *Source:* Authors’ calculations on PALMS 3.2 (2002–2007 and 2010–2015).

Moreover, we are interested in whether the differential response by formal/informal is the result of wages being higher in the formal sector. The question is whether formal workers, for the same wage, respond differently. In order to test this, we look at the effect of the reform on the probability to be formally employed at each quartile of the *informal* wage distribution. Indicatively, the top quartile of the informal distribution roughly begins at the median of the formal wage distribution. The effects on formal employment by wage for men are plotted in Figure 2.5, before and after the reform. Interpretation here is complicated by the effects of private pension schemes with the same age threshold. However, these are concentrated in the top quartile of the distribution, which is where we observe a large drop in formal employment, of identical size, both before and after the reform.

With this caveat, we estimate a negative effect for men at the very bottom of the wage distribution, meaning those formal workers whose wage is within the first quartile of the informal wage distribution.<sup>16</sup> Instead, workers formally employed in the 2nd and 3rd quartile of informal wages do not respond to the reform. The

<sup>16</sup>This group is only 10% of formal employment, but it is as large as the first quartile of informal workers, given the greater size of formal employment overall.

coefficient is slightly negative and insignificant, but identical in size to the response at the same quartile before the reform. The coefficient at the top quartile of informal wages (equivalent to formal workers above the median of formal wages) is large and negative, but constant across periods, and easily attributable to private pension schemes.

Importantly, the difference in the total response between formal and informal employment is coming from workers employed in 2nd and 3rd quartile of the informal wage distribution. At these level of wages, informal workers quit their jobs, while formal workers do not. This implies that the heterogeneous effect by sector is not the result of wages being higher in the formal sector, but of differential responses for the same level of wages. As mentioned before, this could be interpreted in two different ways. On the one hand, this could be the result of intrinsic characteristics of the jobs in question at those level of wages. The same worker, with the same characteristics, would react differently at the same wage in a formal or informal job. Alternatively, this can be interpreted as evidence that formal and informal workers are different across other dimensions (for example, in our simple framework, preferences for consumption), which in turn cause these differential responses.

### **2.5.3 Robustness Checks**

Regression discontinuity designs lend themselves to a wide and well-documented array of robustness checks, which we can easily adapt to the difference-in-discontinuities framework. As mentioned before, “age heaping” is problematic when dealing with age as a forcing variable. In our empirical setting, this is an issue only if age heaping is more or less severe before or after the reform. Therefore, we look at the change in density around the threshold. In the spirit of a [McCrary \(2008\)](#) test, we run Equation 2.2 on the log number of individuals within each cell (age x year). We find no evidence of discontinuous change at the threshold, which suggests this is unlikely to be an issue in our estimations. A standard check in an RD design is also to examine the distribution of pre-determined observables around the threshold. In our modified version, we again focus on the changes in observables before and

after the reform, which may indicate selection. For a set of covariates that includes education, race, province, household size and marital status, we find that they evolve smoothly around the threshold (Table 2.A.5).

Lastly, we check the sensitivity of our results to the selected window. In our main estimations, we have shown that the results are very similar with  $\pm 10$  window with a quadratic fit, and a  $\pm 5$  window with a linear fit (Tables 2.2 and 2.A.4). In Figure 2.A.9, we test the sensitivity of the estimates of Equation 2.2 with linear function as we gradually restrict the bandwidth size between  $\pm 10$  and  $\pm 5$ . For formal employment, the point estimate is negative when the window is large, but then converges to -1pp. as we restrict the window, which is equivalent to the estimate with a quadratic function on a larger window. On the contrary, the point estimate on informal employment remains stable, and roughly varies between -7pp. to -5pp.

Another way to employ our Diff-in-Disc estimator of Equation 2.2 is to exploit the change in discontinuity at 65, which is where the threshold was set for men before the reform. We show the results of this estimation in Table 2.A.6, with the note that coefficients should be interpreted with the opposite sign, as here we capture the effect of a negative discontinuity in the (change in the) share of people receiving the pension. Consistently, the results are very similar (and with opposite sign) of those presented before. The effect on employment is positive and significant, slightly larger but again mostly concentrated on informal employment. The coefficient on formal employment is slightly larger than before, but insignificant and imprecisely estimated.

## **2.6 Market-Level Effects**

Analyzing the effects of a pension reform that concerns several hundreds of thousands of potential beneficiaries raises the question of its potential impact on labor markets in general. Addressing these potential spillovers helps understanding the trade-offs that a wide-scope public policy entails. Indeed, a common motivation for reforming pension schemes is to allow for the replacement of older generations of workers and to provide jobs for younger generations. In the case of South Africa, previous studies

have focused their attention on within-household effects of receiving the pension, with mixed results and conclusions ([Ardington, Case and Hosegood \(2009\)](#), [Abel \(2013\)](#)). More generally, evidence on the question of substitution between old and young workers, which has received thorough empirical investigation in developed countries ([Salem et al. \(2010\)](#), [Banks et al. \(2010\)](#), [Bovini and Paradisi \(2019\)](#)), is largely absent in middle-income countries.

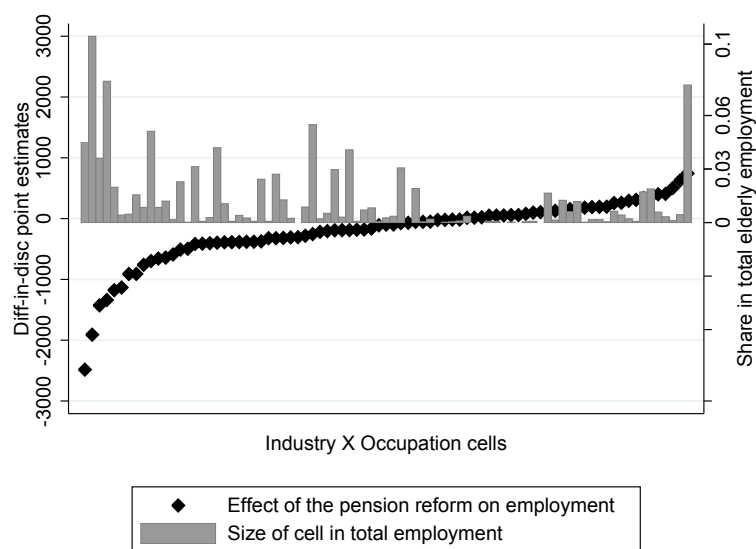
If all ages between the ages of 60 and 64 were treated equally, our estimate suggests that the reform drove 25,500 individuals out of the labor force; these are individuals who leave informal and low-paying formal jobs. In this section we address whether some of those jobs are picked up by people who are not directly concerned by the reform. In order to answer this question, we leverage heterogeneity in the effect of the pension by sector of employment. This follows naturally the heterogeneous results on informal and formal employment rates, as we can expect sectors with more informal employment to be more affected.

However, a very important obstacle for this strategy to yield meaningful results is the limited statistical power provided by the natural experiment we exploit. South African employment is large relative to the direct employment effects of the pension. In the first quarter of 2008, 14.4 million people of all characteristics are employed in South Africa. The effect of the pension reform on the employment of non-beneficiaries only represents, by the largest estimate, 0.2% of the total labor force. More importantly, this also means that those potential effects are small with regards to sampling variability and to the natural temporal variability in the level of employment in the various sectors; in other words, this suggests that the natural experiment we study does not necessarily provide us with enough statistical power to uncover the relevant general equilibrium effects.

We address this difficulty by focusing our attention on the parts of the employment pool that are the closest potential substitutes to workers directly affected, i.e. males aged 55 to 59. The intuition behind this strategy is that people who are very similar in terms of observable characteristics to the treated population may be the ones most likely to be hired as substitutes for them once they exit the labor market.

With this in mind, we carry out the same analysis as before, distinguishing employment across 100 sectors by industry and occupations, and identify the ones where the employment effects are the more pronounced. Figure 2.6 shows that a few key sectors of the economy concentrate a large share of the total drop in employment. This heterogeneity allows us to classify the most heavily affected cells as “treated” and the others as “control” by splitting cells into groups according to the weighted quartiles of their point estimate. We define the first quartile (the sectors with the most negative coefficients) as “treated” and the fourth quartile as “control”.<sup>17</sup> Here, identification relies on the assumption that employment in sectors that were differently affected by the reform of the OAP scheme would have evolved in a similar way absent the reform, after the date it was effectively implemented.

Figure 2.6: Heterogeneous Effects of the Pension Reform by Industry × Occupation Cells



*Note:* This graph plots the point estimate of Equation 2.2 on the number of elderly workers employed in each industry×occupation cell (black dots, left axis), and the share that the corresponding industry×occupation cell represents in total elderly employment (grey bars, right axis). Cells are ordered from left to right by the magnitude of the effect of the pension on the number of employed workers.

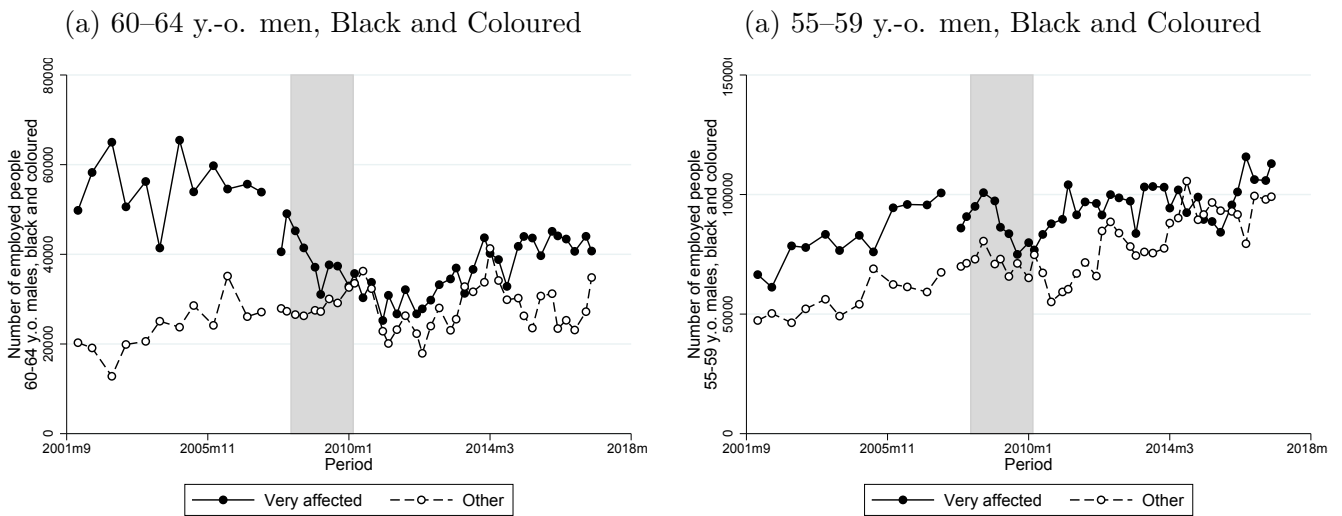
*Source:* Authors’ calculations on PALMS 3.2 (2002–2007 and 2010–2015).

The shock in the aggregate labor supply that is induced by the pension reform is essentially a decrease in the supply curve on a given labor market. According to standard labor economic theory, this could translate into three different responses

<sup>17</sup>Weighting the distribution of coefficients by the size of the employment of each sector guarantees that treatment and control groups are of equal size.

for individuals who are not directly affected by the shock, but can still be deemed as part of the same aggregate labor supply. The first is a change in the labor supply of people who are not working (extensive margin); the second is a change in the labor supply of individuals who are already working (intensive margin), and a third is a change in wages. Therefore, the outcomes of interest are the employment rate, the number of hours worked per employed person, and the wages, for the subgroups of the population we are interested in.

Figure 2.7: Effect of the Reform on the Employment of 60–64 and 55–59, Top vs. Bottom Quartile



*Note:* These graphs plot the total number of employed individuals in two groups of industry×occupation cells split by the value of the point estimates of the diff-in-disc equation 2.2. The solid line indicates industry×occupation in the first quartile, while the dashed line in the fourth quartile. The shaded area indicates the period of the reform. Panel (a) represents the evolution for 60–64 y.-o. Black and Coloured men. Panel (b) represents the evolution for 55–59 y.-o. Black and Coloured men.

*Source:* Authors’ calculations on PALMS 3.2 (2002–2017).

Figure 2.7 displays the result on employment rate. With regards to 60–64 Black and Coloured men, we find that on aggregate the treated cells do exhibit a sizable drop in employment, by about 20,000 workers from the third quarter of 2008 to the first quarter of 2012; while the control is rather stable over the same period. The effect seems to partly disappear over time, as the gap between both groups of sectors widens after a few years. Importantly, the pre-reform evolution of trends in employment is also parallel. After the reform, the employment of the 55–59 year old does not seem to evolve differentially in treated and non-treated sector, which we

take as evidence that the jobs left by pension recipients are not being picked up by these workers. Moreover, there is no visible adjustment neither in terms of hours worked nor wages (see Figure 2.A.10).

To confirm these results, we estimate a simple difference-in-difference model on each of these three outcomes. The estimating equation is:

$$Y_{c,t} = \kappa_c + \tau_t + \theta_1 T \times \{2010q2 < t < 2014\} + \theta_2 T \times \{2014 < t\} + \epsilon_{c,t} \quad (2.3)$$

where  $Y_{c,t}$  is the outcome of interest in cell  $c$  at time  $t$ ,  $\kappa_c$  and  $\tau_t$  are cell and time-period fixed effects,  $T$  is an indicator variable for being a heavily affected cell, and  $\epsilon_{c,t}$  is an error term, clustered at the cell-level. As the graphical evidence indicates that the effect of the reform might not be constant, we allow the treatment effect  $\theta$  to be flexible over time and split it in two components, a short- and a medium-term effect.

Results are displayed in Table 2.A.7. We recover the negative estimates on the treated groups, with a magnitude of about 22,000 Black and Coloured 60–64 y.-o. men, a result that is very compatible with the extrapolation of our diff-in-disc results.<sup>18</sup> This is mirrored by a large negative drop in the total number of hours worked per week, by about 930,000 hours in the treatment. Assuming no effect at the intensive margin for those still working, this would mean that the people who stop working as a result of the pension were working 45 hours per week, a number that is the mode of the distribution of hours per week in the sample. Weighting each cell by its total size in employment in the pre-reform period does not qualitatively affect these conclusions.

Estimates on the 55–59 y.-o. men do not show any sizable nor statistically significant effect of the reform in sectors where men aged 60–64 were more affected. The positive estimates in the unweighted case – which would be compatible with younger men picking up the jobs that the 60–64 are leaving – are very imprecise, and disappear when weighting each sector for its total employment size. As we are

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<sup>18</sup>The small discrepancy could come from the fact that we are focusing here only on the two most extreme quartiles of sectors.

limited in our statistical power to disentangle whether the jobs left by the elderly were picked up by younger individuals, we can only reject a one-to-one substitution with the closest workers (55–59 males). Nonetheless, this indicates that, overall, the number of jobs freed up by the reform is unlikely to have any significant impact on the stock of jobs available to younger workers.

## 2.7 Conclusion

This paper uses a decrease in the age eligibility threshold for men in the public pension scheme of South Africa to study its effect on the employment of the elderly. We show that not properly accounting for other private schemes leads to significantly overestimate the effects of the *Old Age Pension*. Despite no explicit requirement to retire when receiving the pension, we provide causal evidence that this reform triggered a large adjustment in old-age male employment at the extensive margin. This occurs because informal workers at all levels of informal wages quit their informal jobs, while formal workers respond only at very bottom of the wage distribution. Our results indirectly suggest that there might be welfare gains for recipients larger than the income effect, as it seems that the pension relieves some sort of subsistence-level constraint for people in informal employment.

These results contribute to the literature on informality in the labor market. The main debate in this literature has been whether workers choose informal employment, or take it due to the lack of better alternatives and as jobs of last resort. The findings of this paper support the second view, although within the context of old age workers in the South African labor market. For workers at age 60 or more, a transfer that is roughly equal to the median wage in the informal sector decreases informal employment by 30-40%; it also does not seem to cause any significant reallocation from formal to informal employment, suggesting that there are no workers for whom this change in incentives is large enough to induce any switching. This relaxes concerns about how the presence of a large informal sector might cause efficiency losses in means-tested programs such as public pensions.

Lastly, this paper aims to contribute to the design of pension systems in developing



countries. From these results, one could derive two main policy implications, which may apply to different extents to countries other than South Africa: 1) while there are strong disincentive effects on employment when expanding a public, non-contributory pension scheme, these mostly impact informal jobs. We do not observe any significant crowding out of formal jobs, despite the non-negligible amount of the public transfer; 2) for countries with similar demographic and employment structures to South Africa, pension reforms of this kind are unlikely to free-up a significant number of jobs for the young.

## 2.A Appendix

### 2.A.1 Conceptual framework

An individual  $i$  chooses how much to consume ( $C$ ), how much to work ( $l = T - L$ ) and in which sector to work ( $k \in \{F, I\}$ ). Utility is given by:

$$u_i(C, L, k) = \alpha_i \ln(C) + (1 - \alpha_i) \ln(L) - \mu_i(k)$$

under the budget constraint

$$w_{i,k}T \geq C + w_{i,k}L$$

We assume for simplicity that  $\mu_i(I) = 0$  and  $\mu_i(F) = \mu_i$ . Working in the formal sector implies a negative utility component that is not encountered when working in the informal sector. Prior to the reform, we assume that non-labor income is 0. This does not alter the qualitative results we derive but simplifies the notation. As a result of the reform, the non-labor income  $m$  is given to everyone in the informal sector  $I$ , and to people earning less than  $\bar{W}$  in the formal sector.

Prior to  $m$  being implemented, we observe people in the formal and in the informal sector. These are people for whom, respectively,

$$u_i(C^*(I), L^*(I)) < u_i(C^*(F), L^*(F)) \quad (\text{formal-sector workers}) \quad (2.4)$$

or

$$u_i(C^*(I), L^*(I)) > u_i(C^*(F), L^*(F)) \quad (\text{informal-sector workers}) \quad (2.5)$$

where  $L^*(k)$ ,  $C^*(k)$ ,  $u(C^*(k), L^*(k))$ ,  $k \in \{I, F\}$  refer respectively to the optimal levels of leisure and consumption, given that sector  $k$  has been chosen, and the

corresponding utility. Deriving  $L^*(k)$  and  $C^*(k)$  follows from first order conditions:

$$\begin{aligned} L^*(k) &= (1 - \alpha_i)T \\ C^*(k) &= \alpha_i w_{i,k} T \end{aligned}$$

Conditions (2.4) and (2.5) thus translate into:

$$\begin{aligned} &\alpha_i \ln(\alpha_i) + (1 - \alpha_i) \ln(1 - \alpha_i) + \alpha_i \ln(w_{i,I}) + \ln(T) \\ &\leq \alpha_i \ln(\alpha_i) + (1 - \alpha_i) \ln(1 - \alpha_i) + \alpha_i \ln(w_{i,F}) + \ln(T) - \mu_i \end{aligned}$$

Selection into sectors is thus determined by:

$$\mu_i < \alpha_i \ln\left(\frac{w_{i,F}}{w_{i,I}}\right) \quad (\text{formal-sector workers}) \quad (2.6)$$

or

$$\mu_i > \alpha_i \ln\left(\frac{w_{i,F}}{w_{i,I}}\right) \quad (\text{informal-sector workers}) \quad (2.7)$$

In words, people choose to work in the formal sector when the relative monetary benefits of doing so (as opposed to work in the informal sector), weighted by their preference for consumption (the utility associated with this gap in expected earnings), is larger than the (utility) costs associated with it. The difference in their choice of sector is driven by (i) differences in  $\frac{w_{i,F}}{w_{i,I}}$ , (ii) differences in  $\mu_i$ , or (iii) differences in  $\alpha_i$  (of course, several mechanisms might be simultaneously at play). We'll examine the consequences of (i), (ii) and (iii) on the effect of the introduction of  $m$ .

Let's focus on the case where  $w_{i,F} > w_{i,I}$ , so that there is a potential incentive to chose the formal sector in the first place (absent any costs, everybody would chose the formal sector because now,  $\alpha_i \ln(\frac{w_{i,F}}{w_{i,I}}) > 0$ ). If we don't assume that  $w_{i,F} > w_{i,I}$ , then trivially, the informal sector is more attractive than the formal sector on all accounts (expected earnings, costs of entry), and adding non-labor income is not going to change sectoral choice (but is going to have a negative effect on labor supply).

We next detail the effects of introducing  $m > 0$  on people's choice of sector. The effects are different according to the characteristics of people (who are defined by the vector  $\{w_{i,F}, w_{i,I}, \alpha_i, \mu_i\}$ ):

- people who chose to work in the informal sector when  $m = 0$  (A).
- people who chose to work in the formal sector when  $m = 0$  and whose (actual) formal earnings are below the means-test (B).
- people who chose to work in the formal sector when  $m = 0$  and whose (actual) formal earnings are above the means-test (C);

**(A) Informal workers** These workers satisfy condition (2.7)

$$\mu_A > \alpha_A \ln \left( \frac{w_{A,F}}{w_{A,I}} \right)$$

In the case where their pre-reform potential formal earnings are under the means-test, these workers face a new inequality,

$$\mu_A > \alpha_A \ln \left( \frac{w_{A,F}}{w_{A,I}} \right) - \ln \left( \frac{w_{A,F}}{w_{A,I}} \times \frac{w_{A,I}T + m}{w_{A,F}T + m} \right) \quad (2.8)$$

This inequality comes from the new optimal levels of leisure and consumption:

$$L^*(k, m) = (1 - \alpha_i) \left( T + \frac{m}{w_{i,k}} \right)$$

$$C^*(k, m) = \alpha_i (w_{i,k}T + m)$$

We want to compare  $u(C^*(F, m), L^*(F, m), F)$  and  $u(C^*(I, m), L^*(I, m), I)$ , which translate into:

$$\begin{aligned} & \alpha_i \ln(\alpha_i) + (1 - \alpha_i) \ln(1 - \alpha_i) + \alpha_i \ln(w_{i,I}) + \ln \left( T + \frac{m}{w_{i,I}} \right) \\ & > \alpha_i \ln(\alpha_i) + (1 - \alpha_i) \ln(1 - \alpha_i) + \alpha_i \ln(w_{i,F}) + \ln \left( T + \frac{m}{w_{i,F}} \right) - \mu_i \end{aligned}$$

which yields

$$\mu_A > \alpha_A \ln \left( \frac{w_{A,F}}{w_{A,I}} \right) + \ln \left( \frac{T + \frac{m}{w_{A,I}}}{T + \frac{m}{w_{A,F}}} \right)$$

and after some rearranging, (2.8). However, trivially,  $\alpha_A \ln \left( \frac{w_{A,F}}{w_{A,I}} \right) > \alpha_A \ln \left( \frac{w_{A,F}}{w_{A,I}} \right) - \ln \left( \frac{w_{A,F}}{w_{A,I}} \times \frac{w_{A,I}T+m}{w_{A,F}T+m} \right)$ , because  $\frac{w_{A,F}}{w_{A,I}} \times \frac{w_{A,I}T+m}{w_{A,F}T+m} > 1$ , as  $w_{A,F}w_{A,I}T+mw_{A,F} > w_{A,F}w_{A,I}T+mw_{A,I}$ ; so (2.7) implies (2.8), and informal workers will keep working in the informal sector. Note that the same can be said of informal workers for whom pre-reform potential earnings are above the means-test, since (2.8), for these workers, becomes:

$$\mu_A > \alpha_A \ln \left( \frac{w_{A,F}}{w_{A,I}} \right) - \ln \left( \frac{w_{A,F}}{w_{A,I}} \times \frac{w_{A,I}T+m}{w_{A,F}T} \right) \quad (2.8\text{bis})$$

which is also trivially implied by (2.7).

Labor supply will drop for those workers, as a consequence of the income effect, as leisure goes from

$$L(0) = (1 - \alpha_A)T$$

to

$$L(m) = (1 - \alpha_A) \left( T + \frac{m}{w_{A,I}} \right)$$

thus the labor supply drops from:

$$l(0) = T - L(0) = \alpha_A T$$

to

$$l(m) = T - L(m) = \alpha_A T - (1 - \alpha_A) \frac{m}{w_{A,I}}$$

So that:

$$\Delta_{\alpha} l(m) = \frac{l(m) - l(0)}{l(0)} = -\frac{1 - \alpha_A}{\alpha_A} \frac{m}{w_{A,I}} < 0$$

**(B) Formal workers with  $w_{B,F} l^*(F) < \bar{W}$**  These workers satisfy (2.6)

$$\mu_B < \alpha_B \ln \left( \frac{w_{B,F}}{w_{B,I}} \right)$$

And face a new inequality

$$\mu_B < \alpha_B \ln \left( \frac{w_{B,F}}{w_{B,I}} \right) - \ln \left( \frac{w_{B,F}}{w_{B,I}} \times \frac{w_{B,I}T + m}{w_{B,F}T + m} \right) \quad (2.9)$$

This comes straightforwardly from the fact that, in the absence of the reform, workers locate on a segment of the (formal-labor related) budget constraint that will be shifted by the additional non-labor income. The relevant part of the formal-wage budget constraint (and the whole informal-wage budget constraint) thus both include a shift by  $m$ .

Labor supply will drop for those workers, as a consequence of a income effect, as leisure goes from

$$L(0) = (1 - \alpha_B)T$$

to

$$L_k(m) = (1 - \alpha_B) \left( T + \frac{m}{w_{B,k}} \right)$$

if condition (2.9) still holds, then these workers will stay in the formal sector and work less hours:

$$\Delta_m(l) = -\frac{1 - \alpha_B}{\alpha_B} \frac{m}{w_{B,F}} < 0 \quad (2.10)$$

if condition (2.9) does not hold, then these workers will switch to the informal sector and still work less hours:

$$\Delta_m(l) = -\frac{1 - \alpha_B}{\alpha_B} \frac{m}{w_{B,I}} < 0 \quad (2.11)$$

Note that whether the drop in labor supply for “stayers” (2.10) is larger than that of “switchers” (2.11) is not unequivocal. Indeed, stayers might differ from switchers because of a larger expected wage ratios,  $\frac{w_{B,F}}{w_{B,I}}$ , because of a larger  $\alpha_B$ , or because of a smaller  $\mu_B$ . All other things equal, if switching to informal work as a result of the pension  $m$  is due to a smaller  $\alpha_B$ , then the drop in the labor supply of switchers is larger than that of stayers. On the contrary, if it is due to a smaller  $\frac{w_{B,F}}{w_{B,I}}$ , the drop in labor supply of switchers is likely to be smaller than that of stayers.

**(C) Formal workers with  $w_{C,F}l^*(F) > \bar{W}$**  These workers also satisfy (2.6)

$$\mu_C < \alpha_C \ln \left( \frac{w_{C,F}}{w_{C,I}} \right)$$

However, the effect of the pension reform is somewhat more intricate. Indeed the means-tested non-labor income induces a discontinuity in the formal-wage budget constraint. Whether this makes workers prefer to locate under the means-test is determined by whether this discontinuity is large with regards to the utility of the pre-reform optimum, and whether the means-test is close to the pre-reform optimal formal labor supply. We call  $l_{\bar{W}}$  the labor supply that satisfies:  $w_{i,F}l_{\bar{W}} = \bar{W}$ . If  $u(\bar{W} + m, T - l_{\bar{W}}, F) > u_0^*(F)$ , where  $u_0^*(F)$  is the pre-reform formal-sector optimum, then workers, conditional on choosing to work in the formal sector, will prefer to decrease their labor supply to meet the means-test. Note that this does not necessarily imply that the new formal optimum is a corner solution at the means-test: for some workers whose pre-reform optimum was near the means-test, the income-effect of  $m$  is likely to kick in and push them towards a new interior solution.

In the case the new formal optimum is a corner solution at the means-test, whether workers prefer this new formal-sector optimum to switching to informal

sector is determined by:

$$u_m^*(I) \leq u(\bar{W} + m, T - l_{\bar{W}}, F) \quad (2.12)$$

where, similarly to above,  $u_m^*(I)$  refers to the informal-sector optimum with the pension. On the case the new formal-sector optimum is an interior solution, this inequality becomes:

$$\mu_C \leq \alpha_C \ln \left( \frac{w_{C,F}}{w_{C,I}} \right) - \ln \left( \frac{w_{C,F}}{w_{C,I}} \times \frac{w_{C,I}T + m}{w_{C,F}T + m} \right) \quad (2.13)$$

Note that (2.13) is the same as (2.9). If on the other hand, conditional on choosing to work in the formal sector, workers keep with their pre-reform optimum, this means that the size of the pension is not enough to outweigh the amount of formal wage they would have to give up on to meet the means-test. In this case the formal sector optimum is left unchanged by the introduction of the pension. This is more likely when formal hourly wages are large. The condition to switch to the informal sector is given by:

$$\mu_C \geq \alpha_C \ln \left( \frac{w_{C,F}}{w_{C,I}} \right) - \ln \left( \frac{w_{C,F}}{w_{C,I}} \times \frac{w_{C,I}T + m}{w_{C,F}T} \right) \quad (2.14)$$



## 2.A.2 Tables and Figures

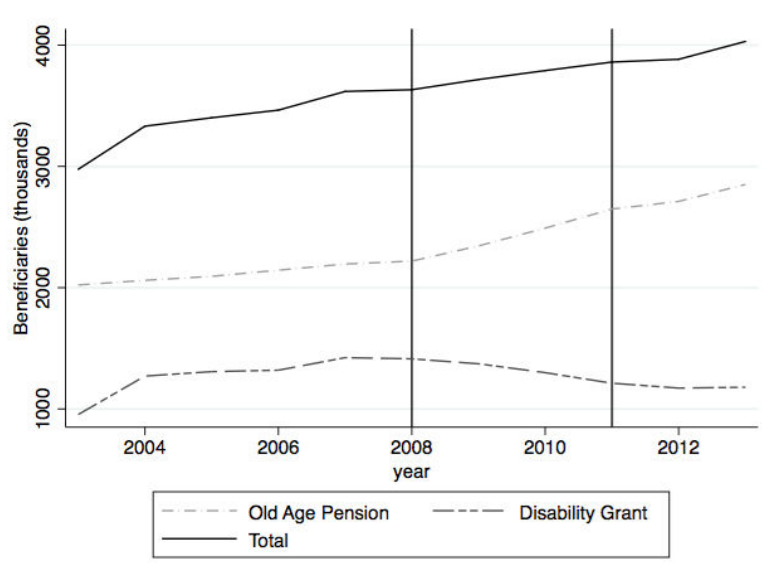
Table 2.A.1: Evolution of the *Old Age Pension*, 1993–2010

Date	Age threshold		Amount	Amount (R '10 )	Means-Test
	Men	Women			
1993	65	60	R 370	R 1008	
1994	65	60	R 390	R 977	R 4440
2000	65	60	R 540	R 902	
2003	65	60	R 700	R 955	R 16,920
2007	65	60	R 870	R 1069	
2008(Q3)	<b>63</b>	60	R 940	R 1049	R 26,928
2009(Q2)	<b>61</b>	60	R 1010	R 1051	R 27,552
2010(Q2)	<b>60</b>	60	R 1080	R 1080	R 31,296
2012	60	60	R 1200	R 1081	R 47,400
2013	60	60	R 1270	R 1081	R 50,340
2014	60	60	R 1350	R 1083	R 61,800
2015	60	60	R 1410	R 1082	R 64,680
2016	60	60	R 1510	R 1087	R 69,000

*Note:* The age threshold was different for men and women until it was equalized between 2008–10. Amount is presented in current Rand and 2010 Rand separately, CPI data is taken from OECD.stat.

*Source:* The main sources on OAP amounts are [Eyal and Woolard \(2011\)](#) and the South African government (<http://www.gov.za/services/social-benefits-retirement-and-old-age/old-age-pension>). Reform dates are from the [National Budget Reviews \(2007-2013\)](#). Information on the means-test for 2009 and 2010 is collected from [US \(2003-2015\)](#); [Ranchhod \(2006\)](#) for 2003; [Case and Deaton \(1998\)](#) for 1994; and from [SASSA \(2010, 2013, 2014, 2016\)](#) for all remaining years.

Figure 2.A.1: Number of *Old Age Pension* and *Disability Grant* Beneficiaries, 2003–2013



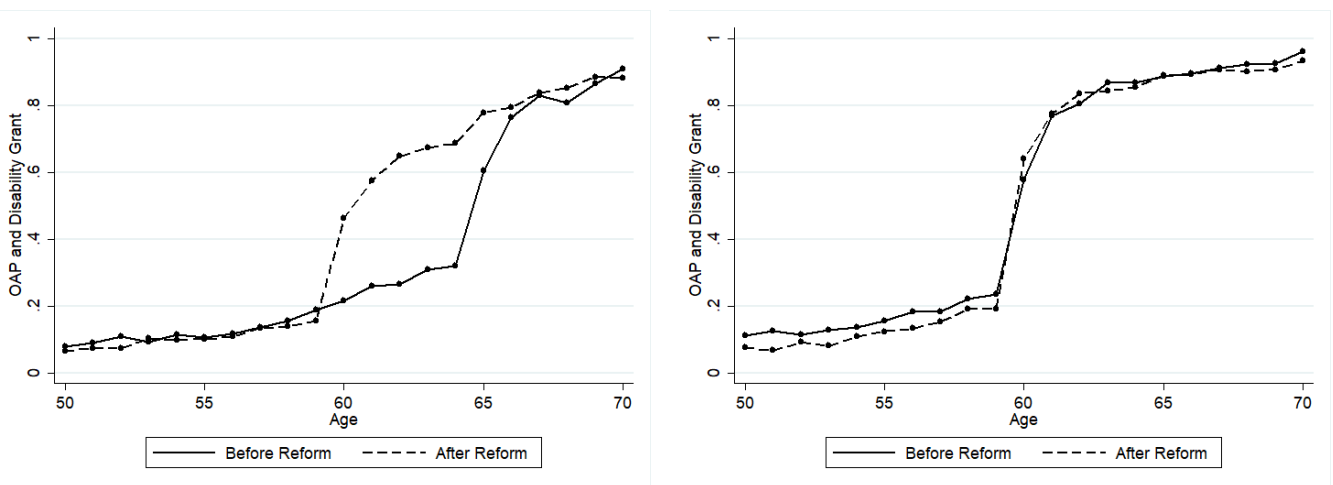
*Note:* This graph gives the number of beneficiaries of the *Old Age Pension* or of the *Disability Grant* between 2003 and 2013, and their sum (in thousands, 2000 on the graph equals 2 million). The vertical lines indicate the reform period for the OAP, where the age threshold for men was gradually lowered from 65 to 60.

*Source:* [National Budget Reviews \(2007–2013\)](#). These figures come from administrative data (SOCPEN). When there are small discrepancies for the same year, the latest available estimate is used.

Figure 2.A.2: Share of People Receiving the *Old Age Pension* or *Disability Grant* by Age, **Before and After** Pension Reform, Black and Coloured Only

(a) Men

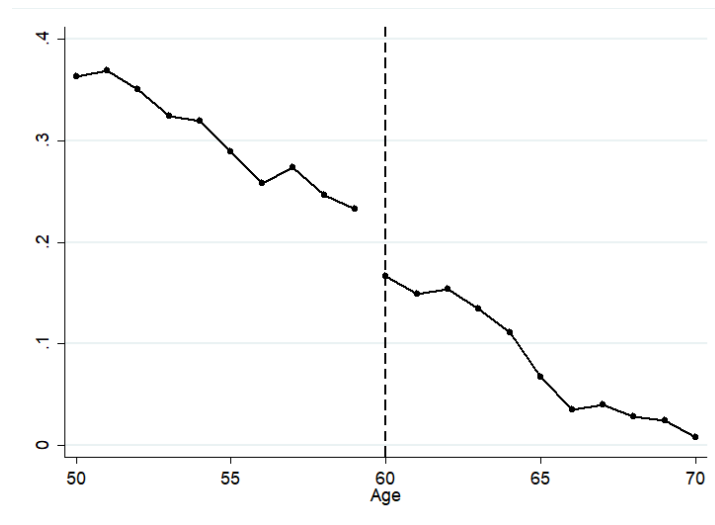
(b) Women



*Note:* These graphs plot the share of individuals receiving the *Old Age Pension* or the *Disability Grant* within each age bin, for men and women separately. The sample is restricted to the Black and Coloured population. The solid line is for the years before the reform (2002–2007), while the dotted line is for the years after the reform (2010–2015).

*Source:* Authors' calculations on GHS.

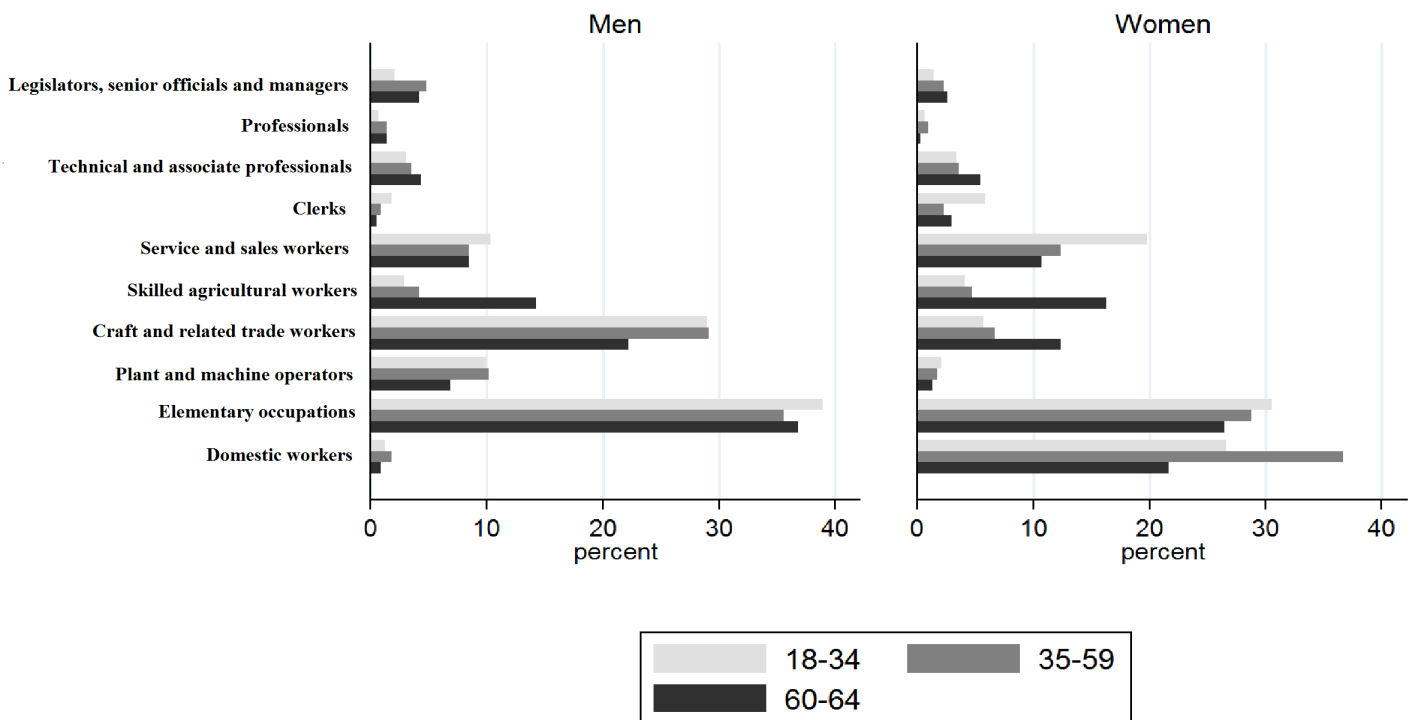
Figure 2.A.3: Joint Probability to Be Employed and Contribute to a Private Pension, Before Reform



Note: This graph plots probability of being employed *and* contributing to a private pension scheme in the years between 2002 and 2007 for men. During the period, the age eligibility threshold for the *Old Age Pension* was set at age 65 for men.

Source: Authors' calculation on LFS (2002–2007).

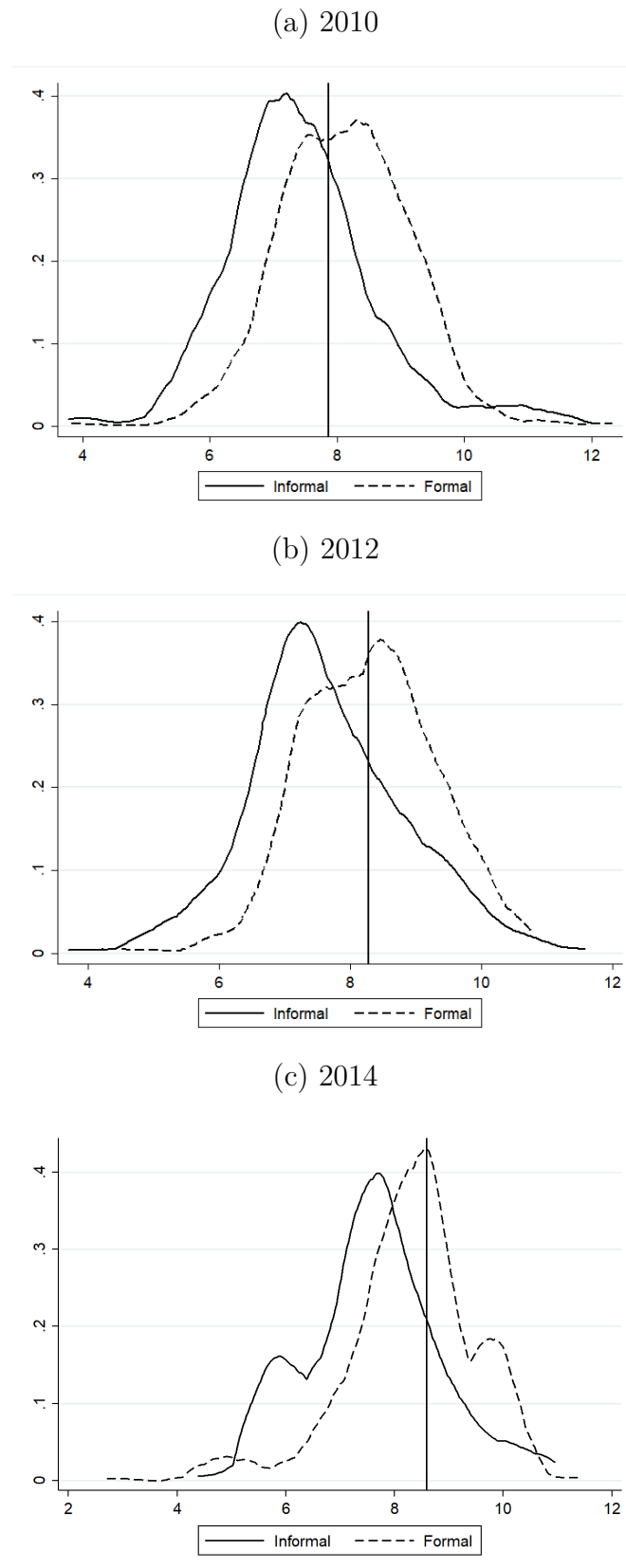
Figure 2.A.4: Informal Employment by Occupation and Age Groups, Men and Women, PALMS



Note: These graphs plot the share of informal employment across occupational categories for different age groups, for men and women respectively. The age groups are 18–35 years old, 35–59, and 60–64.

Source: Authors' calculations on PALMS 3.2 (2006–2008).

Figure 2.A.5: Position of the Means-Test with Respect to Formal and Informal Monthly Earnings, 2010, 2012, 2014



*Note:* These graphs give the location of the means-test with respect to the distribution of formal and informal monthly earnings in three separate years 2010, 2012, and 2014. Informal monthly earnings are indicated by the solid line, while the dashed line is for formal monthly earnings. The solid vertical lines indicate the position of the means-test in the three years. Earnings and means-test are in nominal Rand amount (not adjusted for inflation).

*Source:* PALMS v. 3.2 (2010, 2012, 2014).

Table 2.A.2: Characteristics by Labor Market Status, Men aged 60–64, Before Reform, 2002–2007

<i>Characteristics</i> (pop %)	Informal	Formal	Non- Employed
<i>Proportions</i>	0.13	0.15	0.72
<i>Socio-Demographics</i>			
Black (67.18%)	83.59	65.34	64.52
Married (80.11%)	82.32	87.70	78.09
Education (6.02yrs)	4.68	6.98	6.07
Household Size (4.66 ppl)	4.54	4.11	4.79
<i>Job Characteristics</i>			
Average Weekly Hours	45.7	47.3	
Part-time	15.69	3.01	
Median Monthly Salary (R'10)	1373	4662	
Median Hourly Wage (R'10)	7.3	22.9	
Median Tenure (yrs)	5	16	
Self-Employed	36.08	12.49	
Median Firm Size (ppl)	2–4	20–49	

---

This table gives average characteristics by labor market status, for men aged 60–64 between 2002 and 2007. On average over the period, 13% of men are informally employed, 15% are formally employed, and 72% do not work. Among those informally employed, 83.59% are Black. Education is measured in years of schooling, and household size in number of household members. Salary and wage information is in 2010 Rand, tenure is the number of years since the start of the current job. Self-employed is the share of the people running their own business. Firm size is a categorical variable for the number of co-workers.

*Source:* Authors' calculations on PALMS 3.2 (2002–2007).

Table 2.A.3: Old Age Pension and Employment, RDD Results, Linear Fit 55–64, PALMS

	Before Reform			After Reform		
	(1) Employed	(2) Informal	(3) Formal	(4) Employed	(5) Informal	(6) Formal
<b>a. Men</b>						
<i>Age</i> <sub>(60+)</sub>	-0.0573*** (0.0182)	-0.0007 (0.0140)	-0.0566*** (0.0157)	-0.1097*** (0.0107)	-0.0471*** (0.0077)	-0.0626*** (0.0100)
Mean <i>Y</i> at Age 59	0.41	0.18	0.23	0.46	0.15	0.31
Observations	22,405	22,405	22,405	46,764	46,764	46,764
R-squared	0.0295	0.0069	0.0279	0.0827	0.0176	0.0557
<b>b. Women</b>						
<i>Age</i> <sub>(60+)</sub>	-0.0812*** (0.0124)	-0.0510*** (0.0104)	-0.0302*** (0.0084)	-0.0720*** (0.0083)	-0.0430*** (0.0061)	-0.0290*** (0.0068)
Mean <i>Y</i> at Age 59	0.25	0.15	0.09	0.31	0.14	0.17
Observations	33,026	33,026	33,026	68,977	68,977	68,977
R-squared	0.0609	0.0325	0.0277	0.0677	0.0247	0.0385

*Note:* \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . This table gives the results of Equation 2.1 with a **linear function**, for men (upper panel), and women (lower panel), on the age window 55–64. The sample is limited to Black and Coloured men and women. We only report the coefficient of interest,  $\beta_{RD}$ . The dependent variables are binary variables for: (1) employed, (2) informally employed, (3) formally employed, such that (1)=(2)+(3), and (4)=(5)+(6). *Mean Y at Age 59* refers to the value of the dependent variable at age 59 in the years after the reform. Robust standard errors in parentheses.

*Source:* Authors' calculations on PALMS 3.2 (2002–2007 and 2010–2015).

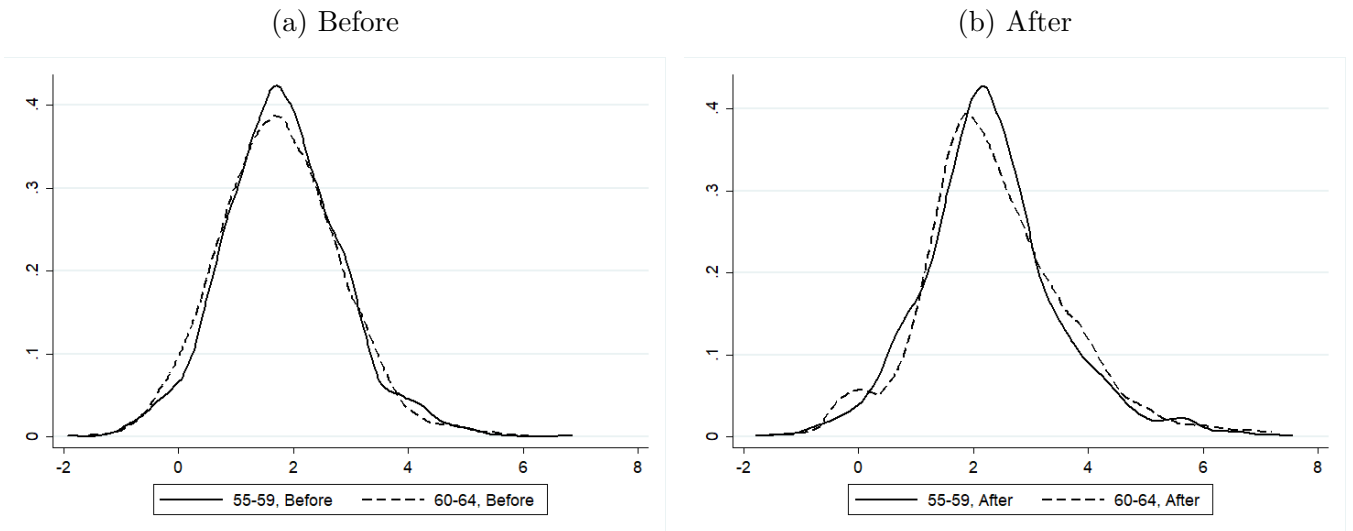
Table 2.A.4: Old Age Pension and Employment, Linear Fit 55–64, Diff-in-Disc, PALMS

	Employed	Informal	Formal	
	(1)	(2)	Extensive (3)	Intensive (4)
<b>a. Men</b>				
$Post \times Age_{(60+)}$	-0.0539** (0.0214)	-0.0465** (0.0179)	-0.0074 (0.0216)	-0.1027 (1.0786)
Mean $Y$ at Age 59	0.52	0.14	0.38	16.70
Observations	69,169	69,169	69,169	69,169
R-squared	0.0652	0.0177	0.0514	0.0444
<b>b. Women</b>				
$Post \times Age_{(60+)}$	0.0092 (0.0129)	0.0080 (0.0145)	0.0012 (0.0116)	0.1025 (0.5178)
Mean $Y$ at Age 59	0.34	0.12	0.21	8.10
Observations	102,003	102,003	102,003	102,003
R-squared	0.0698	0.0276	0.0462	0.0400

*Note:* \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . This table gives the results of Equation 2.2 with a **linear function**, for men (upper panel) and women (lower panel) on the age window 55–64. The sample is limited to Black and Coloured men and women. We only report the coefficient of interest,  $\beta_{DiDRD}$ . The dependent variables are binary variables for: (1) employed, (2) informally employed, (3) formally employed, such that (1)=(2)+(3), and (4) hours in formal employment. *Mean Y at Age 59* refers to the value of the dependent variable at age 59 in the years after the reform. Robust standard errors clustered at the race-cohort group.

*Source:* Authors' calculations on PALMS 3.2 (2002–2007 and 2010–2015).

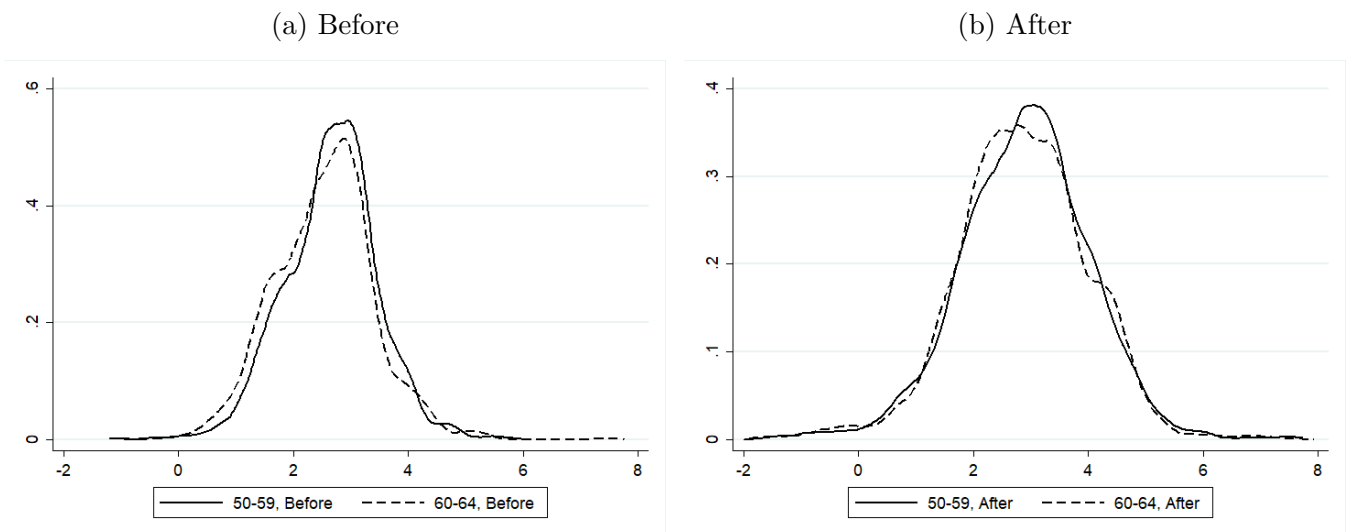
Figure 2.A.6: Distribution of Informal Hourly Wages, 55–59 vs. 60–64, Before and After



*Note:* These graphs plot the distribution of log-hourly wage for informal workers by age group before the reform (2002–2007), panel (a), and after the reform (2010–2015), panel (b), adjusted for inflation. The sample includes Black and Coloured males only.

*Source:* Authors' calculations on PALMS 3.2 (2002–2007 and 2010–2015).

Figure 2.A.7: Distribution of Formal Hourly Wage, 55–59 vs. 60–64, Before and After



*Note:* These graphs plot the distribution of log-hourly wage for formal workers by age group before the reforms (2002–2007), panel (a), and after the reform (2010–2015), panel (b), adjusted for inflation. The sample includes Black and Coloured males only.

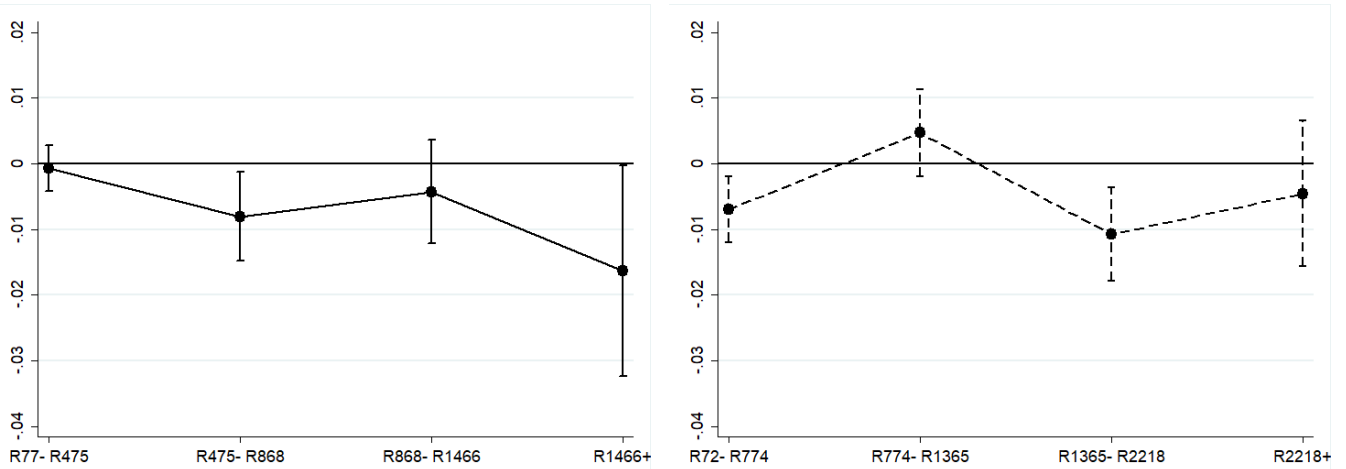
*Source:* Authors' calculations on PALMS 3.2 (2002–2007 and 2010–2015).



Figure 2.A.8: Effect on Probability to be **Formally** Employed by Quartile of **Informal** Hourly Wage, RDD, Before and After, Women

(a) Women - Before

(b) Women - After



*Note:* These graphs plot the coefficients of Equation 2.1 on the probability to be formally employed within each quartile of the informal hourly wage distribution before and after the OAP reform. Quartiles are defined according the distribution of informal hourly wages for the 55-59 years old population in each period. The x-axis is labelled with the bounds of the quartiles for the monthly salary equivalent at a 43 hours working week. The sample includes Black and Coloured women only.

*Source:* Authors' calculations on PALMS 3.2 (2002–2007 and 2010–2015).

Table 2.A.5: Density and Balancing Test, PALMS, Men Only

	Diff-in-Disc (1)
<b>a. Density</b>	
<i>Log of Individuals</i>	-0.1444 (0.1085)
<i>Observations</i>	240
 <b>b. Balancing Test</b>	
<i>Y Variable</i>	
Black	0.0016 (0.0221)
White	-0.0307 (0.0204)
Married	0.0026 (0.0183)
Education	-0.1354 (0.2080)
Cape Province	0.0011 (0.0175)
Household Size	-0.0223 (0.1280)
<i>Observations</i>	166,599

*Note:* \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . The sample is restricted to men. This table reports the coefficient  $\beta_{DiDRD}$  of Equation 2.2 with a quadratic function, on the age window 50 to 70. In the upper panel, observations are collapsed at age $\times$ year cell level. The dependent variable is the log of individuals within each cell. In the lower panel, the variables aligned vertically are the dependent variables in the regression. *Education* is equal to the completed years of schooling. *Cape Province* is a binary variable equal to one for an individual residing in either the Western, Eastern, or Northern Cape provinces. Robust standard errors in parentheses.

*Source:* Authors' calculations on PALMS 3.2 (2002–2007 and 2010–2015).

Table 2.A.6: Old Age Pension and Employment, Linear Fit 60–70, Diff-in-Disc with Threshold at 65, PALMS

	<b>Employed</b>	<b>Informal</b>	<b>Formal</b>	
	(1)	(2)	Extensive (3)	Intensive (4)
<b>a. Men</b>				
$Post \times Age_{(65+)}$	0.0629** (0.0295)	0.0389*** (0.0126)	0.0240 (0.0252)	1.6033 (1.1366)
	66,903	66,903	66,903	66,903
R-squared	0.0764	0.0193	0.0929	0.0815
<b>b. Women</b>				
$Post \times Age_{(65+)}$	-0.0096 (0.0122)	-0.0097 (0.0080)	0.0002 (0.0097)	-0.0355 (0.4305)
Observations	99,982	99,982	99,982	99,982
R-squared	0.0532	0.0146	0.0613	0.0545

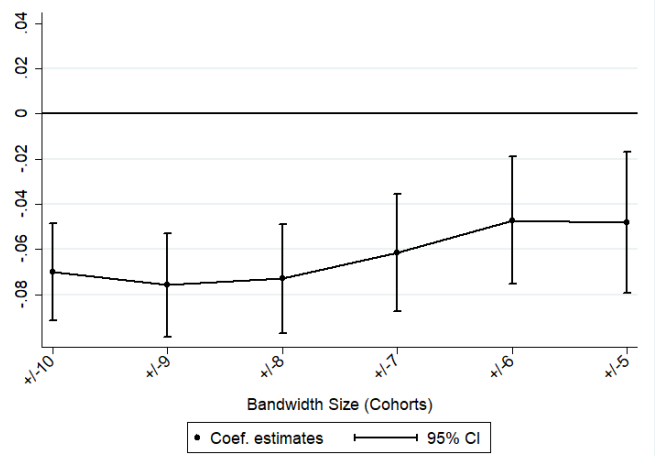
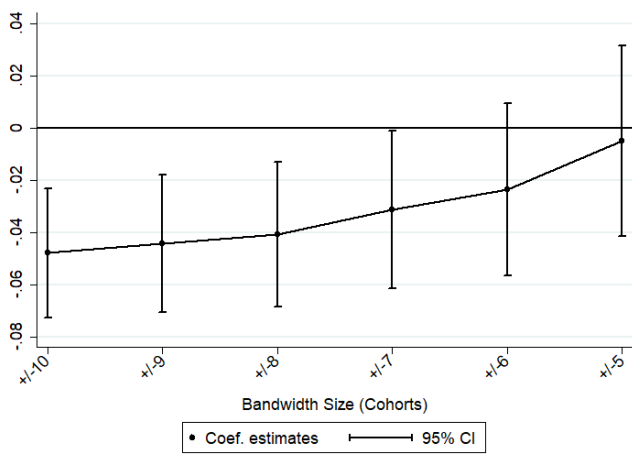
*Note:* \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . This table gives the results of Equation 2.2 with a linear function, for men (upper panel) and women (lower panel) on the age window 60–70. The threshold is set at 65, rather than 60. We only report the coefficient of interest,  $\beta_{DiDRD}$ . The dependent variables are binary variables for: (1) employed, (2) informally employed, (3) formally employed, such that (1)=(2)+(3), and (4) hours in formal employment. Robust standard errors clustered at the race-cohort group.

*Source:* Authors' calculations on PALMS 3.2 (2002–2007 and 2010–2015).

Figure 2.A.9: Impact on Formal and Informal Employment, Bandwidth Sensitivity, Linear Function, PALMS

(a) Formal Employment

(b) Informal Employment



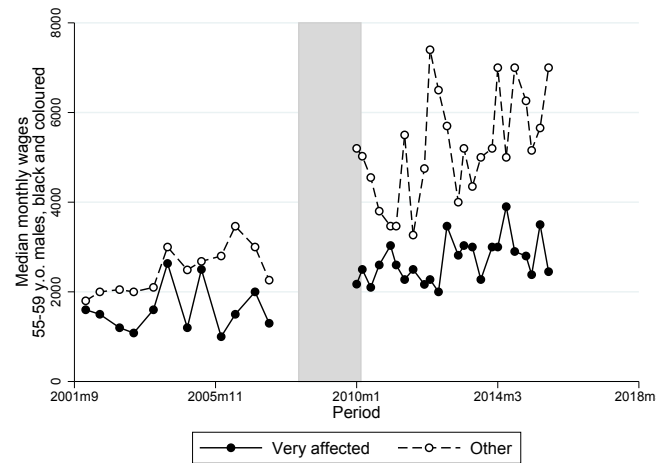
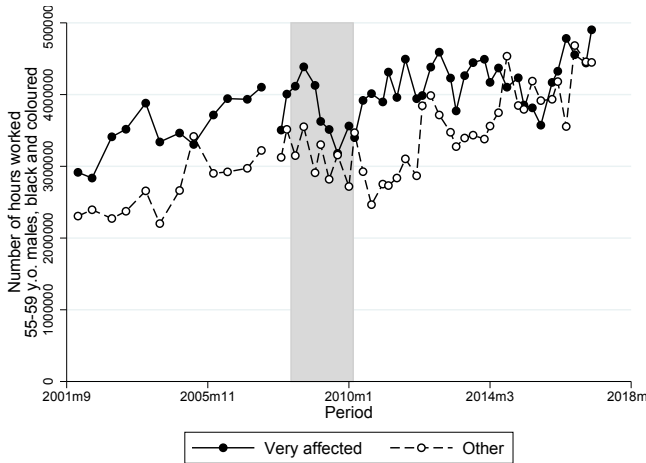
*Note:* These graphs plot the coefficients of Equation 2.2 with a linear function on formal and informal employment for different age windows, with 95% confidence intervals. The x-axis reports the number of age-values included in the estimation, where +/- 10 equals the window from 50 to 70 years of age.

*Source:* Authors' calculations on PALMS (2002–2007 and 2010–2015).

Figure 2.A.10: Effect of the Reform on Hours Worked and Wages of 55–59 y.o. Black and Coloured Men

(a) Hours worked

(b) Median wages



*Note:* These graphs plot the total number of hours worked (Panel (a)) and the median wages (Panel (b)) of 55–59 y.o. Black and Coloured men in two groups of industry×occupation cells split by the value of the point estimates of the diff-in-disc equation 2.2 in each cell. The solid line indicates industry×occupation in the top quartile, while the dashed line in the bottom quartile. The shaded area indicates the period of the reform. Wage data is missing for the 2008–2010 period.

*Source:* Authors' calculations on PALMS 3.2 (2002–2017).

Table 2.A.7: Labor Outcomes of 55–59 Men; Employment, Hours and Wages

	Unweighted			Weighted		
	60-64 Black & Col.	60-64 All	55-59 Black & Col.	60-64 Black & Col.	60-64 All	55-59 Black & Col.
<b>Panel A: Nb of employed people</b>						
Treatment $\times$ (2010q4 $\geq$ Year < 2014)	-2043.8** (990.4)	-2120.7* (1204.4)	630.9 (1130.5)	-3314.8** (1583.7)	-4067.8** (1856.4)	-1624.8 (1822.6)
Treatment $\times$ (2014 $\geq$ Year $\geq$ 2017)	-1233.7 (957.7)	-1163.3 (1134.0)	319.9 (1150.1)	-2794.1* (1454.9)	-3461.0** (1678.3)	-2831.4 (1921.9)
<i>N</i>	1764	1764	1764	1722	1722	1722
<b>Panel B: Nb of hours worked</b>						
Treatment $\times$ (2010q4 $\geq$ Year < 2014)	-84524.6** (33885.4)	-93976.7** (43854.1)	39030.4 (42554.6)	-124636.7** (51283.5)	-166383.4** (63566.8)	-16145.1 (64835.9)
Treatment $\times$ (2014 $\geq$ Year $\geq$ 2017)	-46549.3 (31997.0)	-47047.1 (40414.5)	21958.8 (41289.6)	-98312.1** (43469.8)	-128169.1** (55193.4)	-79453.2 (65113.8)
<i>N</i>	1764	1764	1764	1722	1722	1722
<b>Panel C: Median monthly wage</b>						
Treatment $\times$ (2010q4 $\geq$ Year < 2014)	-277.9 (1288.0)	-1308.2 (981.6)	-1839.3*** (446.4)	-199.0 (2079.9)	-1744.2** (796.0)	-1634.8*** (442.3)
Treatment $\times$ (2014 $\geq$ Year $\geq$ 2017)	3490.7 (3326.8)	-208.5 (1064.7)	-1677.2*** (506.5)	5719.4 (5281.1)	-922.3 (848.5)	-1542.1*** (466.1)
<i>N</i>	568	568	587	568	568	587

Stars indicate the statistical significance of the coefficients. \* :  $p < 0.1$  , \*\* :  $p < 0.05$  , \*\*\* :  $p < 0.01$  .

Each column corresponds to the estimation of Equation 2.3 relative to the population indicated in each column.

Treatment status is defined by the magnitude of the industry  $\times$  occupation-specific labor supply response of the 60–64 y.-o. Black and Coloured men.

The weights in columns (4) to (6) correspond to the size of the industry  $\times$  occupation cells in terms of employment of non-treated groups in 2007.

Standard errors are clustered at the industry  $\times$  occupation level.

### 2.A.3 Heterogeneity by Hourly-Wage Levels

We detail in this Appendix the issues with carrying out a difference-in-discontinuities estimation procedure on employment dummies for different levels of wages, and why and under which assumptions a simpler regression discontinuity design is more appropriate and easier to implement.

We call  $Y_{01}$ ,  $Y_{00}$ ,  $Y_{11}$  and  $Y_{10}$  the potential outcomes of the individual when he receives the treatment in  $T = 0$ , when he does not (still in  $T = 0$ ), and when he receives the treatment in  $T = 1$ , and when he does not. Let us define the treatment as being eligible to the pension.  $D$  is a dummy variable for receiving the treatment,  $T$  is a time variable equal to 1 if the period is post-reform, and 0 if it is pre-reform, and  $X$  represents the age of the individual:  $D = \mathbb{1}\{X \geq 60 \cap T = 1\}$ .  $\tau = E[Y_{11} - Y_{10} | X = 60]$  measures the effect that we are interested in, which is the local effect of the pension at 60, for people who are 60 after the reform is implemented. We do not observe  $Y_{01}$ ,  $Y_{00}$ ,  $Y_{11}$  and  $Y_{10}$  for all  $X$  and  $T$ , but only:

$$Y = (1 - T)(1 - D)Y_{00} + (1 - T)DY_{01} + DTY_{11} + (1 - D)TY_{10}$$

The classical RDD estimand is defined as:

$$\tau_{RDD} = E[Y | T = 1, D = 1, X = 60] - \lim_{x \rightarrow 60^-} E[Y | T = 1, D = 0, X = x]$$

which, under the continuity assumption ( $\star$ ) that  $\lim_{x \rightarrow 60^-} E[Y | T = 1, D = 0, X = x] = E[Y | T = 1, D = 0, X = 60]$ , identifies  $\tau$ :

$$\begin{aligned} \tau_{RDD} &= E[Y | T = 1, D = 1, X = 60] - E[Y | T = 1, D = 0, X = 60] \\ &= E[Y_{11} | T = 1, D = 1, X = 60] - E[Y_{10} | T = 1, D = 0, X = 60] \\ &= E[Y_{11} - Y_{10} | T = 1, D = 1, X = 60] \\ &= \tau \end{aligned}$$

A potential problem with the RDD estimand – and justification for using the

difference-in-discontinuities – lies in the fact that  $(\star)$  might not be verified: there could be a discontinuity at the threshold in the absence of the pension reform, i.e. in the absence of a change in  $D$ . However, at the price of making two assumptions, we can leverage the data from the pre-reform period, which yields information on this discontinuity. These assumptions  $(\star\star)$  and  $(\star\star\star)$  write down as:

$$\begin{aligned}
 (\star\star) \quad & E[Y_{10}|T = 1, D = 0, X = 60] - \lim_{x \rightarrow 60^-} E[Y_{10}|T = 1, D = 0, X = x] \\
 &= E[Y_{00}|T = 0, D = 0, X = 60] - \lim_{x \rightarrow 60^-} E[Y_{00}|T = 0, D = 0, X = x] \\
 (\star\star\star) \quad & \lim_{x \rightarrow 60^-} E[Y_{11} - Y_{10}|T = 1, X = x] \\
 &= \lim_{x \rightarrow 60^+} E[Y_{11} - Y_{10}|T = 1, X = x]
 \end{aligned}$$

(provided those limits exist.) Assumption  $(\star\star)$  means that the discontinuity in the absence of treatment is constant over time: absent the reform, the same discontinuity, if any, would be observed in  $T = 1$  as is observed in  $T = 0$ ; it is similar to the continuity assumption  $(\star)$ . Assumption  $(\star\star\star)$  expresses the fact that the discontinuity for the treated and control group are equal.

And this leads to the now well-known difference-in-discontinuities strategy:

$$\begin{aligned}
 \tau_{\text{DiDisc}} = & E[Y|T = 1, D = 1, X = 60] - \lim_{x \rightarrow 60^-} E[Y|T = 1, D = 0, X = x] \\
 & - (E[Y|T = 0, D = 0, X = 60] - \lim_{x \rightarrow 60^-} E[Y|T = 0, D = 0, X = x])
 \end{aligned}$$

as documented for instance in [\(Grembi, Nannicini and Troiano, 2016\)](#).

Suppose now that we wish to distinguish the employment response by the levels of hourly wages:  $\tau_{\bar{w}} = E[Y_{11} - Y_{10}|T = 1, X = 60, w = \bar{w}]$ . We do not observe potential wages but only realized ones. In other words, rather than observing the conditional labor supply at certain levels of hourly wages, we observe the joint distribution of wages and labor supply. Naively running the difference-in-discontinuities on a dummy equal to 1 if an individual is employed at the level of wage  $\bar{w}$  and 0 otherwise is equivalent to taking the following estimand for  $\tau_{\bar{w}}$ :

$$\begin{aligned}
\tau_{\text{naive}} &= E[Y, w_{T=1} = \bar{w} | T = 1, D = 1, X = 60] \\
&\quad - \lim_{x \rightarrow 60^-} E[Y, w_{T=1} = \bar{w} | T = 1, D = 0, X = x] \\
&\quad - \left( E[Y, w_{T=0} = \bar{w} | T = 0, D = 0, X = 60] \right. \\
&\quad \quad \left. - \lim_{x \rightarrow 60^-} E[Y, w_{T=0} = \bar{w} | T = 0, D = 0, X = x] \right) \\
&= P(Y, w_{T=1} = \bar{w} | T = 1, D = 1, X = 60) \\
&\quad - \lim_{x \rightarrow 60^-} P(Y, w_{T=1} = \bar{w} | T = 1, D = 0, X = x) \\
&\quad - P(Y, w_{T=0} = \bar{w} | T = 0, D = 0, X = 60) \\
&\quad + \lim_{x \rightarrow 60^-} P(Y, w_{T=0} = \bar{w} | T = 0, D = 0, X = x) \\
&= P(Y | T = 1, X = 60, w_{T=1} = \bar{w}) P(w_{T=1} = \bar{w} | T = 1, X = 60) \\
&\quad - \lim_{x \rightarrow 60^-} P(Y | T = 1, X = x, w_{T=1} = \bar{w}) P(w_{T=1} = \bar{w} | T = 1, X = x) \\
&\quad - P(Y | T = 0, X = 60, w_{T=0} = \bar{w}) P(w_{T=0} = \bar{w} | T = 0, X = 60) \\
&\quad + \lim_{x \rightarrow 60^-} P(Y | T = 0, X = x, w_{T=0} = \bar{w}) P(w_{T=0} = \bar{w} | T = 0, X = x)
\end{aligned}$$

Let's assume that  $\lim_{x \rightarrow 60^-} P(w_{T=0} = \bar{w} | T = 0, X = x) = P(w_{T=0} = \bar{w} | T = 0, X = 60)$  on the one hand, and  $\lim_{x \rightarrow 60^-} P(w_{T=1} = \bar{w} | T = 1, X = x) = P(w_{T=1} = \bar{w} | T = 1, X = 60)$  on the other, so that wage distributions in each time period are continuous at the threshold. Then:

$$\begin{aligned}
\tau_{\text{naive}} &= (P(Y | T = 1, X = 60, w_{T=1} = \bar{w}) - \lim_{x \rightarrow 60^-} P(Y | T = 1, X = x, w_{T=1} = \bar{w})) \\
&\quad \times P(w_{T=1} = \bar{w} | T = 1, X = 60) \\
&\quad - (P(Y | T = 0, X = 60, w_{T=0} = \bar{w}) - \lim_{x \rightarrow 60^-} P(Y | T = 0, X = x, w_{T=0} = \bar{w})) \\
&\quad \times P(w_{T=0} = \bar{w} | T = 0, X = 60)
\end{aligned}$$



$$\begin{aligned}
&= \left[ P(Y|T = 1, X = 60, w_{T=1} = \bar{w}) - \lim_{x \rightarrow 60^-} P(Y|T = 1, X = x, w_{T=1} = \bar{w}) \right. \\
&\quad \left. - \left( P(Y|T = 0, X = 60, w_{T=0} = \bar{w}) - \lim_{x \rightarrow 60^-} P(Y|T = 0, X = x, w_{T=0} = \bar{w}) \right) \right] \\
&\quad \times P(w_{T=1} = \bar{w}|T = 1, X = 60) \\
&+ \left[ P(Y|T = 0, X = 60, w_{T=0} = \bar{w}) - \lim_{x \rightarrow 60^-} P(Y|T = 0, X = x, w_{T=0} = \bar{w}) \right] \\
&\quad \times \left( P(w_{T=1} = \bar{w}|T = 1, X = 60) - P(w_{T=0} = \bar{w}|T = 0, X = 60) \right) \\
&= \tau_{\bar{w}} \times P(w_{T=1} = \bar{w}|T = 1, X = 60) \\
&+ \left[ P(Y|T = 0, X = 60, w_{T=0} = \bar{w}) - \lim_{x \rightarrow 60^-} P(Y|T = 0, X = x, w_{T=0} = \bar{w}) \right] \\
&\quad \times \left( P(w_{T=1} = \bar{w}|T = 1, X = 60) - P(w_{T=0} = \bar{w}|T = 0, X = 60) \right)
\end{aligned}$$

As can be seen in the expression above, the issue with this strategy is that the naive estimand is not equal to  $\tau_{\bar{w}}$ , nor to  $\tau_{\bar{w}}P(w_{T=1} = \bar{w})$ , which is arguably an object of interest, as it is equal to  $\tau_{\bar{w}}$ , weighted by the density of the corresponding level of wages<sup>19</sup>. The pre-reform discontinuity enters as a second term, weighted by the difference in densities of the level of wage of interest. Unless we can make the very strong assumption that the distribution of wages is stable over time, this term is different from 0 and biases our estimates of  $\tau_{\bar{w}}$ .

However, the existence of a pre-reform discontinuity is something we can check. Therefore, under the assumption the discontinuity in the absence of the reform, if any, is time-invariant, whether the difference-in-discontinuity strategy is required to estimate  $\tau$  can be decided upon by looking at the pre-reform discontinuity. Provided that we can assume that there is no discontinuity in the absence of the reform, the regression discontinuity framework is enough to estimate the aggregate treatment effect. This is helpful, as using the joint distribution of employment and wages in a simpler regression discontinuity framework is less problematic than in the difference-in-discontinuity framework. Indeed, it writes down as:

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<sup>19</sup>In any case, the latter density is observed, and can thus be estimated.

$$\begin{aligned}
 \tau_{RDD, \bar{w}} &= E[Y, w_{T=1} = \bar{w} | T = 1, X = 60] - \lim_{x \rightarrow 60^-} E[Y, w_{T=1} = \bar{w} | T = 1, X = x] \\
 &= P(Y, w_{T=1} = \bar{w} | T = 1, X = 60) - \lim_{x \rightarrow 60^-} P(Y, w_{T=1} = \bar{w} | T = 1, X = x) \\
 &= P(Y | T = 1, X = 60, w_{T=1} = \bar{w}) P(w_{T=1} = \bar{w} | T = 1, X = 60) \\
 &\quad - \lim_{x \rightarrow 60^-} P(Y | T = 1, X = x, w_{T=1} = \bar{w}) P(w_{T=1} = \bar{w} | T = 1, X = x)
 \end{aligned}$$

Assuming that  $\lim_{x \rightarrow 60^-} P(w_{T=1} = \bar{w} | T = 1, X = x) = P(w_{T=1} = \bar{w} | T = 1, X = 60)$ , then:

$$\begin{aligned}
 &= \left( P(Y | T = 1, X = 60, w_{T=1} = \bar{w}) - \lim_{x \rightarrow 60^-} P(Y | T = 1, X = x, w_{T=1} = \bar{w}) \right) \\
 &\quad \times P(w_{T=1} = \bar{w} | T = 1, X = 60) \\
 &= \tau_{\bar{w}} \times P(w_{T=1} = \bar{w} | T = 1, X = 60)
 \end{aligned}$$

Continuity of the potential wage distribution at the threshold achieves identification of  $\tau_{\bar{w}}$ .

Finally, note that in the case the difference-in-discontinuity is warranted (that is, when a pre-period discontinuity is observed), then both the RDD and naive difference-in-discontinuity strategies on the joint distribution of labor supply and wages will fail to yield a consistent estimate of the conditional treatment effect.



## Chapter 3

### Cash Transfers, Liquidity

### Constraints and Self-Employment

### in South Africa\*

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\*This chapter has benefited greatly from discussion with Luc Behaghel, Matteo Bobba, Gary Fields, David Margolis, Patrizio Piraino, Sara Tonini, Peng Zhang, and seminar participants at the Paris School of Economics.

## **Abstract**

This paper investigates the role of liquidity constraints, and indirectly risk aversion, as potential barriers to self-employment in South Africa, a country with strikingly low levels of entrepreneurial activities. By making use of two unconditional cash transfer programs, I show that, contrary to what is often found in the literature, recipient households do not increase self-employment when experiencing large, positive income shocks. Neither the arrival of expected, nor unexpected cash transfers leads to a higher level of self-employment among recipients. Consistently, I show that self-employment has stagnated in the past 20 years, despite a large increase in the number of cash transfer recipients. This indicates that other barriers to entry into self-employment are likely to be binding in South Africa. I present evidence that the lack of self-employment is likely a long-term consequence of Apartheid, and discuss avenues for future research on this phenomenon.

**JEL Codes:** L26, J62, I38

**Keywords:** self-employment; cash transfers, South Africa, intergenerational transmission

## 3.1 Introduction

The labor market of developing countries is often characterized by a high rate of self-employment, usually concentrated in the informal sector. With a few exceptions, this stylized fact is true virtually all over the developing world. However, South Africa is an outlier. With very low overall employment and high unemployment, its level of self-employment is among the lowest in developing countries (and also lower than many developed economies). To put this self-employment “gap” in perspective, South Africa’s has one-fourth of the self-employment rate of Brazil, despite similar GDP per capita, and one-third of the self-employment rate of Namibia, its neighboring country (see Figures 3.A.1 and 3.A.2 for more cross-country comparisons). Its level of self-employment is comparable only to ex-Soviet countries. This pervasive absence of entrepreneurial activities, in particular among the native African population, is puzzling, and may be one reason behind the country’s low employment rate.

This paper shows that, contrary to an established finding in other countries, relaxing liquidity constraints in South Africa does not lead to increases in self-employment. Indeed, large cash transfers to the household, either in the form of child grants or old age pensions, do not result in higher entrepreneurship among working-age individuals. These findings rule out liquidity constraints as the main reason for the absence of self-employment in South Africa, and suggest that other barriers might be binding. This paper also explores, descriptively, other potential causes for the low self-employment of South Africa, and puts forward an explanation that is consistent with the available evidence.

The literature on the labor market of developing countries has pointed out that self-employment can be a desirable occupational status, often more so than informal or even formal wage employment (see [Maloney \(1999\)](#), [Maloney \(2004\)](#), [Falco et al. \(2015\)](#), [Falco and Haywood \(2016\)](#)). Another important finding is that liquidity constraints can be a barrier to entry into business ownership in developing countries. [Blattman, Fiala and Martinez \(2013\)](#) identify this channel through a policy experiment in Uganda, where they show that recipients of an “unsupervised” grant are more likely to start their own businesses. [Bianchi and Bobba \(2013\)](#) show

that a conditional cash transfer targeted at children in Mexico increases parents' self-employment rate. These findings directly contrast with the results of this paper, where I show that this mechanism is not at play: South Africans do not respond to large, unconditional cash transfer by increasing entry into entrepreneurship.

Several explanations have been put forward to justify this well-known peculiarity of the South African labor market. South Africans perceive widespread crime as one of the main barriers to owning and running a business (Cichello et al. (2011)). However, recent empirical evidence has shown that this explanation has little support in the data: the correlation between crime rates and self-employment, both across time and geographical areas, is weak and small (Grabrucker and Grimm (2018)). Similarly, many have pointed to strict labor market regulation as a reason that prevents South Africans from entering self-employment activities. Magruder (2012) has investigated the role played by unions and centralized bargaining in discouraging (self-) employment. While he does find that strictly enforced collective bargaining rules decrease employment and self-employment, these effects remain marginal. Moreover, the self-employment rate among immigrants and Whites is three to four times higher than among Black or Coloured individuals born in South Africa (Figure 3.A.3), and this is true within and across municipalities, which is where these regulations are usually set. These facts suggest that overall labor market conditions are unlikely to be the only explanation behind the lack of self-employment activities among native South Africans.

By looking at South Africa's two largest cash transfer programs, the *Old Age Pension* (OAP) and the *Child Support Grant* (CSG), I am able to rule out even small positive effects on the probability to be self-employed. Because both programs have age-thresholds that have been significantly reformed over time, one can exploit exogenous variation to causally identify their labor market effects on recipients. There is no self-employment response for direct recipients (mothers or elderly), but also for other working-age adults of the same household. Cash transfers relax liquidity constraints, but may also have an "insurance" effect, and therefore increase willingness to bear risk, and enter self-employment (Baird, McKenzie and Özler (2018)). Falco

(2014) shows that more risk averse workers tend to queue for formal jobs instead of entering self-employment, because of the higher earnings volatility when owning a business. Similarly, Bianchi and Bobba (2013) underline the role of this mechanism by showing that entry into self-employment responds more to future cash transfers than to current ones. In the South African case, I am not able to cleanly disentangle the role of liquidity constraints from the effect through risk aversion. However, I show that both anticipated (OAP) and unanticipated (CSG) cash transfers do not result in higher self-employment. Intuitively, while an anticipated income shock should only capture the role of liquidity constraints, an unanticipated shock would combine both the effect on risk and liquidity. Hence, showing that neither seems to matter points against both channels as explanations for the low self-employment rate. Instead, I present evidence, discussed in Section 6, that the lack of self-employment in South Africa possibly has historical roots. Informal self-employment was strongly repressed during the Apartheid years; through the mechanisms of inter-generational transmission, this is likely to still have consequences today. Descriptive evidence on migrants to South Africa seems to support this hypothesis. I also discuss avenues for future research on this phenomenon, and potential policy implications.

The paper is organized as follows: Section 2 describes South Africa's two main cash transfer programs. Section 3 summarizes the channels suggested in the literature through which a cash transfer should impact self-employment. Section 4 provides descriptive statistics about self-employment in South Africa, and the data sources at hand. Section 5 presents the empirical analysis and the results. Section 6 discusses other potential explanations for the lack of self-employment in South Africa, and policy implications, while Section 7 concludes.

## **3.2 South Africa's Cash Transfer Programs**

Since the end of the Apartheid period, South Africa has put in place (or significantly extended) two main cash transfer programs, the *Child Support Grant* (CSG) and the *Old Age Pension* (OAP). Together, these programs account for a significant portion



of government spending (Gomersall (2013)). The CSG provides a small cash amount, around 50\$ 2010 PPP, per month for each child below a certain age threshold. This cash transfer is very extended, and covers a large portion of the population. In 2010, around 10 million children were beneficiaries of the CSG every month, and 5 million mothers received it (in a country, back then, of roughly 50 million people).<sup>1</sup> Although it is formally targeted to the “primary caregiver of the child”, in practice the transfer is always paid to women, and a large majority of the time to the biological mother. Instead, the OAP provides a large transfer, around 250\$ 2010 PPP, per month to elderly individuals above a certain age threshold. In 2010, around 2.5 million elderly received the OAP every month. The *Old Age Pension* was formally introduced in the early 1920s, but extended to the Black and Coloured population only in the 1990s, while the *Child Support Grant* was introduced in 1998, and rolled-out in the subsequent years. More than 30% of households in South Africa receive at least one of these transfers, and many receive both.<sup>2</sup>

Both programs are unconditional, meaning there are no conditionalities attached to these grants, and means-tested. However, the means-test has not always been consistently applied across programs. In particular for the CSG, there is no evidence that it was enforced during the first decade of the program (at least until the reform in 2008, when the means-test was set at 10 times the grant). How strict the application of the means-test has been for the OAP is also unclear, given that it is the applicant who has to provide proof of income. It has also been dramatically increased in more recent years, so that most of the population is eligible in any case. Both programs are also age-tested, and this criteria has always been binding and strictly applied. The age-thresholds, for both the CSG and the OAP, have been changed over time. This is what provides the sources of variation that are used for identification.

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<sup>1</sup> *Beneficiary* is the individual who is eligible for the grant, *recipient* is the person actually being paid the grant.

<sup>2</sup> For a full description of these programs and their history refer to the first chapter of this thesis for the *Child Support Grant*, and the second chapter for the *Old Age Pension*.

### 3.3 Conceptual Framework

To better understand the results of this paper, it is useful to think in terms of the model developed by [Bianchi and Bobba \(2013\)](#), who show formally how cash transfers impact entry into self-employment. I do not report here the model in full, but just its main conclusions, which are intuitive and helpful to think about the (absence of) results in the South African case.

[Bianchi and Bobba \(2013\)](#) consider agents in a two-period model who are heterogeneous in terms of their initial wealth and risk aversion. Individuals make occupational choice decision (wage- vs. self-employment) and savings decisions, with borrowing constraints. Self-employment has higher returns on average, but also higher variance; it is therefore the more profitable but also riskier occupation. Moreover, self-employment requires an initial capital investment, individuals who cannot make this investment end up either in wage jobs or unemployed.

In their model, the goal is to formalize how cash transfers impact entry into self-employment. There are two channels at play. On the one hand, cash transfers relax a *liquidity constraint*: individuals who were unable to make the fixed investment necessary to enter self-employment are more able to do so in the presence of a cash transfer. On the other hand, cash transfers also relax an *insurance constraint*: risk averse individuals are more likely to enter self-employment because cash transfers provide a guaranteed stream of resources. In order to disentangle these two channels, they show that individuals should respond more to the size of *current* cash transfers if liquidity constraints bind, or to the size of *future* cash transfers if insurance constraints bind, because those transfers insure against future earnings volatility. They use variation in the timing of a cash transfer in Mexico (*Progres*a) to disentangle these two channels, and show that future transfers have a larger effect on the probability to be self-employed.

In South Africa, there is no variation in the timing of the transfers that I can exploit in a similar way. However, I argue that differences in the effects of the CSG and OAP shocks could be interpreted along the same lines. The CSG shock for the

relevant cohorts is unexpected, as mothers whose child was not eligible to the transfer suddenly become so as a consequence of the age eligibility reforms. In the light of the aforementioned framework, it is a shock to both current and future transfers. As we are comparing mothers who are never eligible with mothers who are eligible for a few years, any positive effect on self-employment might go through both the liquidity and insurance channels. On the other hand, the OAP shock is fully expected. People know about the age eligibility threshold, hence what is captured is simply the fact of becoming eligible, net of previous adjustments. People just before the threshold know that they will soon receive the pension, but do not yet receive the transfer. Therefore, by comparing people just before and just after the threshold one should capture only the liquidity effect, as individuals/households who are about to become eligible have already internalized the insurance effect coming from the public pension.

The difficulty in this setting is that, contrary to [Bianchi and Bobba \(2013\)](#), we are looking at two separate transfers, which are different across many dimensions (size, targeting within the household, labelling). This significantly complicates the interpretation. If the two programs were identical, other than one was expected and one was not, it would be possible to net out the effect of relaxing only the insurance constraint. However, I argue the lack of a positive effect on self-employment for both transfers suggests that neither liquidity nor insurance seem to be the binding constraint preventing entry into self-employment in South Africa.

### **3.4 Data and Descriptive Statistics**

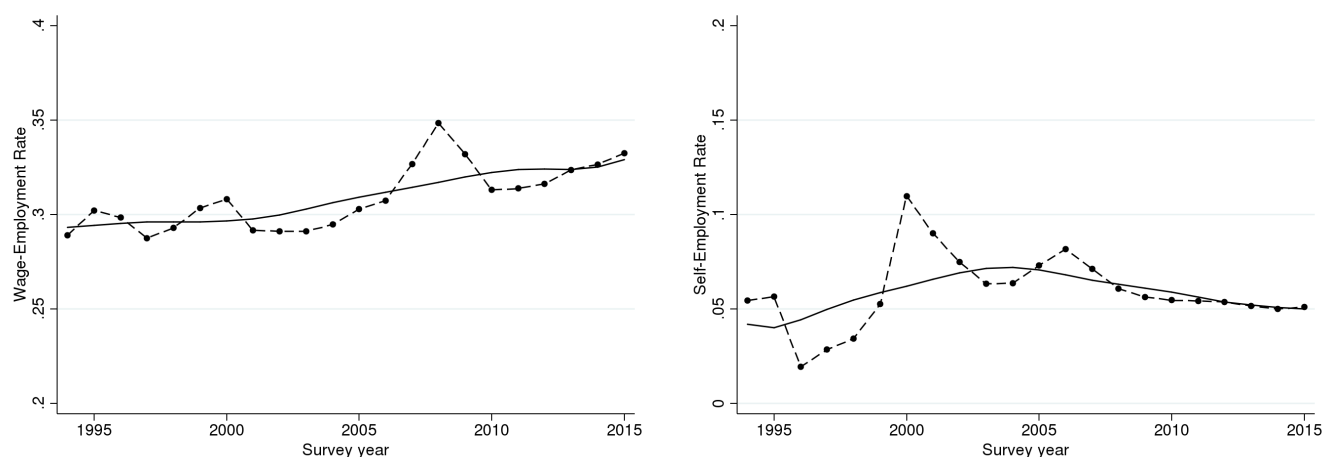
For the analysis of the impacts of the two cash transfers programs on self-employment, I make use of Census data, both the 2001 and 2011 wave, complemented by the Community Survey in 2007. This survey, carried out on purpose in between two Census waves, has a questionnaire that is virtually identical to the census and a large sample size. For simplicity, I refer to Census data as both the actual census and the Community Survey, given their high comparability. These are the only sources of data that allow to identify, at the same time, the impact of both the *Child Support Grant* and the *Old Age Pension*. Identification of the pension only requires data

with a discrete age variable, this is available in most South African datasets. By contrast, identification of the effects of the CSG requires information on fertility (and a large sample size), which is only available in the Census. Information on fertility (specifically, year of birth of the youngest child) is what determines whether a mother is treated or not. One could alternatively link child and mother in the household, but this would provide a much noisier measure of treatment status, as the probability of co-residence between mother and child declines as the child gets older, and could be endogenously determined by the grant itself. Moreover, the Census is the only source that allows to identify migrants to South Africa. I use this information in Section 7 to present evidence about the direct and intergenerational impacts of Apartheid on self-employment.

Figure 3.1: Wage- and Self-Employment Rates, 1994–2015

(a) Wage

(b) Self



*Note:* These graphs give the evolution of the wage-employment rate (left panel) and the self-employment rate (right panel) between 1994 and 2015. The sample is limited to the Black and Coloured population. The years 2000 and 2001 significantly over-measure self-employment as reported by [Neyens and Wittenberg \(2016\)](#).

*Source:* Author's calculations on PALMS data.

Therefore, Census data is suited for the majority of this analysis. To further document employment trends, I make use of the *Post-Apartheid Labour Market Series* (PALMS), an appended cross-section of all South Africa's main labor force surveys since 1994 to 2015, consistently coded and weighted. Figure 3.1 shows the evolution of the wage- and self-employment rates since the end of Apartheid until

2015, restricted to the Black and Coloured population.<sup>3</sup> While wage-employment has increased over the period by around 17%, self-employment has stagnated. The complete absence of growth in the number of self-employed is striking, given the very low self-employment rate in 1994. When considering that during the same period, South Africa experiences a dramatic growth in the number of cash transfer recipients<sup>4</sup>, the results from the next section should come as no surprise. The roll-out of large, unconditional cash transfers in South Africa has not led to a higher level of micro-entrepreneurial activities, and this is true in the aggregate, as it is at the individual level.

### **3.4.1 Measurement of Self-Employment**

Self-employment is generally measured by asking individuals whether they ran any kind of business in the past week. Alternatively, other surveys ask individuals directly about who is their employer (and an option is to work for one's self), or to identify directly as self-employed. In Table 3.A.1 in the Appendix, I show the level of self-employment according to the various survey sources in South Africa. All give very low levels of self-employment in any year, regardless of the definition of self-employment, or whether the surveys are run by the national statistical agency. This suggests that the lack of self-employment in South Africa is unlikely to be a measurement issue. Even in years when informal self-employment was significantly over-measured (2001 of the PALMS)<sup>5</sup>, the rate was never above 10%. Self-employment is almost entirely concentrated in the informal sector, but, in South Africa, it makes up less than half of it, as informal wage-employment is larger. Those in the formal sector, i.e. registered businesses, are an even smaller portion of employment, around 1% of the working-age population.

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<sup>3</sup>Including other population groups in South Africa (slightly) changes the levels but not the trend.

<sup>4</sup>The OAP is extended to the Black and Coloured Population in the early 1990s, while the CSG is formally introduced in 1999. There is a dramatic increase in the share of the population receiving the CSG in the early 2000s, as can be seen in Figure 3.A.4.

<sup>5</sup>This was the result of financial incentives for enumerators to find self-employed individuals (Kerr and Wittenberg (2015)).

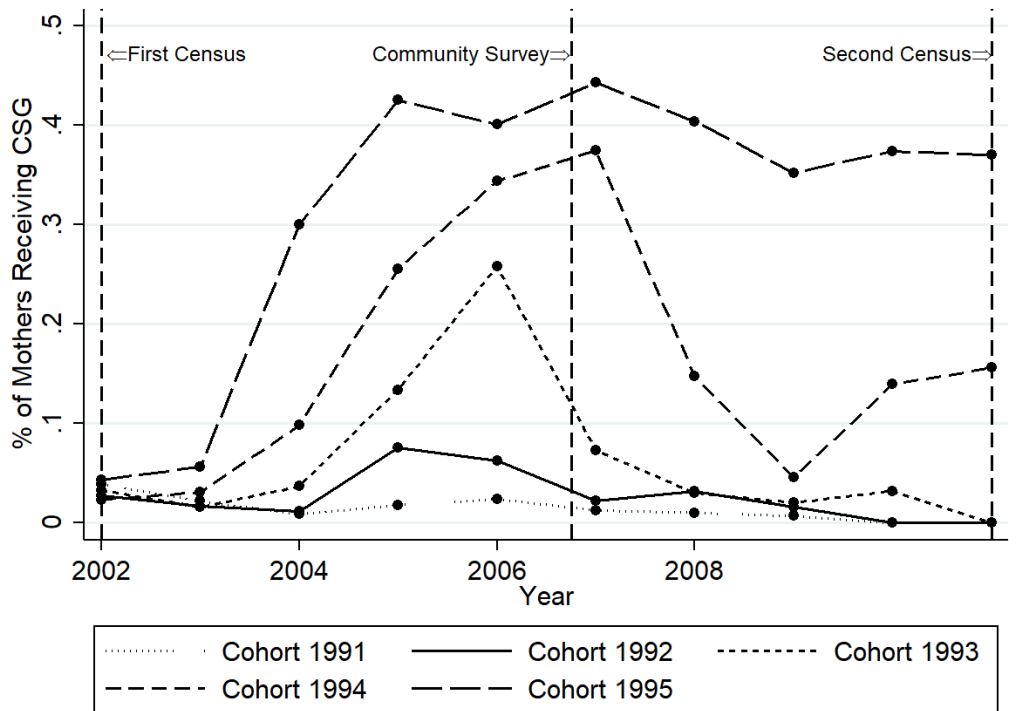
## 3.5 Empirical Analysis

This paper exploits exogenous variation in eligibility to the CSG and the OAP to test whether recipients (or members of recipients' households) are more likely to enter self-employment. The identification strategies are different for the two programs, but similarly exploit variation created by changes in age conditions for eligibility. Conceptually, the important difference in the liquidity shocks introduced by these two programs is whether they are expected or not. While the age threshold for the OAP is “expected”, meaning that households know exactly when a family member becomes eligible for the pension, the shock used to identify the effect of the CSG was unexpected. Therefore, while we could interpret the effect of the OAP as a pure liquidity effect at the household level, the effect of CSG will combine both the liquidity effect, and the “insurance” effect, meaning that it might also increase willingness to bear risk.

### 3.5.1 Effect of the *Child Support Grant* on Self-Employment

As in the first chapter of this thesis, to identify the effect on the *Child Support Grant*, I exploit the large variation in terms of eligibility across birth years of the child. Mothers whose youngest child was born before 1993 are virtually never eligible to the grant, while those whose youngest child is born in 1993 or after are eligible for at least one year. Children born after 1993 are “treated” for increasingly longer periods of time; those born in 1994, for example, are eligible on average for around 3 years between 2001 and 2011. Figure 3.2 gives the share of mothers receiving the *Child Support Grant* depending on the year the child was born, between 2002 and 2010. As outlined in Chapter 1, the identification strategy relies on the fact that there is a last unexposed cohort, those born in 1992, and a first exposed cohort, those born in 1993, which have large differences in CSG receipt. I run the following specification on data from the 2001 census (before), 2007 Community Survey (during), and 2011 census (after), which is meant to capture the effect of receiving at least one year of

Figure 3.2: Share of Mothers Receiving the CSG by Cohort of Birth, 2002–2011



*Note:* This graph gives the share of mothers receiving the CSG by year of birth of the youngest child for the period 2002–2011. The graph is representative of mothers whose child is born in January of each year, see Chapter 1 for explanation. 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.

grant:

$$SE_i = f(c_i - 1993) + \mathbb{1}\{c_i \geq 1993\} \times f(c_i - 1993) + \beta_{CSG} \mathbb{1}\{c_i \geq 1993\} + \varepsilon_i \quad (3.1)$$

where  $SE_i$  is a binary variable equal to one if individual  $i$  is self-employed;  $f$  is a function of the cohort of the youngest child centered at the cut-off point. In order to maximize power, and be able to observe an effect on self-employment, the preferred window is the one of cohorts going from 1987 to 1998, which is the largest window where a linear function is preferable to a quadratic according to an Akaike Information Criterion (AIC).  $\mathbb{1}\{c_i \geq 1993\}$  is a binary variable equal to 1 when: 1) the mother’s youngest child is born in 1993 or later, when restricting the specification to mothers only; 2) the latest birth in the household occurred in 1993 or later, when running the regression on other working-age household members.

The proposed specification captures the effects of being eligible to the CSG for at least one year, and has the advantage of limiting the bias from age effects. Alternatively, one can run a diff-in-diff specification that allows to explore the large variation in the length of eligibility across cohorts. This allows to test whether being eligible for more years has a larger (or any) impact on the probability to be self-employed. In this case, the specification is the following:

$$SE_{it} = \alpha_0 + \alpha_1 \mathbb{1}\{c_i = c\} + \sum_{t=1}^2 \delta_t \mathbb{1}\{year = t\} + \mathbb{1}\{c_i = c\} \times \sum_{t=1}^2 \beta_{CSG,t} \mathbb{1}\{year = t\} + \varepsilon_{it} \quad (3.2)$$

where  $\mathbb{1}\{c_i = c\}$  is a binary variable equal to one if the youngest child is born in a given cohort,  $c$ . As a control group, I arbitrarily choose those whose youngest child is born in 1991 and 1992, hence the last cohorts not to receive the CSG. I then interact this with a variable indicating the year to get the evolution of the relative difference over time.

Graphical evidence that the CSG did not create a discontinuity in the self-employment rate is presented in Figures 3.3 and 3.4. Table 3.1 gives the RD coefficient of Equation 3.1 on the probability of being self-employed. Those mothers who have received at least one year of the grant are as likely to be self-employed as mothers who never have. There is no evidence of neither a short-term, nor a long-term effect. The lack of an effect on recipient mothers might be specific to this group, which, as shown in Chapter 1, is impacted in terms of their labor market outcomes, but not in terms of self-employment. However, I also find no evidence that the CSG increases entry into self-employment for other household members who are not the direct recipients of the grant. Adult members of households that receive (or have received) the grant are not more likely to engage in self-employment activities. It should be noted that the particularly low level of self-employment in the 2001 Census is the result of different measurement from the 2011 wave and the Community Survey. In the first Census wave, individuals self classify as self-employed, rather than being asked whether they run their own business or not. As Figure 3.1 shows, when consistently measured, we observe no significant increase in self-employment



since the early 2000s.

Table 3.1: Child Support Grant and Self-Employment, Linear Fit 1987–1998, RD Results

	<i>Self-Employed</i>			
	(1)	(2)	(3)	(4)
CSG	0.0028 (0.0051)	-0.0012 (0.0032)	0.0032 (0.0055)	-0.0019 (0.0037)
Year	2007	2011	2007	2011
Sample	<i>Direct Recipients</i>		<i>Other HH Members</i>	
Observations	43,927	112,093	49,985	124,158
R-squared	0.0176	0.0094	0.0148	0.0123

*Note:* \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. This table gives the results of Equation 3.1 with a **linear function** on the cohort window 1987–1998. I only report the coefficient of interest,  $\beta_{CSG}$ . The dependent variable is always a binary variable equal to one for individuals who are self-employed. Robust standard errors clustered at the household level in parentheses.

*Source:* Author’s calculations on Community Survey (2007) & Census (2011).

It is possible that one year of grant is not a large enough shock to induce a self-employment response. The coefficients of the Diff-in-Diff estimation of Equation 3.2 are shown in Figure 3.A.5. If we exploit the large variation in duration across treated cohorts to test whether longer duration has a positive effect on self employment, the results are virtually identical. Households with children treated for several years do not increase their self-employment any more than household that are never eligible to the grant. For example, households with a youngest child born in 1995<sup>6</sup>, who is eligible for the CSG uninterruptedly between 2004 and 2011, have the same evolution of the self-employment rate as households who never receive the grant.

### 3.5.2 Effect of the *Old Age Pension* on Self-Employment

To identify the effect of the OAP, I exploit the 2008–2010 reform in the age-eligibility for men, in the same way as outlined in the second chapter of this dissertation. Starting from 2008, the age threshold for men was lowered from 65 to 60 in 2010,

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<sup>6</sup>For one child, this is equivalent to roughly 4200\$ '10 PPP over 7 years.

Figure 3.3: Self-Employment Rates and the Child Support Grant, Mothers, 2001 and 2011 and 2011

(a) 2001 - “Before”

(b) 2007 - “During”

(c) 2011 - “After”



*Note:* These graphs give the probability of being self-employed as a function of the year of birth of the youngest child, for Black and Coloured mothers. Mothers of children to the left of the threshold never receive the *Child Support Grant*, those to the right have received at least one year of grant by 2011. A linear function is fitted on both sides of the threshold with 95% confidence intervals.

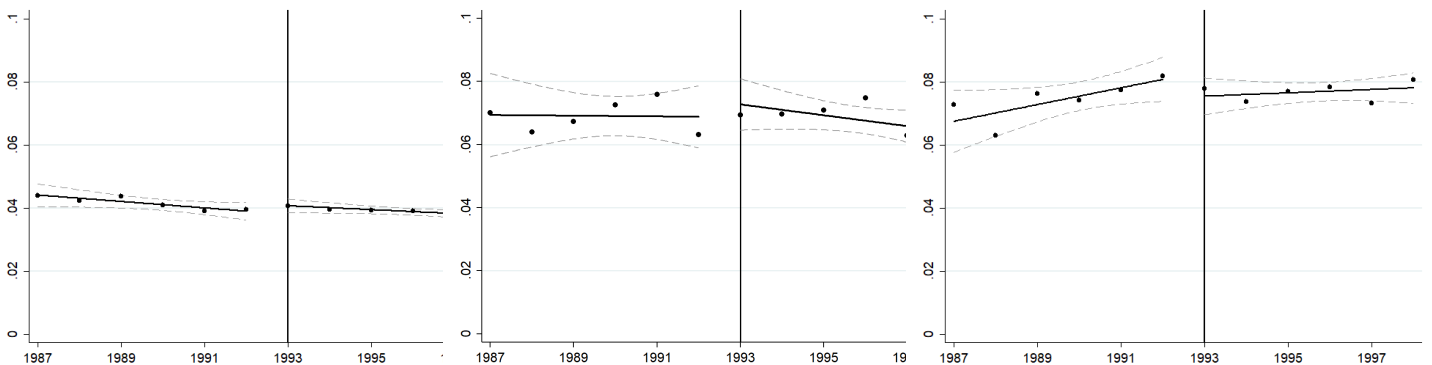
*Source:* Author’s calculations on Census (2001 & 2011), and Community Survey (2007).

Figure 3.4: Self-Employment Rates and the Child Support Grant, Other Household Members, 2001 and 2011

(a) 2001 - “Before”

(b) 2007 - “During”

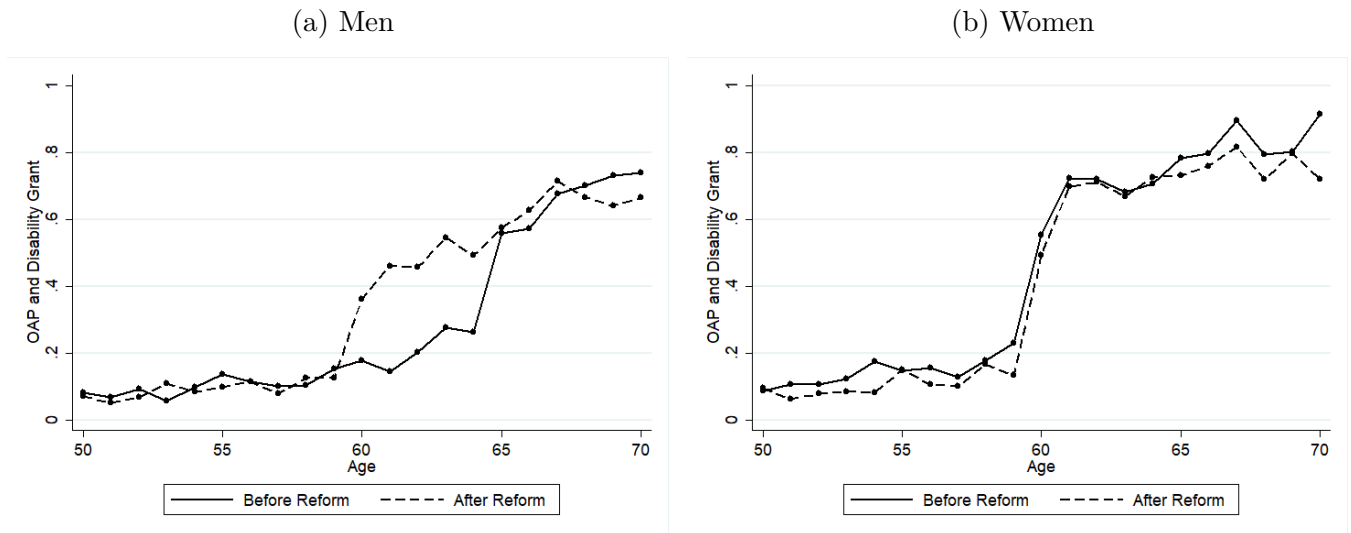
(c) 2011 - “After”



*Note:* These graphs give the probability of being self-employed as a function of the year of birth of the youngest child in the household, for household members (who are not the mother) born before 1978. A linear function is fitted on both sides of the threshold with 95% confidence intervals.

*Source:* Author’s calculations on Census (2001 & 2011), and Community Survey (2007).

Figure 3.5: Share Receiving *Old Age Pension* or *Disability Grant* by Age, **Before and After** Pension Reform



*Note:* These graphs plot the share of individuals receiving the *Old Age Pension* or the *Disability Grant* within each age bin, for men and women separately. The solid line plots the share receiving before the reform (2007), while the dotted line for the years after the reform (2011).  
*Source:* Authors' calculations on GHS (2007 & 2011).

while it was left unchanged for women. The effect of this reform can be clearly observed in Figure 3.5: the share of men between 60 and 64 receiving the pension increases dramatically. The reform allows to estimate the effect of the OAP without mixing in the effect of other, private pension schemes with the same age thresholds. In order to capture the effect of becoming eligible on self-employment, I run the following specification on data from the 2001 (before) and 2011 (after) census:

$$SE_i = f(AgeOM_i) + f(AgeOM_i) \times \mathbb{1}\{AgeOM_i \geq 60\} + \beta_{OAP} \mathbb{1}\{AgeOM_i \geq 60\} + \varepsilon_i \quad (3.3)$$

where  $SE_i$  is again a dummy for self-employment. The forcing variable,  $AgeOM$ , is a linear function of the *age of the oldest man* within the household. As the oldest man is virtually always older than the oldest woman, this is what determines whether the household receives at least one OAP. Therefore, the coefficient of interest is the  $\beta_{OAP}$  estimated in 2011, after the reform. One could alternatively run a specification that takes into account the change in threshold, and estimate directly the difference in the discontinuities before and after. As this is significantly more demanding in

terms of statistical power, to increase the chances of being able to observe an effect on self-employment, I recur here to a simpler RDD specification, with the age of the oldest man as a discrete running variable, and qualitatively compare the coefficients in the different years.

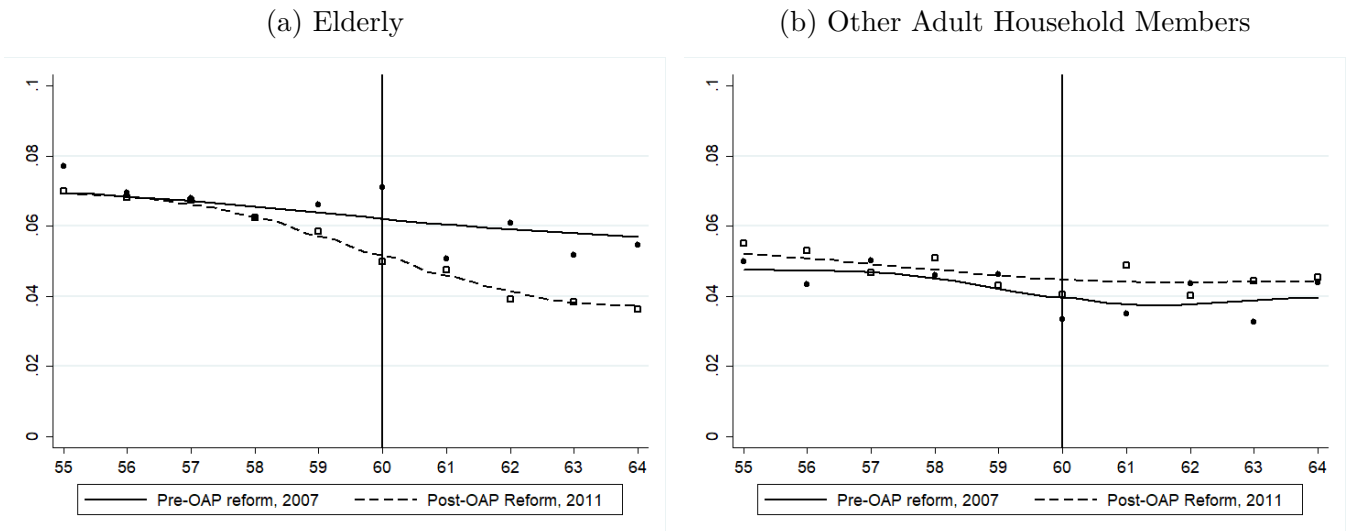
Alternatively, I also compare the relative difference, before and after, of unaffected and affected age groups. This is the estimation that allows the maximum statistical power, while relying still on a reasonable identification assumption: that the evolution of affected and unaffected age groups would have been the same in the absence of the reform. Formally, I estimate the following specification for people aged between 55 and 65 (or for those other adults in households where the oldest male is between 55 and 65):

$$SE_{it} = \alpha_0 + \alpha_1 \mathbb{1}\{AgeOM_i \geq 60\} + \alpha_2 \mathbb{1}\{year = 2011\} + \beta_{OAP} \mathbb{1}\{AgeOM_i \geq 60\} \times \mathbb{1}\{year = 2011\} + \varepsilon_{it} \quad (3.4)$$

where  $\mathbb{1}\{AgeOM_i \geq 60\}$  indicates the treated age groups, and  $\mathbb{1}\{year = 2011\}$  the only year after the reform in Census data. The coefficient of the interaction term,  $\beta_{OAP}$ , captures the relative evolution in the self-employment rate of the treated group.

The lack of an effect of the OAP on self-employment is clear in Figure 3.6. After the reform, self-employment among elderly individuals drops for those who become eligible to the pension, but, at the same time, it does not increase for those other adult members in pension-eligible households. The findings indicate that: similarly to evidence on the CSG, 1) the relaxation of liquidity constraints at the household level that comes with the pension does not lead to higher self-employment; additionally, 2) those jobs “left” by older South Africans who receive the pension are not necessarily picked-up by younger household individuals. However, I can only reject effects slightly smaller than a one-to-one substitution within the household of these self-employment jobs. It is still possible that there are smaller increases that I do not have sufficient statistical power to identify. Results in regression form are in Table 3.2 for the

Figure 3.6: Self-Employment by Age of Oldest Man in the Household, Before and After Reform



*Note:* These graphs give the rate of self-employment by age of the oldest man in the household, for the elderly in the left panel, and for other household working-age members in the right panel, both before the pension reform (2007) and after (2011). The reform lowered the public pension age of men from 65 to 60.

*Source:* Community Survey (2007) and Census (2011).

RD estimation, and in Table 3.3 for the DiD. Both specifications show that treated elderly men decrease their probability of being self-employed by between 0.7 and 1 pp., which is around one fourth of the total employment response of the elderly presented in Chapter 2. Again, we observe no evidence of a statistically significant increase in self-employment for other adult members within the household.

I present several robustness checks for the results both on the CSG and OAP in the Appendix, in the same spirit as those of Chapter 1 and 2. For the RD estimations, I check the robustness to different bandwidths, while for the DiD I check whether changing the composition of the treatment group affects the results. The point estimates are always small and centered around zero. For either program, there is no specification that shows a positive impact on self-employed. The only estimated effect is the decrease in the self-employment rate among pension-eligible elderly, which is robust across specifications and bandwidths.

Table 3.2: *Old Age Pension* and Self-Employment, Linear Fit 55–64, RD Results, Census

	<i>Self-Employed</i>			
	(1)	(2)	(3)	(4)
$\mathbb{1}\{AgeOM_i \geq 60\}$	-0.0006 (0.0029)	-0.0066** (0.0031)	0.0027 (0.0021)	0.0011 (0.0029)
Year	Before	After	Before	After
Sample	<i>Direct Recipients</i>		<i>Other HH Members</i>	
Observations	59,922	89,956	62,446	85,000
R-squared	0.0010	0.0028	0.0003	0.0006

*Note:* \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . This table gives the results of Equation 3.3 on the PALMS with a **linear function** on the age window 55–64. I only report the coefficient of interest,  $\beta_{OAP}$ . The dependent variable is always a binary variable equal to one if the individual is self-employed. Robust standard errors clustered at the household level in parentheses.

*Source:* Author's calculations on Census (2001 & 2011).

Table 3.3: *Old Age Pension* and Self-Employment, 55–65, DiD Results, Census

	<i>Self-Employed</i>	
	(1)	(2)
$\mathbb{1}\{AgeOM_i \geq 60\} \times \mathbb{1}\{year = 2011\}$	-0.0112** (0.0044)	0.0037 (0.0036)
Sample	<i>Direct Recipients</i>	<i>Other HH Members</i>
Observations	108,181	102,380
R-squared	0.0018	0.0004

*Note:* \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . This table gives the coefficient  $\beta_{OAP}$  of Equation 3.4 on direct recipients in Column (1), and other adult household members in Column (2). The dependent variable is always a binary variable equal to one if the individual is self-employed. Robust standard errors clustered at the household level in parentheses.

*Source:* Author's calculations on Census (2011) & Community Survey (2007).

## **3.6 Further Evidence**

The results of this paper leave the question open of why South Africans do not enter self-employment: what barriers are there (if any) that impede the start of small informal businesses? This remains a key question to solve (some of) the issues of the South African labor market, namely its persistently high unemployment rate. This paper provides evidence that liquidity constraints are unlikely to be the main barrier to self-employment in South Africa. Large cash transfers to the household do not increase self-employment. Importantly, they also do not significantly decrease it, implying that self-employment in South Africa is not simply absorbed by the Welfare state. Indeed, we observe that households/individuals who do not receive any of these cash transfers still report very low levels of entrepreneurial activities.

To recapitulate, we can list three many facts about self-employment in South Africa, which help to frame the following discussion: 1) First, the absence of self-employment activities is confined to the native Black and Coloured population (which makes up around 80% of the total population of South Africa, hence the low rate overall). Other groups of the population have much higher levels of self-employment. This is generally true for the White population, but also for migrants from other African countries. In general, this shows that there are individuals for whom it is possible to be self-employed in the labor market of South Africa. While the high-end entrepreneurship among the White population might be of a completely different nature, the self-employment that we observe among non-native South Africans is often the kind of low-earning, informal self-employment prevalent in several developing countries. Therefore, any convincing explanation for this phenomenon has to reconcile why migrants from other African countries are able to enter self-employment activities, and native South Africans are not. 2) The lack of self-employment among Black and Coloured is a nation-wide phenomenon. The rate of entrepreneurial activities is low across provinces and municipalities (Figure 3.A.9), suggesting that local labor market conditions or regulations are not a satisfactory explanation. 3) Lastly, self-employment is low regardless of the level of (formal) human capital. Indeed, we observe that the probability of having a business is a decreasing function of years

of education (Figure 3.A.8). More precisely, the probability of owning a business is increasing in absolute terms, but less than wage-employment, so that the share of self-employment actually decreases with education. This is the opposite of what we observe for the White or for the migrant population, for whom the relative probability of being self-employed increases in the number of years of schooling.

### **3.6.1 Apartheid and Self-Employment**

The fact that self-employment is not increasing in physical or human capital is puzzling, and suggests that barriers to entry into self-employment are of a different nature. It is natural to look at South Africa's troubled political and economic history for an explanation. Indeed, Apartheid potentially provides an explanation that is both nation-wide, yet confined to the native Black and Coloured population, to whom restrictions on political and economic activities were applied. Indeed, self-employment, both formal and informal, was a largely inaccessible occupational choice for Africans during the Apartheid years (Rogerson (1986), Rogerson (2003)). Rogerson (2000) states that "*policymakers either largely neglected the SMME economy or, in the case of black-owned informal enterprise, actively discouraged the SMME economy through negative or repressive measures. Historically, under apartheid, the minority white-run government, at both national and local levels, opposed the informal economy, especially the black informal entrepreneur.*" The figure of the informal black entrepreneur was therefore largely absent from the South African labor market during the Apartheid period. Moreover, the many legal restrictions to the general freedoms of the black population also made formal business ownership extremely complicated (Fauvelle (2009)). For these reasons, the hypothesis has been put forward that the lack of self-employment among the Black and Coloured population today is a long-term consequence of this past repression. As stated by Kingdon and Knight (2007):

*"The lack of African self-employment is, to some extent, a legacy of apartheid. Historically the apartheid system repressed the informal activities of black South Africans through such restrictive legislation as*



*the Group Areas Act, harsh licensing, strict zoning regulations, and effective detection and prosecution of offenders. Bouts of slum clearance and other periodic attacks on the illegal spaces within which informal enterprise thrived, served to rid South African cities of black-dominated informal sector niches... Moreover, **repression and disempowerment of Africans under apartheid would have inhibited the development of entrepreneurial and social skills and of social networks.** These factors are important for confidence in entering the self-employed sector and for success in it.”*

There are several reasons to believe that the effects of this past repression would persist onto younger generations, who have not been directly affected by the Apartheid years. The literature in socio-economics has relentlessly pointed out the importance of the intergenerational component in the transmission of entrepreneurship and self-employment (Sørensen (2007), Parker (2009), Lindquist, Sol and Van Praag (2015)). Children of entrepreneurs are systematically more likely to be self-employed themselves (Lindquist, Sol and Van Praag (2015)).<sup>7</sup> Moreover, there is evidence that inheritance of the business or wealth effects are not the most important factors in explaining this correlation (Parker (2009)), and that post-birth transmission factors are twice as important as biological ones (Lindquist, Sol and Van Praag (2015)).

Sørensen (2007) identifies three channels in the intergenerational transmission of entrepreneurship: an 1) *aspirations/awareness* (role modelling) channel, whereby children of entrepreneurs are more likely to consider this form of employment than those who have not been exposed to an entrepreneurial role model. Moreover, higher 2) *physical or social capital*, as those with entrepreneurial parents have higher resources necessary to start a business, or directly inherit the business itself. This channel does not only include the direct transmission of wealth, but also those networks that might facilitate entry and/or increase returns to self-employment. Lastly, 3) *entrepreneurial skills*, as children of entrepreneurs might possess skills (human capital) that make them more successful in running a business, which in turn

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<sup>7</sup>“Having an entrepreneur for a parent increases the probability that a child ends up as an entrepreneur by a factor of 1.3 to 3.0” Lindquist, Sol and Van Praag (2015).

may also incentivize entry at the extensive margin. [Sørensen \(2007\)](#) and [Lindquist, Sol and Van Praag \(2015\)](#) find evidence of all these channels at play, but both point to role modelling and awareness as the most important channel.<sup>8</sup>

These findings are limited to the context of developed countries, where entrepreneurship (self-employment) is of a different nature. The extent to which these conclusions carry through to developing countries, where much less is known about the mechanism of inter-generational transmission, is unclear. Nonetheless, if we acknowledge that these mechanisms might be at play to a certain extent in the South African context, the persistent absence of self-employment up to this day should not be surprising. The importance of a the inter-generational component in the transmission of self-employment necessarily implies a strong correlation over time. Almost mechanically, if self-employment was pushed to zero, or to very low levels, for several generations, recovery would not occur automatically. Another way of thinking about this is in terms of the role-modelling mechanism, which has been shown to be central. Most young South Africans today did not have self-employed parents, for the simple reason that their parents could not be self-employed. It follows that this channel could not have been at play for the current generations, which lack, at the very least, this direct role-modelling channel through the parents.

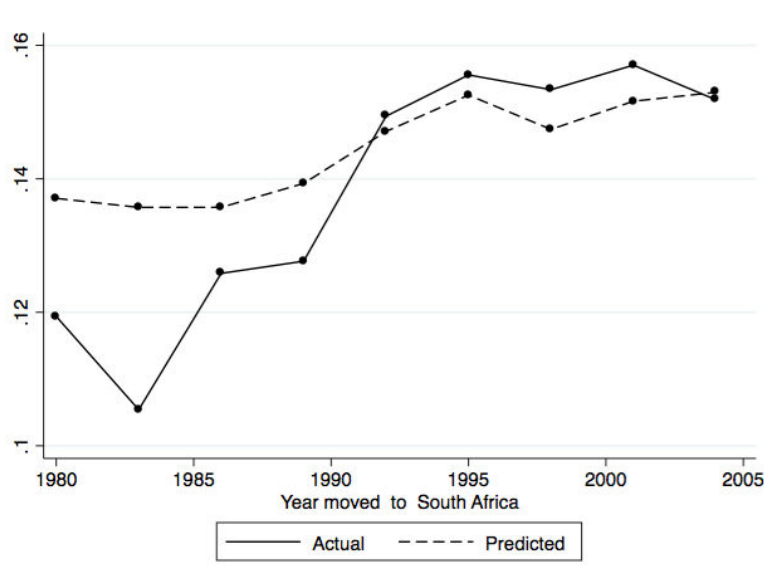
### **3.6.2 Evidence on Migrants to South Africa**

These two propositions, that i) the low self-employment rate is a long-term consequence of Apartheid and that ii) inter-generational transmission plays a role, can partly be examined by looking at migrants to South Africa. In fact, this is the only group that has some variation in terms of their exposure to Apartheid, defined by the timing of migration. Migrants who arrived in South Africa before the 1990s entered a labor market where informal self-employment was still actively discouraged ([Kingdon and Knight \(2007\)](#)). Instead, as from the 1990s, these restrictions were

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<sup>8</sup>[Lindquist, Sol and Van Praag \(2015\)](#) uses the gender component to disentangle the role modelling mechanism. [Sørensen \(2007\)](#) uses the timing of the entrepreneurship spell in the parents' life. [Sørensen \(2007\)](#): "The results suggest that parental role modelling is an important source of the transmission of self-employment. However, there is little evidence to suggest that children of the self-employed enter self-employment because they have privileged access to their parent's financial or social capital, or because they have superior entrepreneurial abilities."

Figure 3.7: Self-Employment Rate in 2011 by Year of Migration to South Africa



*Note:* This graph plots the self-employment rate by the year when the individual moved to South Africa. The dotted line draws the “predicted” rate of self-employment based on country of birth, district of residence, age, education, race, gender, and urban/rural status.

*Source:* Author’s calculations on the Census 2011.

gradually removed. This allows me to check whether migrants who arrived later are more likely to be self-employed. Most permanent migrants from other African countries, who are still in South Africa in 2011, arrive as from the 1980s (Figure 3.A.11). During the Apartheid years, the government strictly discouraged migrants from other African countries to stay, while it promoted permanent immigration from Whites, through what has been called a “two-gates” policy (Siddique (2004)).

Indeed, we observe that migrants who entered South Africa after the 1990s have a higher self-employment rate (Figure 3.7 and Table 3.4), and this is not explained by socio-demographic observables, including the country of migration or district of current residence. While this cannot be interpreted as more of a correlation, it matches the prediction that migrants who arrived after Apartheid (hence after the end of the self-employment restrictions) have, to this day, a higher self-employment rate. There is certainly the issue that the year of migration is a strongly endogenous variable, as individuals with a high self-employment “potential” might have decided to migrate after the restrictions were lifted. Alternatively, if instead of the year of migration one uses the year of birth, which is a strong predictor of having migrated before 1990s (Figure 3.A.10), this pattern still holds. Indeed, in Figure 3.A.12, we

see that the self-employment rate tends to be lower for migrants born before 1970s, and that this difference persists over time. Interestingly, this is not a pattern that we observe for natives, or for wage-employment, which is instead a smooth function of age (Figure 3.A.13).

Table 3.4: Correlations of Self- and Wage- Employment with Year of Migration, and with Father's Year of Migration

	Migrants to South Africa					
	Self	Wage	Self	Wage	Self	Wage
<i>Year Moved</i>	0.0020*** (0.0002)	-0.0006 (0.0004)	0.0022*** (0.0003)	0.0004 (0.0004)	0.0009*** (0.0003)	0.0003 (0.0005)
Socio-Demographic Controls	No	No	Yes	Yes	Yes	Yes
Geographic Controls	No	No	No	No	Yes	Yes
Observations	44655	44655	44655	44655	44655	44655
$R^2$	0.0012	0.0001	0.0213	0.0898	0.0455	0.1139

	<i>Children of</i> Migrants to South Africa					
	Self	Wage	Self	Wage	Self	Wage
Father's <i>Year Moved</i>	0.0013** (0.0006)	0.0008 (0.001)	0.0009 (0.0006)	0.0010 (0.0009)	0.0012* (0.0006)	0.0006 (0.001)
Socio-Demographic Controls	No	No	Yes	Yes	Yes	Yes
Geographic Controls	No	No	No	No	Yes	Yes
Observations	3,464	3,464	3,464	3,464	3,464	3,464
$R^2$	0.0297	0.0003	0.0598	0.1813	0.1410	0.2196

*Note:* \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. *Year Moved* is a linear function of the year when the person (or the father) has moved to South Africa. Only individuals who (whose father) have moved between 1980 and 2005 are kept in the sample. Column (1) & (2) presents the OLS estimates of regressing *Year Moved* on self- and wage- employment, with no controls. In column (3) & (4), controls for age, sex, education, race, and urban status are included. In column (5) and (6), controls for country of birth and district of residence are included. The bottom panel only includes individuals over 15 years old who share an household with their father. Robust standard errors in parentheses. *Source:* Author's calculations on Census 2011.

In a similar way, if the mechanism of inter-generational transmission is at play, we would expect children of people who migrated later to enter more into self-employment. This is indeed the case, as shown in Table 3.4, and in Figure 3.A.14 when using the year of birth of the father. There is a strong positive correlation between the self-employment probability and the year of birth of the father (which implies later migration), which is again a pattern that is not observed for South

African natives.

Although only descriptively, this evidence reinforces the two hypotheses mentioned before about the direct effect of Apartheid on self-employment, but also of its lasting consequence on younger generations. Migrants who arrived as from the 1990s are more likely to self-employed, and so are their children.

### **3.6.3 Policy Implications**

This conclusion has important policy implications, first because it suggests that there is room (and a justification) for policy action. Although the restrictions of the Apartheid years have been lifted, one implication of this argument is that the self-employment rate in South Africa is unlikely to recover on its own to a “normal” level. Without some sort of policy intervention, meant to supplement for the lack of inter-generational transmission, the level of self-employment in South Africa is likely to remain low. The trend since the end of Apartheid seems to confirm this intuition: there is no evidence of any growth in the number of self-employed individuals in the past 20 years. This despite a doubling of GDP per capita since 1994, and the entry into the labor market of generations of young workers born and educated entirely after the end of Apartheid.

This gloomy view, while unfortunately consistent with the stagnation in self-employment, contrasts with the optimistic predictions at the turn of the century, which forecast upcoming growth of the informal sector and, in particular, self-employment. Indeed, it was widely believed that the informal sector would flourish as the anti-micro business environment of Apartheid was wiped away:

*“..South Africa’s informal economy is in the process of coming out of the dark shadows cast by forty years of apartheid planning. The reconstruction initiatives of post apartheid South Africa, promoting and supporting the development of the small, medium and micro-enterprise (SMME) sector, including the informal economy, have been of major policy significance.”*

*“The changing informal economy of Gauteng signals that South Africa’s*

*informal economy is set to emerge from the shadows cast by the restrictions on entrepreneurship imposed during the apartheid period.” (Rogerson (2000))*

This did not happen. The only outcome that dramatically increased during the period, alongside participation in the labor market, is unemployment (Banerjee et al. (2008)). Despite the formal policy shift towards promoting and encouraging Black micro-entrepreneurship, self-employment has remained low, and the informal sector small. The fundamental flaw in this strategy was the belief that switching to a pro (micro-) business strategy would be enough to undo the damage of the Apartheid years. As argued before, the mechanism of inter-generational transmission implies that it should not necessarily be the case. The staggering lack of growth in the number of self-employed jobs over the past 20 years, which drastically contradicts this optimistic prediction, supports this intuition.

### 3.6.4 Implications for Future Research

Among the channels listed by Sørensen (2007), it is challenging to identify through observational data exactly what component of inter-generational transmission might be missing for South Africans. Future research on self-employment in South Africa should try to disentangle which are (if any) the binding constraints that are likely to make a difference. The empirical evidence presented in this paper helps to rule out liquidity constraints as the *only* barrier to entry into self-employment, but it is very possible that these constraints are nonetheless binding; a conservative interpretation is that relaxing them is not enough to induce self-employment responses because other constraints are also binding.

Policy interventions promoting self-employment in South Africa should focus on understanding whether South Africans do not consider self-employment as an option when looking for a job, or whether they do but do not manage to enter it. This would suggest a bundled intervention that would simultaneously relax: i) the lack of awareness of self-employment as a potential occupational choice due to a lack of role models; ii) the lack of pre-existing networks that help entry into

self-employment; iii) the lack of entrepreneurial skills, meaning those basic skills necessary to run a business or that increase returns to business ownership. On top of this, iv) relaxing liquidity constraints once these other constraints are lifted could enhance the (presumed) effectiveness of these interventions. Most interventions on self-employment/entrepreneurship, which are extremely common in development economics, usually focus on people who already own a business (or aspire to do so). This is not the main margin of interest in South Africa, or at least not what is specific about the South African case.

Nonetheless, the questions remains of whether self-employment promotion in South Africa is a priority, or other interventions, for example easing access to good formal jobs, are more relevant. I argue that the results of this paper provide at least some evidence that it should be a prime concern. If one agrees with the premise that the lack of self-employment in South Africa has historical roots, this also implies that the absence of entry into self-employment is likely to be largely involuntary. Another main motivation is that the lack of self-employment has not translated into higher wage-employment, but into higher unemployment. South Africa's level of wage employment is not higher than that of countries at similar levels of GDP per capita. Therefore, it is unlikely that a significant component of growth in the employment rate, which remains unusually low, would come from wage employment, which is already at "normal" levels (see Figure 3.A.2 for comparison of the wage-employment rate across countries). With this in mind, if the alternative is the high unemployment rate that has plagued South Africa for the past 20 years, with the associated negative repercussions, nudging people towards self-employment (as an alternative to unemployment) could potentially be positive for welfare.

### **3.7 Conclusion**

This paper shows that large, positive income shocks, through unconditional cash transfers, do not increase self-employment in South Africa. Through the use of large Census data, I can rule out even small increases in self-employment among cash transfer recipients. This contradicts a common finding in the empirical literature in

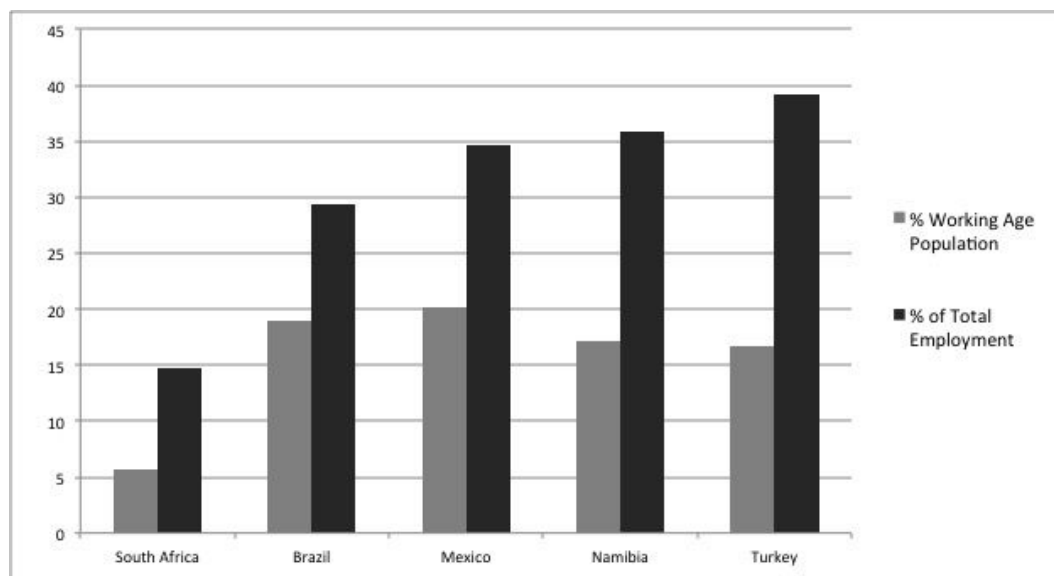
development economics, and the theoretical prediction that cash transfers should promote entry into self-employment by relaxing liquidity constraints and increasing willingness to bear risk. Consistently with this micro-level evidence, self-employment in South Africa has stagnated since the end of Apartheid, with no visible growth in the number of self-employed individuals.

This finding suggests that other barriers are likely to prevent entry into self-employment in South Africa. A look at the country's troubled political and economic past shows that Black and Coloured South Africans were prevented from being self-employed for several generations. Given the importance of inter-generational transmission of self-employment, this may explain the stagnation of self-employment in the post-Apartheid era. This suggests that the self-employment rate would not recover to normal levels without some sort of policy intervention. Exactly which one remains to be tested.



### 3.A Appendix

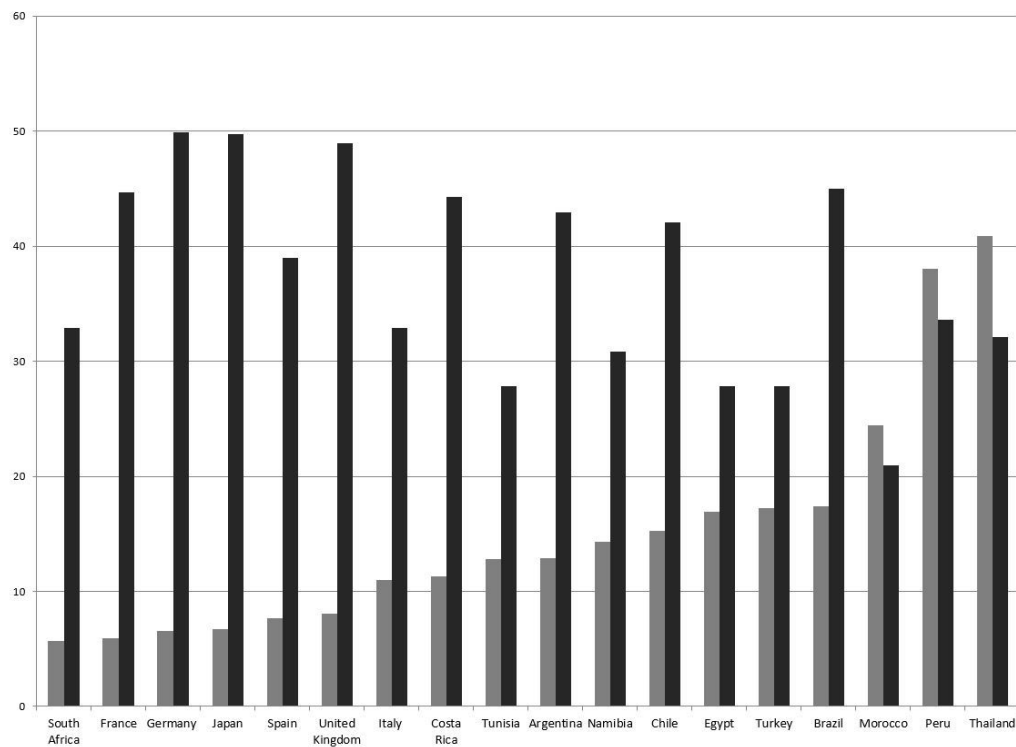
Figure 3.A.1: Self-Employment Rate in Selected Countries with Similar GDP per Capita, 2010



*Note:* This graph gives the share of self-employment in selected countries with similar GDP per capita to South Africa in 2010, over working-age population (grey bar) and total employment (black bar) respectively. The data is from 2010, or the closet available year.

*Source:* World Bank.

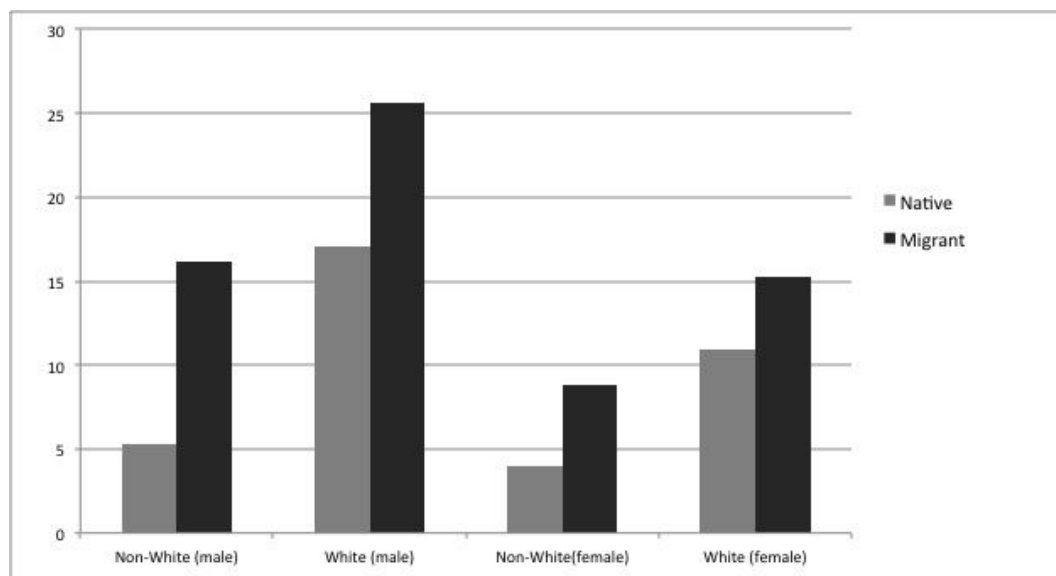
Figure 3.A.2: Self- and Wage- Employment Rates across Several Countries, 2010



*Note:* This graph gives the self- (grey bar) and wage- (black bar) employment rates over adult population (18+) across several countries. The data is from 2010, or the closet available year.

*Source:* World Bank.

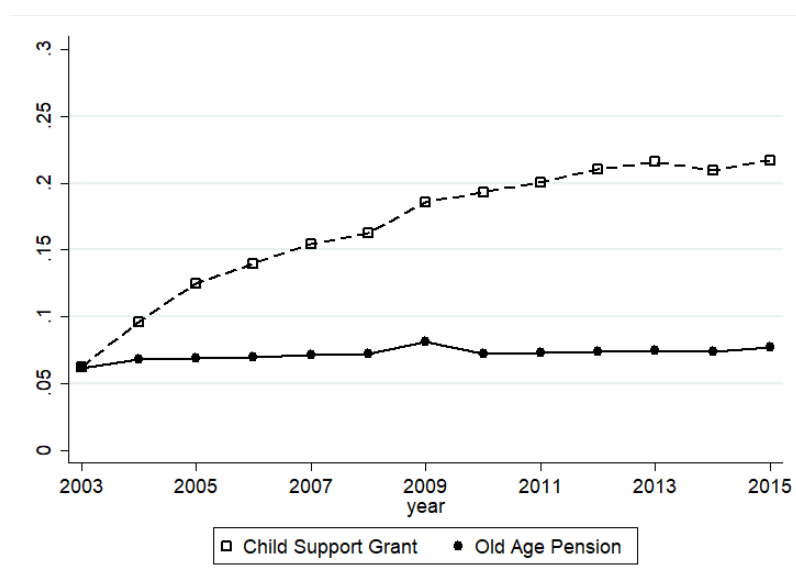
Figure 3.A.3: Self-Employment Rate by Gender and Native Status



Note: This graph gives the rate of self-employment for several sub-groups of the population. The light bar indicates the rate for people born in South Africa, while the dark bar for those born in other countries.

Source: Census (2011).

Figure 3.A.4: Share of Population Receiving CSG or OAP, 2003 to 2015



Note: This graph plots the share of population receiving either the *Child Support Grant*, dotted line, or the *Old Age Pension*, solid line.

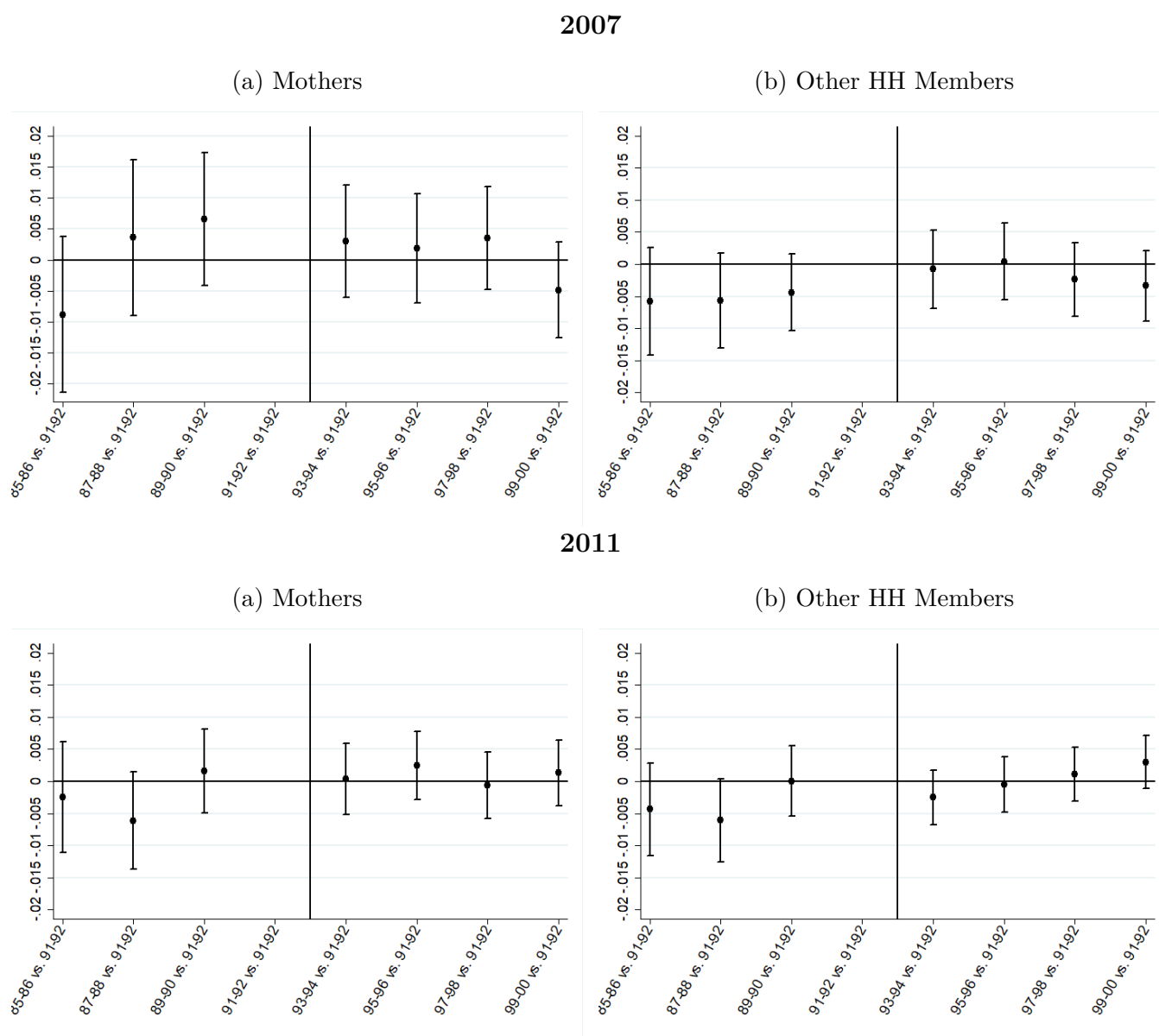
Source: GHS (2003–2015).

Table 3.A.1: Measurement of Self-Employment in South Africa Across Surveys and Years

Measurement	Year																					
	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015
<i>PALMS</i>	5.7	5.9	2.0	2.9	3.5	5.3	10.8	8.9	7.4	6.4	6.4	7.3	8.2	7.2	6.3	5.8	5.6	5.6	5.6	5.3	5.1	5.3
<i>GHS</i>									4.9	4.2	4.5	3.1	3.0	3.7	2.6	5.1	5.0	5.0	5.1	5.3	5.4	5.6
<i>Census/CS</i>								1.8						3.9				4.9				
<i>NIDS</i>															6.2		4.2	5.4	4.8		7.5	5.6
<b>Definitions</b>																						
<i>PALMS</i>	Definition based on Individuals who Report Working for Themselves																					
<i>GHS</i>	Definition based on Individuals who Report Running Any Kind of Business in the Last Week																					
<i>Census/CS</i>	Census 2001: Definition based on Individuals who Identify Directly as Self-Employed CS 2007 & Census 2011: Definition based on Individuals who Report Running Any Kind of Business in the Last Week																					
<i>NIDS</i>	Definition based on Individuals who Report Having Engaged in Self-Employment Activities in the Past 30 Days																					

In the upper panel, this table gives the level of self-employment in South Africa for the Black and Coloured working-age population (15–65), over time and across several surveys: *Post-Apartheid Labor Market Series* (PALMS), *General Household Survey* (GHS), *Census* 2001 and 2011, *Community Survey* (CS) 2007, *National Income Dynamics Survey* (NIDS). In the lower panel, I report the definition on which the measurement of self-employment is based in the different data sources. Issues in the measurement of self-employment in the PALMS for the years 2000 and 2001 are documented in [Kerr and Wittenberg \(2015\)](#) and [Neyens and Wittenberg \(2016\)](#).

Figure 3.A.5: Diff-in-Diff Estimates, Effect of the CSG



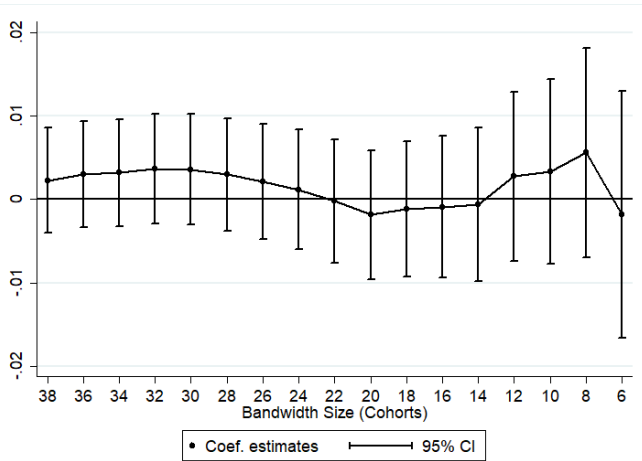
*Note:* These graphs gives the coefficients  $\beta_{CSG,t}$  of Equation 3.2, the difference-in-differences estimation, on the probability of being self-employed, in the years 2007 and 2011. The reference control group are those mothers (household members) whose youngest child (youngest child born in the household) is born in 1991 or 1992. The dots represent the point estimate, while the vertical bars are 95% confidence intervals.

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

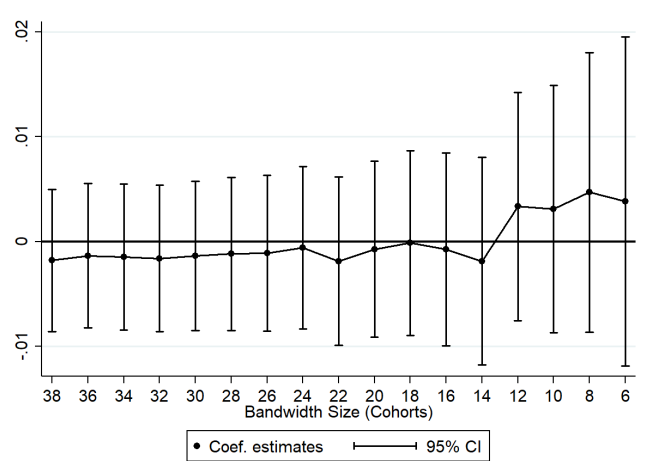
Figure 3.A.6: Bandwidth Sensitivity, Effect of the CSG

2007

(a) Mothers

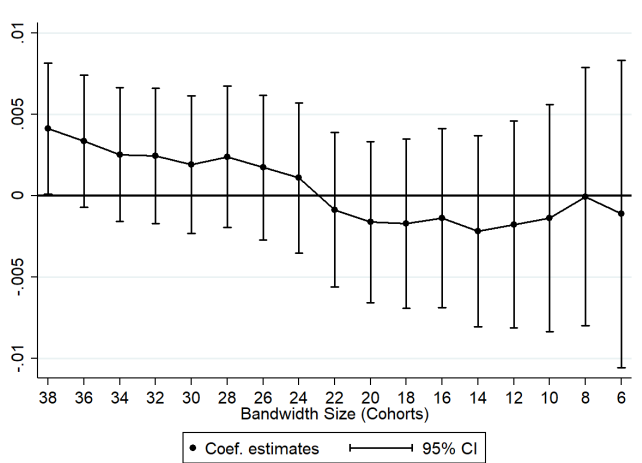


(b) Other HH Members

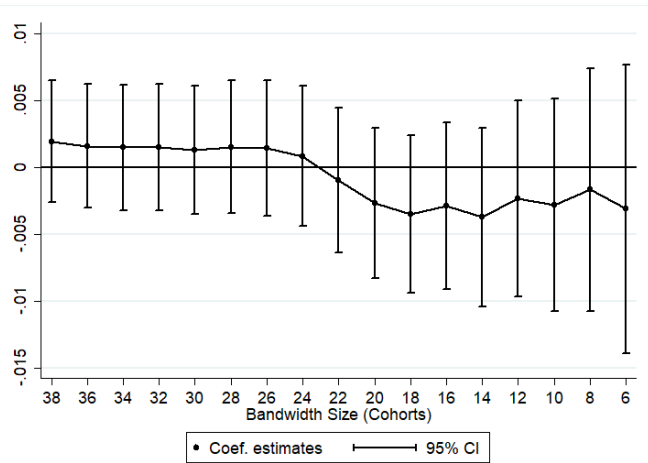


2011

(a) Mothers



(b) Other HH Members



Note: Panel (a) tests the sensitivity of the estimates of Equation 3.1 on the probability to be self-employed to the size of the bandwidth with a linear fit. The boundaries around the coefficients are 95% confidence intervals.

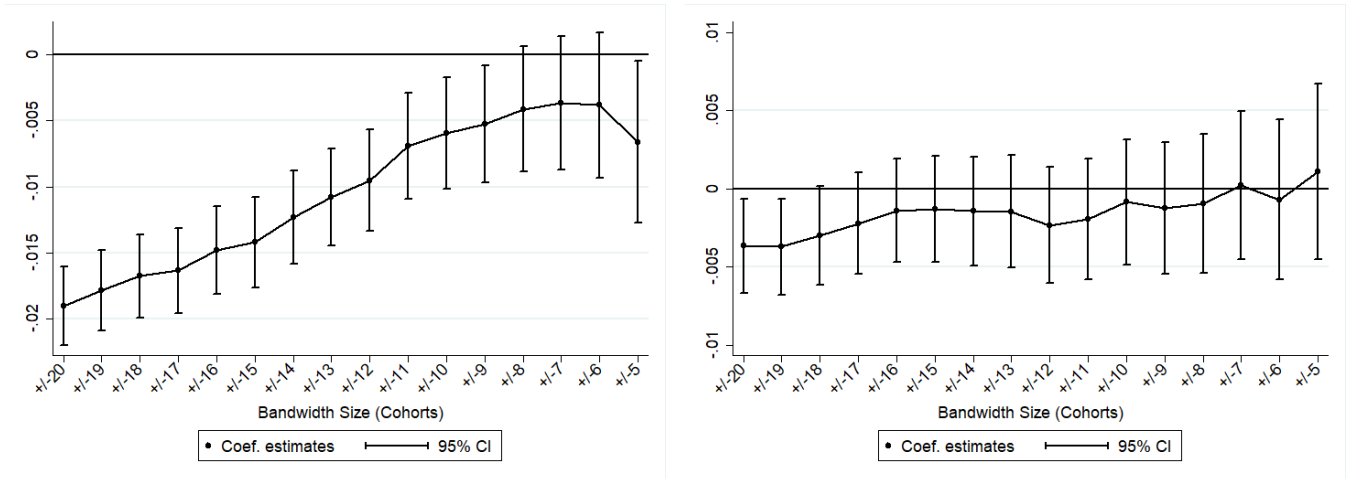
Source: Author's calculations on Community Survey (2007) and Census (2011).

Figure 3.A.7: Bandwidth Sensitivity, Effect of the OAP

2011

(a) Elderly Men

(b) Other HH Members



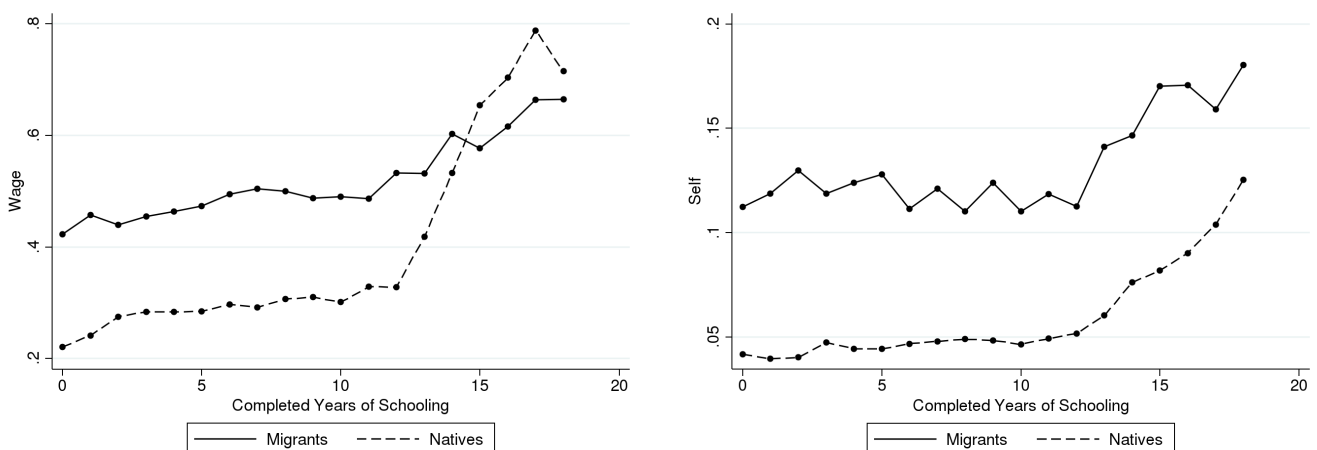
Note: Panel (a) tests the sensitivity of the estimates of Equation 3.3 on the probability to be self-employed to the size of the bandwidth with a linear fit. The boundaries around the coefficients are 95% confidence intervals.

Source: Author's calculations on Census (2011).

Figure 3.A.8: Wage- and Self- Employment by Education, Natives vs Migrants

(a) Wage

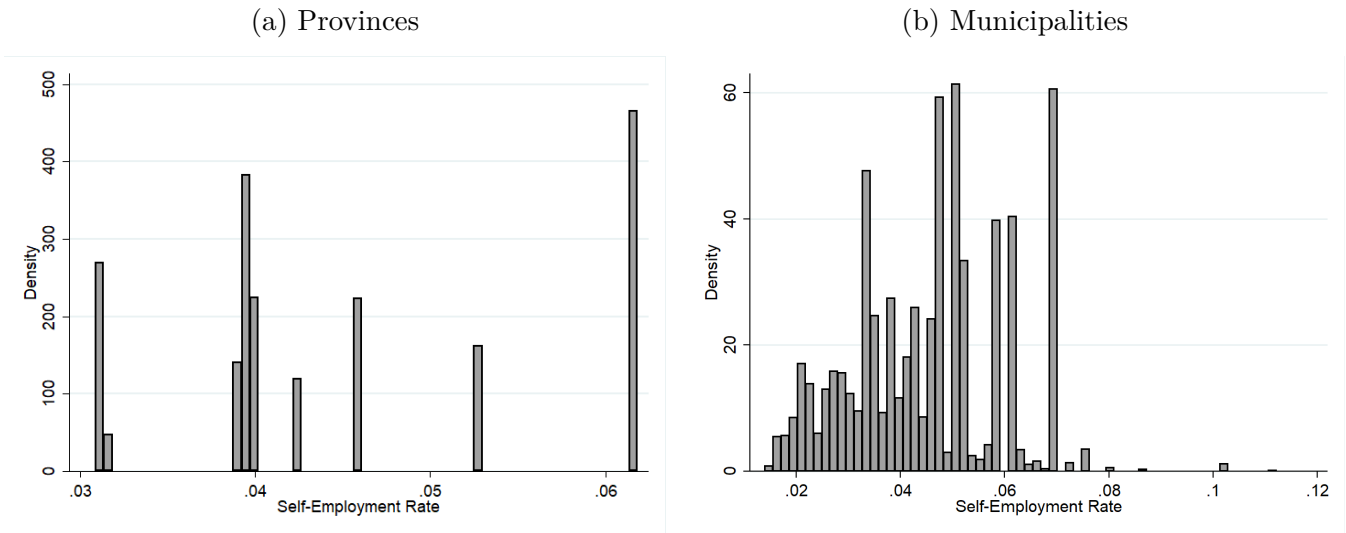
(b) Self



Note: These graphs give the probability of being wage-employed (left panel), and self-employed (right panel) by number of years of completed schooling for Black and Coloured South Africans with the dashed line, and migrants from other African countries with the solid line.

Source: Census (2011).

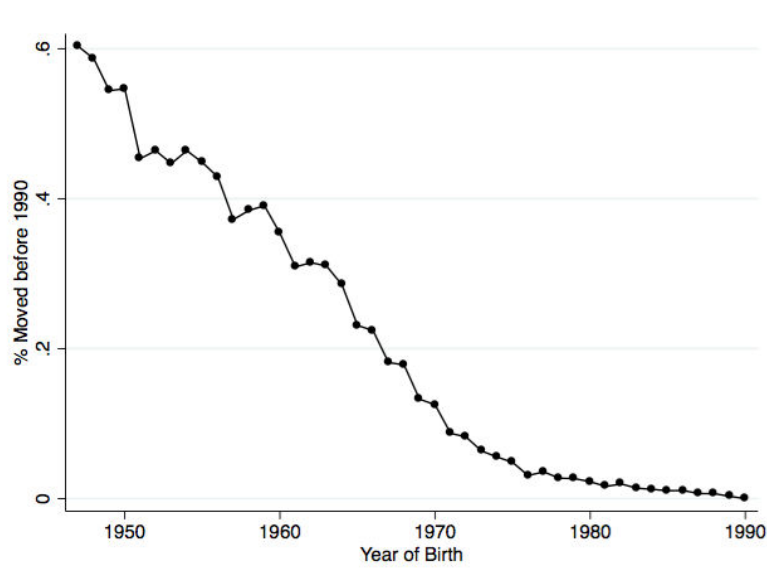
Figure 3.A.9: Self-Employment Rate Across Provinces and Municipalities



*Note:* These graphs give the distribution of the self-employment rates across provinces (left panel), and municipalities (right panel). The sample only includes Black and Coloured individuals born in South Africa.

*Source:* Census (2011).

Figure 3.A.10: Share of Migrants who Moved before 1990 by Year of Birth

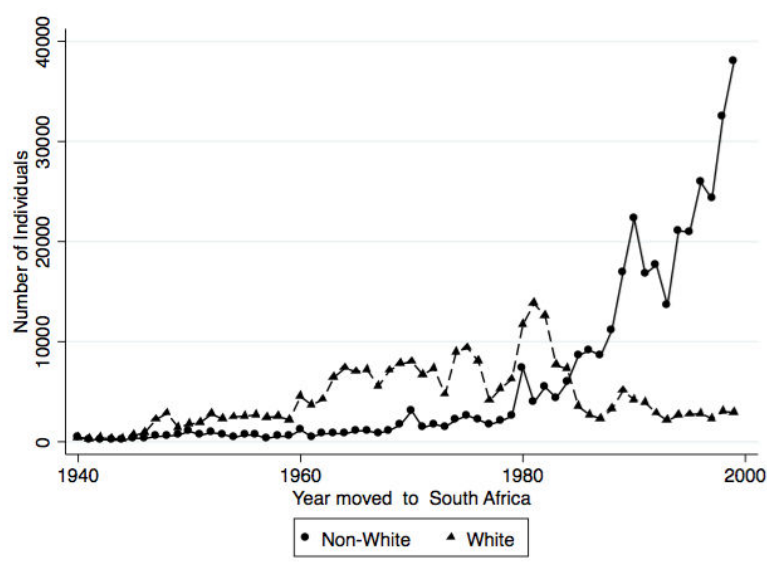


*Note:* This graph plots the share of people, among those who are not born in South Africa, who have moved to South Africa before 1990 by year of birth.

*Source:* Census (2011).



Figure 3.A.11: Number of People by Year of Migration to South Africa



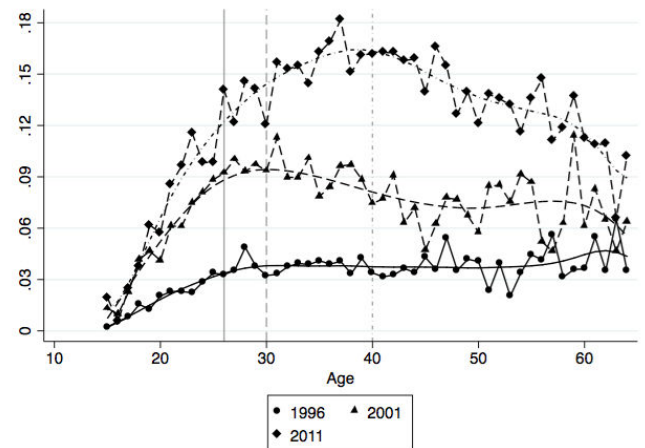
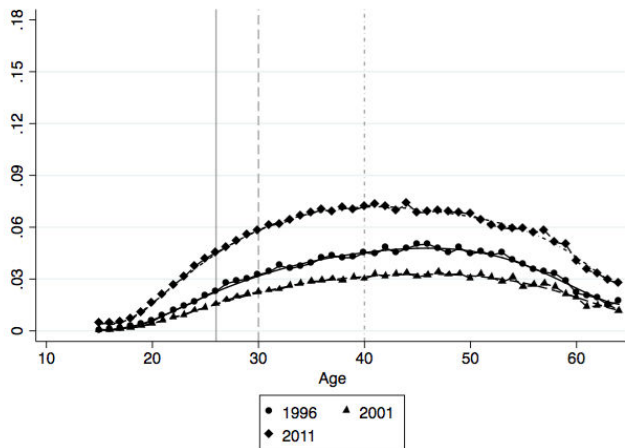
*Note:* This graph gives the number of people in the 2011 Census who report having moved to South Africa by year of migration.

*Source:* Author's calculations on Census 2011.

Figure 3.A.12: Self-Employment by Age, 1996, 2001 & 2011

(a) Natives

(b) Migrants



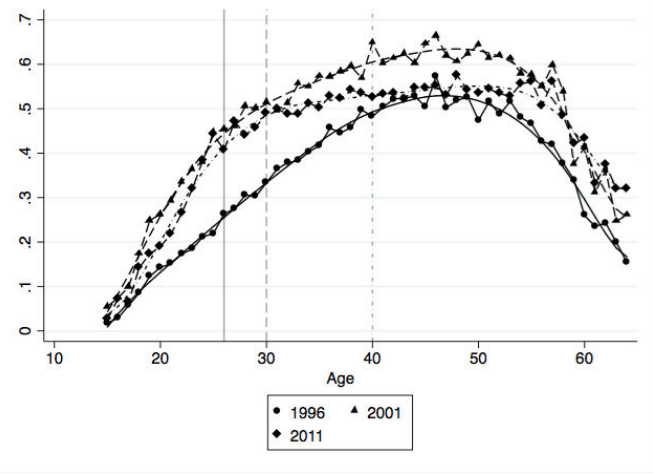
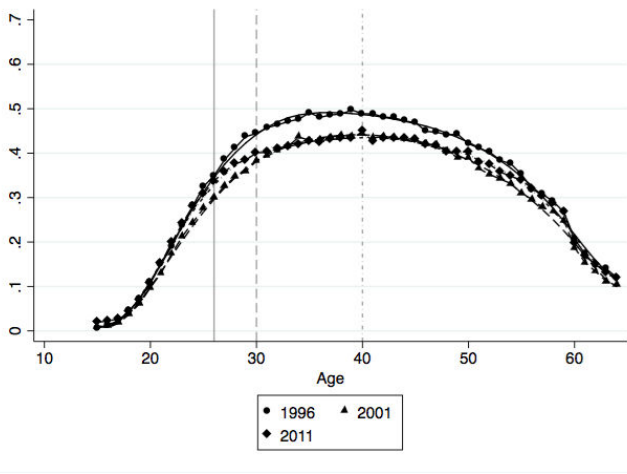
*Note:* This graph plots the self-employment rate by age in 1996, 2001 and 2011. The left-panel includes only Black and Coloured individuals born in South Africa. The right panel includes only migrants from other African countries. The vertical lines indicate people born before 1970 in each year.

*Source:* Census (1996, 2001 & 2011).

Figure 3.A.13: Wage-Employment by Age, 1996, 2001 & 2011

(a) Natives

(b) Migrants



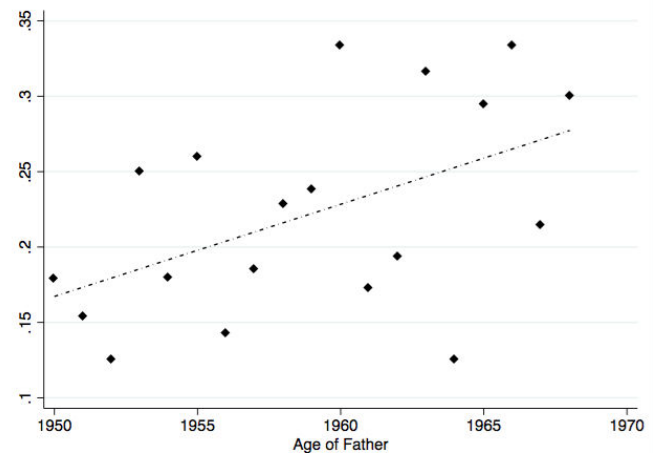
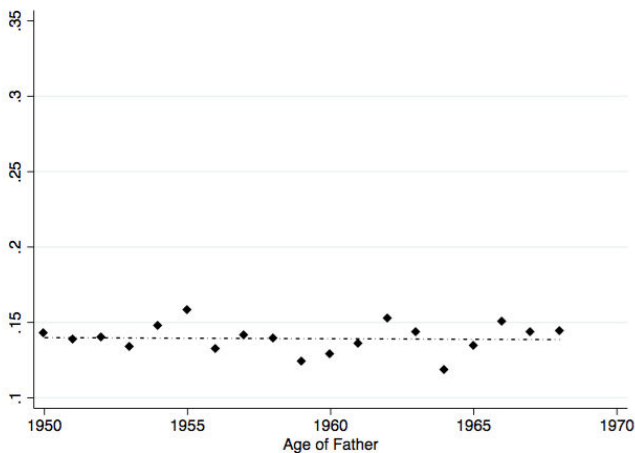
*Note:* This graph plots the wage-employment rate by age in 1996, 2001 and 2011. The left-panel includes only Black and Coloured individuals born in South Africa. The right panel includes only migrants from other African countries. The vertical lines indicate people born before 1970 in each year.

*Source:* Census (1996, 2001 & 2011).

Figure 3.A.14: Share of Self-Employment by Father's Year of Birth

(a) Natives

(b) Migrants



*Note:* These graphs plot the share of self-employment (conditional on being employed) by father's year of birth for both natives (panel (a)), and migrants from other African countries (panel (b)). The fathers born earlier were more likely to have migrated to South Africa before 1990s (Figure 3.A.10), and more likely to have experienced the Apartheid restrictions.

*Source:* Census (2011).



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# Summary

## Cash Transfers, Employment and Informality in South Africa

This dissertation studies the employment effects of cash transfers in a segmented labor market. The first and main chapter shows that an unconditional cash transfer program targeted at mothers has lasting positive impacts on job quality. Five years after having received the cash transfer, treated mothers are more likely to be employed in the formal sector. This appears to be the result of changes in the way recipients search for a job, as treated mothers are unemployed for longer and target better jobs. The second chapter shows the employment effects of a reform in the means-tested, non-contributory pension system of South Africa, which lowered the age of retirement from 65 to 60 for men. The reform caused a large extensive-margin response, as informal workers stop working when they become eligible to the pension. Instead, formal workers do not quit their jobs nor switch to the informal sector to become eligible to the pension. Lastly, this dissertation discusses the lack of self-employment in South Africa. Building on the results of the first two chapters, the last chapter shows that South Africans do not increase entry to self-employment as a result of cash transfers. This indicates that liquidity constraints are not the main reason for the lack of self-employment in South Africa, which is likely to have historical roots stemming from Apartheid. The chapter discusses evidence and potential policy implications of this explanation, alongside possible avenues for future research on this phenomenon.

**Keywords:** Cash Transfers, Informal Sector, Job Quality, Job Search, Self-Employment, South Africa



# Résumé

## **Transferts Monétaires et Emploi dans le Secteur Informel en Afrique du Sud.**

Cette thèse porte sur les effets de transferts monétaires sur l'emploi dans le marché du travail sud-africain, un marché fortement segmenté entre secteur formel et informel. Le premier et principal chapitre montre qu'un programme de transferts monétaires inconditionnels destinés aux mères a eu des effets positifs durables sur la qualité de leurs emplois. Sur le long terme, les mères bénéficiaires du transfert sont plus susceptibles d'être employées dans le secteur formel. C'est la conséquence de changements dans la façon dont les mères traitées cherchent un emploi. En leur donnant la possibilité de rester au chômage pendant plus longtemps, le programme de transferts inconditionnels leur permet de viser des emplois de meilleure qualité. Le deuxième chapitre étudie les effets sur l'emploi d'une réforme du système public de retraites en Afrique du Sud, qui est non-contributif et soumis à conditions de ressources. Cette réforme a abaissé l'âge de la retraite de 65 à 60 ans pour les hommes. Elle a entraîné une forte diminution du taux d'activité des travailleurs informels, qui cessent de travailler lorsqu'ils atteignent 60 ans et deviennent éligibles à la pension de retraite non-contributive. Au contraire, les travailleurs du secteur formel ne quittent pas leur emploi et ne se tournent pas vers le secteur informel pour avoir droit à la pension de retraite. Enfin, cette thèse aborde la question du faible nombre de travailleurs indépendants en Afrique du Sud. Le dernier chapitre montre que les Sud-Africains ne travaillent pas plus à leur compte en réponse à des transferts monétaires. Cela indique que les contraintes de liquidité ne sont pas la principale raison du manque de travailleurs indépendants en Afrique du Sud. Cette faible présence de travailleurs indépendants a probablement des racines historiques liées à l'apartheid. Ce troisième chapitre examine les implications potentielles de cette explication, ainsi que les pistes de recherches futures possibles pour une compréhension plus fine de ce phénomène.

**Mots-clés:** Transferts monétaires, Secteur informel, Qualité de l'emploi, Recherche d'emploi, Travail indépendant, Afrique du Sud

