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

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Abstract

We investigate how means-tested public pensions interact with the informal sector, by exploiting a reform in the non-contributory *Old Age Pension* system in South Africa, where the eligibility age was lowered from 65 to 60 for men. 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 from informal workers, who drop out of the labor force, while formal employment is mostly unaffected. This heterogeneity is not due to 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 simultaneously work informal jobs, and even if the means-test is located where formal and informal wages largely overlap. In total, we estimate that the pension reform has driven about 25,000 elderly individuals away from informal jobs.

JEL Codes: H55, J26, J46, O17
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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 investigates this concern by exploiting a reform of the public, non-contributory, and means-tested pension system of South Africa, which significantly lowered the eligibility age to access the pension, for men only. By making use of the reform in the age threshold, 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. In this context, the potential labor market effects of means-tested social programs are unclear because of the presence of a large informal sector. Direct recipients, i.e. elderly male, experience both an income and a substitution effect: the money from the social pension is available only to workers earning less than a certain amount in the formal sector. If this change in incentives is large enough, which seems *a priori* plausible given the high amount of the public pension, this could induce switching towards informal jobs. Instead, we show that the main effect of the reform is that 30 to 40% of informal workers stop working when eligible to the pension, regardless of the level of hourly wages. Formal workers drop out of the labor force only at very low level of hourly wages, and we find no evidence of a re-allocation to the informal sector. We draw from these results three main implications for both economic research and policy design. First, these findings support, for this subgroup, the view of informality as mainly the symptom of segmentation in the labor market: informal workers who experience this income effect stop working, and there is not a significant number of formal workers for whom the change in incentives is large enough to induce switching towards the informal sector. Indirectly, this goes against the view that workers allocate across sectors according to their comparative advantage, which would imply "marginal" workers between the two sectors could easily switch. Second, this finding also has important implications for policy design, as it alleviates concerns about the potential distortions introduced by means-tested programs, despite the means-test of this specific program being located in an area where the wage distributions of the two sectors largely overlap. Lastly, 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 (but not at the intensive margin), suggesting that this type of program might relieve subsistence-level constraints.

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.

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 (de Carvalho Filho (2008), Kaushal (2014), Juárez and Pfutze (2015)), but these programs are usually less universal and provide lower amounts than the South African *Old Age Pension*.[1] For this reason, there is a large literature on this specific social program, 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)).[2] Instead, the second branch deals with the labor market effects of the OAP, which 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.[3] 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 (Edmonds et al. (2005), Hamoudi and Thomas (2014)).[4]

[1]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.

[2]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.

[3]To our knowledge, the only other paper to use these reforms for an analysis of the effects of public pension is Matsuda (2016).

[4]Edmonds (2006) shows that pension receipt leads to a lowering of child labor and a symmetric increase in educational attainment. Cross-sectional evidence from Bertrand et al. (2003) revealed the presence of disincentive effects for other adult 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 et al. (2006)). To solve this selection issue, Ardington et al. (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 other private schemes that might have the same age threshold, and that create discontinuities in the age profile regardless of the social pension.[5] 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 describes the data. Section 4 provides a simple conceptual framework to better frame the results of the paper. Section 5 presents the empirical strategy, showing the bias from not accounting for other pension schemes, and the results. It also discusses how workers at the same level of wages respond differently across formal and informal jobs. Section 6 concludes.

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

[5]This concern was already acknowledged by Edmonds et al. (2005): "..we are concerned that our identification assumptions are more suspect for men. First, we are more comfortable with our assumption that the underlying age trends in our measures of household composition are smooth absent the pension for women. Men are more likely to face retirement incentives (absent the pension) at the age of pension-eligibility in the formal sector of the South African economy."

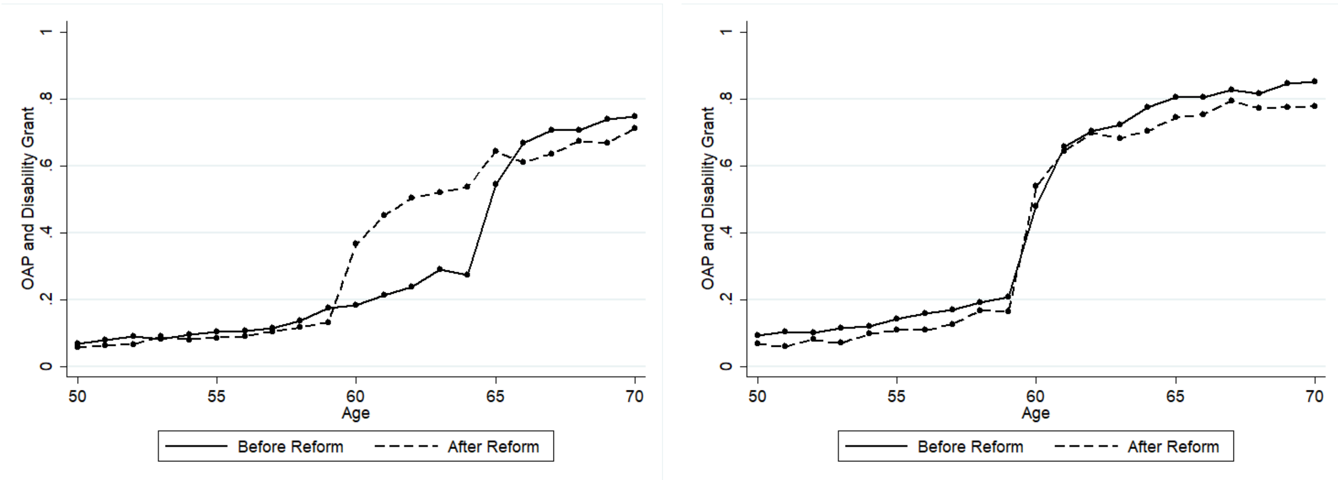
to Whites (Duflo (2003)). Several other administrative loopholes were exploited to keep groups other than White to fully access the grant (for a full account of the history of the OAP in Apartheid South Africa refer to Lund (1993) and Woolard and Leibbrandt (2010)). The means-tests were equalised in 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 (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 A1. 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.[6] 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.

Figure 1: Share of People Receiving the *Old Age Pension* or *Disability Grant* by Age, **Before and After** Pension Reform

(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 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.[7] 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,

[6]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`

[7]Information about the *Disability Grant* can be found here: `http://www.gov.za/services/social-benefits/disability-grant`

in that the DG provides some form of income support to people who cannot work before they reach pension age. Figure A1 gives the number of beneficiaries of the OAP and *Disability Grant*, and their sum, over time.[8]

In Figure 1, we plot the share receiving the *Disability Grant* and the *Old Age 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 A2.) 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.

3 Data and Descriptive Statistics

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

[8]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.

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 *Quaterly 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.[9] 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.[10]

[9]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`

[10]Moreover, it has been reported that the initial waves of the LFS significantly over-measured informal employment. See Kerr and Wittenberg (2015) for discussion.

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.[11] 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 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.

3.3 Descriptive Statistics

Men aged 60–64 make-up between 1.5%-2% of the working-age population (15–65)[12], 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 A3, 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

[11]Questions about whether the employer pays social contributions may be more difficult to recollect, or might not be known to the worker.

[12]This share grows slightly during the period of our study (2002–2015), as life expectancy, which which was well below 60 over the period in South Africa, increased.

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 A2 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 time in real terms (Table A1). 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 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 A4, 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 A5. We focus on the period after the reform as this is the relevant one to our analysis, and for which we have better information. As we can see from Figure A5, 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.

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 7.1.

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 μ_i, or (iii) differences in α_i.

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 A6 and A7).

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 α and μ 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.

5 Empirical Analysis

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 et al. (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.[13] 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, we begin by estimating the following equations, separately for men and women, before and after the reform:

$$Y_{i,t} = \delta_t + f(age_i) + f(age_i) \times Age_{(60+)} + \beta_{RD} Age_{(60+)} + \varepsilon_{i,t} \tag{1}$$

where $Y_{i,t}$ is the outcome of interest. δ_t indicates year effects. $f(Age_i)$ 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. $Age_{(60+)}$ is a dummy variable indicating whether the individual is older than 60, which is the cut-off point after the reform. β_{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 1 in the following form:

$$\begin{aligned} Y_{i,t} = \delta_t + f(age_i) + f(age_i) \times Age_{(60+)} + f(age_i) \times Post_t + f(age_i) \times Age_{(60+)} \times Post_t \\ + \beta_1 Age_{(60+)} + \beta_{DiDRD} Post_t \times Age_{(60+)} + \varepsilon_{i,t} \end{aligned} \tag{2}$$

[13]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.

where the difference is that we interact the function of age with the $Post_t$ and $Age_{(60+)}$ variables. $Post_t$ 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: $Age_{(60+)}$, and $Post_t \times Age_{(60+)}$. β_{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, 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 β_{RD} and β_{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.

5.2 Results

The results of the RD estimation are presented in Table 1, and those of the "diff-in-disc" in Table 2. Graphical evidence of the drop in total employment and by sector is presented in Figure 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.

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

Table 1: Old Age Pension and Employment, RDD Results, Quadratic Fit 50–70, PALMS

	Before Reform			After Reform		
	(1)	(2)	(3)	(4)	(5)	(6)
	Employed	Informal	Formal	Employed	Informal	Formal
a. *Men*						
$Age_{(60+)}$	-0.0278	0.0083	-0.0361**	-0.0907***	-0.0427***	-0.0480***
	(0.0195)	(0.0151)	(0.0170)	(0.0115)	(0.0084)	(0.0108)
Mean Y 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. *Women*						
$Age_{(60+)}$	-0.0720***	-0.0427***	-0.0293***	-0.0536***	-0.0360***	-0.0176**
	(0.0136)	(0.0114)	(0.0096)	(0.0090)	(0.0067)	(0.0074)
Mean Y 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 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, β_{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: Old Age Pension and Employment, Quadratic Fit 50–70, Diff-in-Disc, PALMS

	Employed	**Informal**	**Formal**	
			Extensive	Intensive
	(1)	(2)	(3)	(4)
a. *Men*				
$Post \times Age_{(60+)}$	-0.0639**	-0.0511**	-0.0128	-0.4621
	(0.0260)	(0.0195)	(0.0261)	(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. *Women*				
$Post \times Age_{(60+)}$	0.0183	0.0067	0.0116	0.4686
	(0.0131)	(0.0134)	(0.0137)	(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 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, β_{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).

total employment effect of the pension by a factor between 1.5 and 2.[14] Intuitively, the coefficients suggest that the size of the bias is roughly the same for women, but this cannot be tested directly as the age threshold for women is left unchanged.[15]

When we correct for this bias, by employing the estimator of Equation 2, the point estimate on total employment decreases significantly (Table 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 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 formally test the statistical significance of the difference between the coefficients across informal and formal employment, in a "seemingly-unrelated regression" (SUR) framework; we find that these coefficients are statistically different at the 10% level in a less demanding specification (as in Zellner (1962)) but only marginally significant in the more demanding specification, with a p-value of .18 (using the strategy detailed in Wooldridge (2010)[16]). We show in the next section that this can be explained by the fact 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, Column 4).[17] This partly contradicts the prediction of our conceptual framework

[14]The RD overestimates the employment effect by a factor of 2 when using a linear function on the 55-64 window, see Table A3.

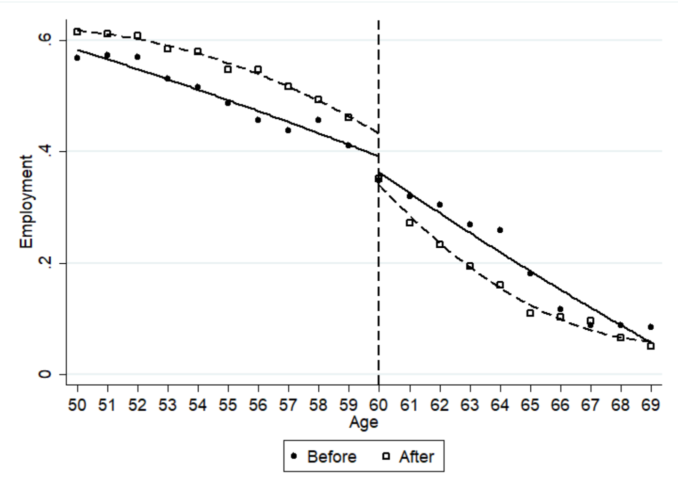
[15]The drop in total employment is roughly of the same magnitude as for men, and split in equal parts between formal and informal. If, as for men, there is little or no effect of the pension on formal employment, we can also speculate that the RD estimation over-estimate the effect on employed by a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

[16]Chapter 7, pp. 151-153

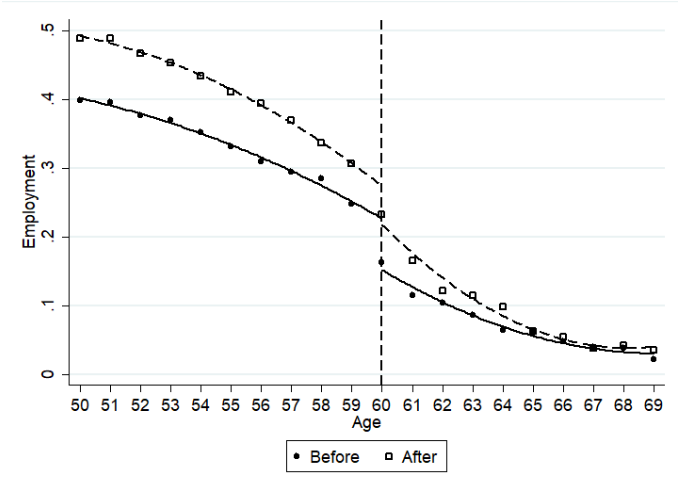
[17]Estimating an effect on the intensive margin of informal employment is more complicated because of the large extensive margin response.

Figure 2: Total, Formal and Informal Employment, Before and After, Men & Women

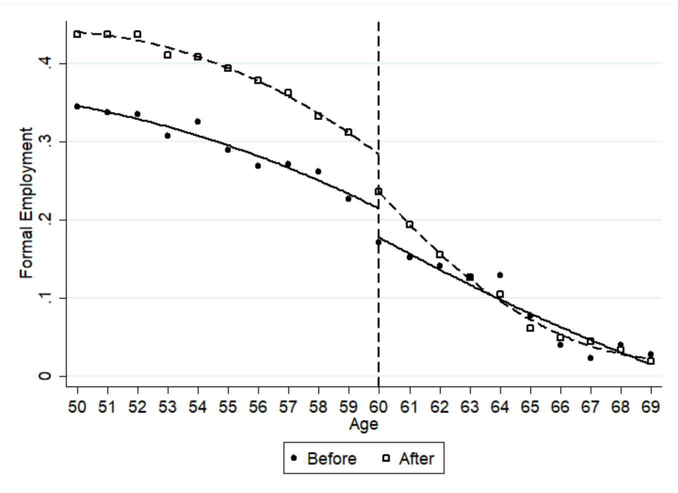
(a) Men: Employment



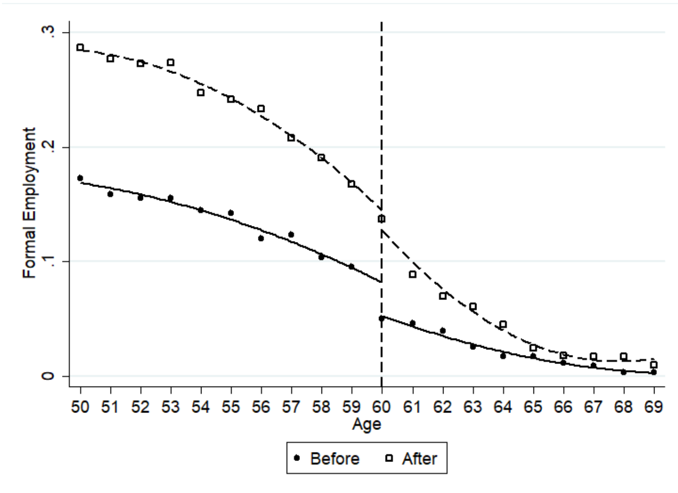
(b) Women: Employment



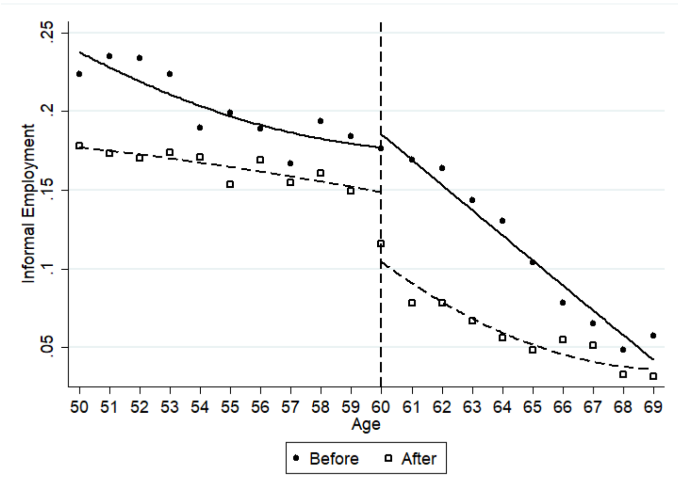
(a) Men: Formal Employment



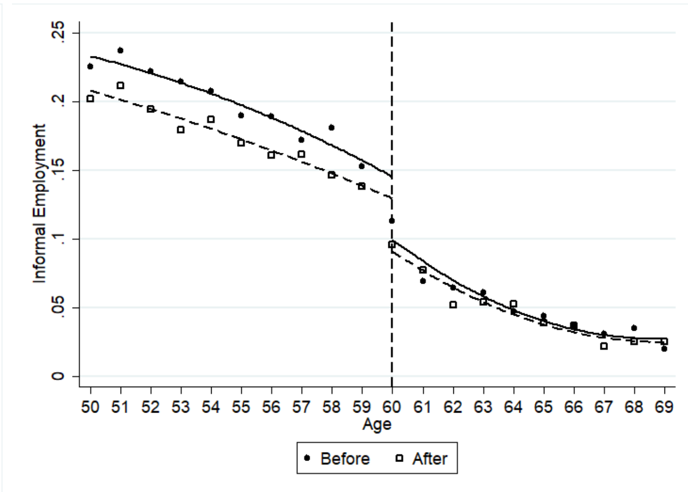
(b) Women: Formal Employment



(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).

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 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.

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.[18] 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 7.4.[19] As there was no discontinuity pre-reform in 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 A6 and A7). 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 3 for men, and in Figure 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

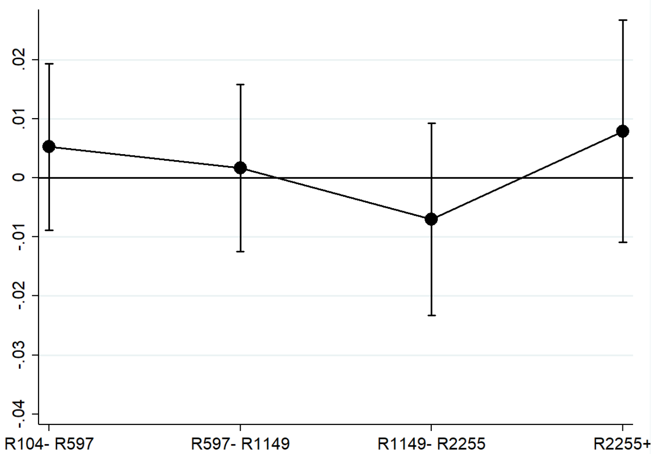
[18] Because of growth in real wages over time, the identification assumption that the magnitude of the discontinuity would have stayed the same over time *at each level of wages* is unlikely to hold. For more details see Appendix 7.4.

[19] 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.

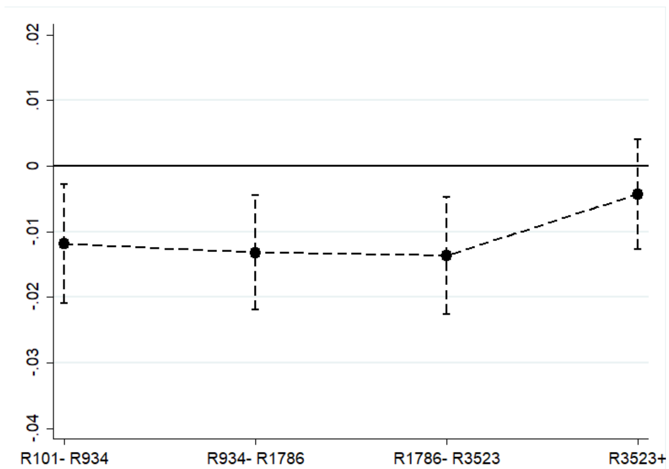
whose full-time equivalent monthly salary, i.e. what they would make in a month given their hourly wage and 43 hours working week, is larger than the amount of the pension (R 1000). In other words, some informal workers are willing to give up more earnings than what they get with the pension. One interpretation 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 3: Effect on Probability to be **Informally** Employed by Quartile of **Informal** Hourly Wage, Quadratic Fit 50–70, RDD, Before and After, Men

(a) Men - Before



(b) Men - After

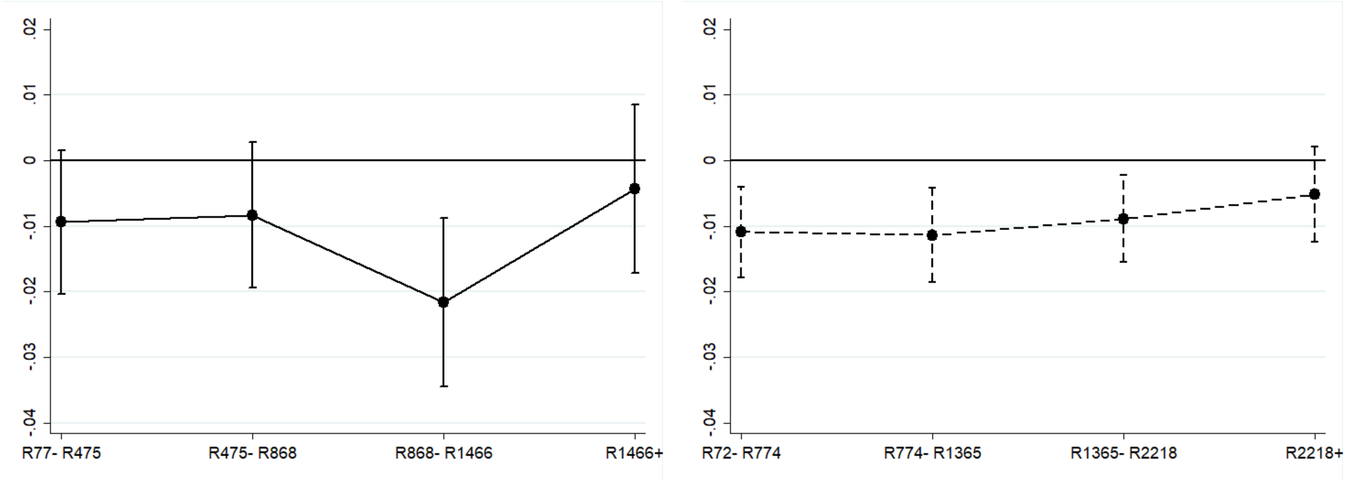


Note: These graphs plot the coefficients of Equation 1 on the probability to be informally employed within each quartile of the informal hourly wage distribution before and after the OAP reform. Quartiles are defined according the distribution of informal hourly wages for the 55–59 years old population 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).

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 in-

Figure 4: Effect on Probability to be **Informally** Employed by Quartile of **Informal** Hourly Wage, RDD, Before and After, Women

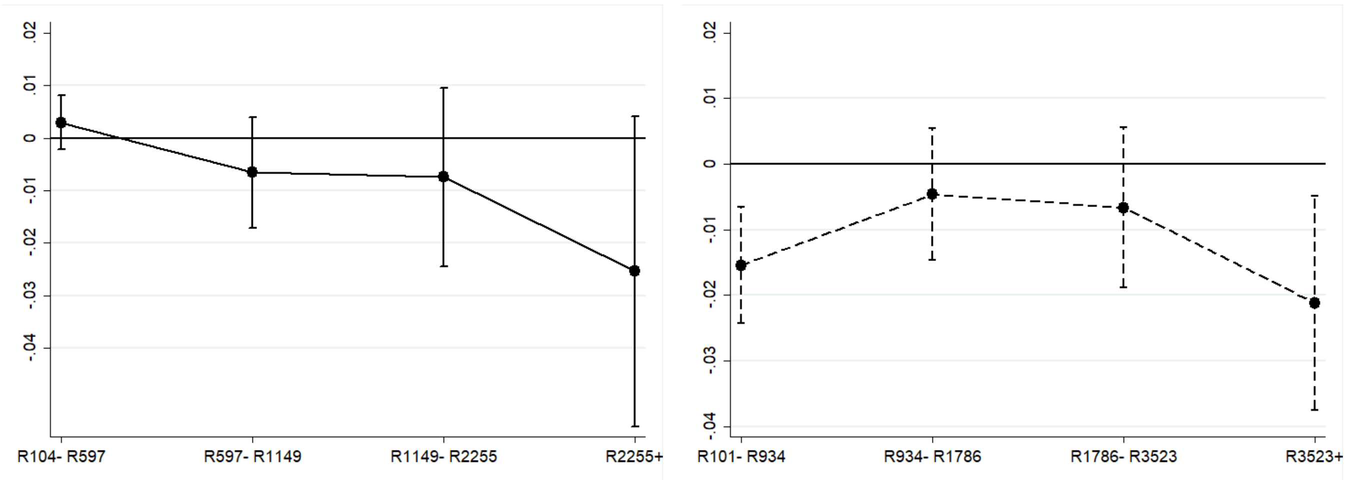
(a) Women - Before (b) Women - After



Note: These graphs plot the coefficients of Equation 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 5: Effect on Probability to be **Formally** Employed by Quartile of **Informal** Hourly Wage, RDD, Before and After, Men

(a) Men - Before (b) Men - After



Note: These graphs plot the coefficients of Equation 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).

formal distribution roughly begins at the median of the formal wage distribution. The effects on formal employment by wage for men are plotted in Figure 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.[20] Instead, workers formally employed in the 2nd and 3rd quartile of informal wages do not respond to the reform. The 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.

[20]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.

5.3 Other Heterogeneity Dimensions

We also study heterogeneity in the employment response across other dimensions. First, we check whether it is (informal) employees or self-employed who respond more to the reform. The capacity to adapt one's hours may very well depend on his occupational status. At age 55 to 64, over the period we consider, informal wage earners and self-employed are roughly equally split. However, in Table A7, we show that the point estimate is larger for wage-employees, but not statistically different from the self-employed, on whom the effect is more imprecisely estimated.[21]

We also identify important heterogeneity across the location of residence.[22]. In Table 3, we show that the overall effect is mostly driven by non-metropolitan areas. This difference between metropolitan and non-metropolitan areas can be attributed to different factors. One interpretation is that consumer prices are often much higher in denser and urbanized areas, this would make the relative importance of the same nominal transfer larger to rural individuals than to urban ones, all other things equal. Otherwise, this may be an indication that informal jobs outside metropolitan areas are of a different type, and possibly less desirable. We also identify marital status as another important dimension of heterogeneity. As shown again in Table 3, the effect is stronger among married men. This could indicate that the level of the OAP's transfer is not large enough for the eligibility to the OAP to allow a single recipient to give up informal work. Living in a couple typically lowers per-capita costs of living, and especially per-capita costs of subsistence, which could explain why the transfer may be sufficient to push married individuals out of informal work. It could also be that lower-paying informal employment is accepted or undertaken by married individuals to start with. Without additional exogenous variation to exploit, it is difficult to make a causal claim with regards to these dimensions of heterogeneity.

[21] Informal wage employment represent 61% of the total effect of the reform, while informal self-employment represents the 39% remaining.

[22] We distinguish individuals according to whether they live in one of South Africa's 6 metropolitan areas or not. These areas are (their seats in parentheses): Cape Town, Johannesburg, Tshwane (Pretoria), Ekurhuleni (Germiston), eThekwini (Durban), Nelson Mandela Bay (Port Elizabeth). These 6 metropolitan areas are home to 24% of the total population of South Africa.

Table 3: Heterogeneous Effects by Location and Marital Status, Quadratic Fit 50–70, Diff-in-Disc, PALMS

	Employed	**Informal**	**Formal**	
			Extensive	Intensive
	(1)	(2)	(3)	(4)
(i) Non-metropolitan areas				
$Post \times Age_{(60+)}$	-0.110***	-0.0761***	-0.0338	-1.343
	(0.0370)	(0.0213)	(0.0333)	(1.641)
Mean Y at Age 59	0.42	0.16	0.26	11.55
Observations	93232	93232	93232	93232
R-squared	0.1463	0.0288	0.0955	0.0854
(ii) Metropolitan areas				
$Post \times Age_{(60+)}$	-0.0393	-0.0328	-0.00648	-0.335
	(0.0677)	(0.0426)	(0.0559)	(2.655)
Mean Y at Age 59	0.52	0.13	0.38	17.03
Observations	28713	28713	28713	28713
R-squared	0.1293	0.0250	0.0879	0.0795
(i) Non-married				
$Post \times Age_{(60+)}$	-0.0185	-0.0442	0.0257	1.544
	(0.0590)	(0.0403)	(0.0385)	(1.738)
Mean Y at Age 59	0.31	0.15	0.16	6.93
Observations	39504	39504	39504	39504
R-squared	0.0827	0.0326	0.0467	0.0418
(ii) Married				
$Post \times Age_{(60+)}$	-0.0766**	-0.0553***	-0.0214	-0.980
	(0.0313)	(0.0199)	(0.0325)	(1.689)
Mean Y at Age 59	0.50	0.16	0.33	14.97
Observations	100560	100560	100560	100560
R-squared	0.1626	0.0255	0.1191	0.1044

Note: *** p<0.01, ** p<0.05, * p<0.1. This table gives the results of Equation 2 with a **quadratic function**, for men on the age window 50–70. The sample is limited to Black and Coloured men. The lower number of observation in the upper panel is due to missing information about location of residence. We only report the coefficient of interest, β_{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).

5.4 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 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 A5).

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 and A4). In Figure A9, we test the sensitivity of the estimates of Equation 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 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 A6, 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.

5.5 Market-Level Effects

How many jobs are freed up by this reform in the public pension system? This is a key policy question especially if the jobs left behind by the elderly can make room in the labor market for younger individuals. We discuss this at length in Appendix 7.3. The employment response of the elderly as a result of the OAP reform is significantly higher in some industries and occupation. We can, in theory, use the differential impact of the program across industries to capture potentially positive displacement effects on younger individuals. In practice, this analysis is limited by the relatively small size of the elderly labor force in the overall labor market. We calculate that, at most, the pension reform has freed up between 20 000 to 30 000 jobs, which is a "drop in the bucket" when compared to the size of the South African labor force. Therefore, despite a relatively large employment response of the elderly, the size of this population group in the demographic structure of South Africa make its impact on the overall number of jobs negligible. In our empirical analysis, we only have sufficient statistical power to reject one-to-one substitution with the closest substitutes of the affected workers (55–59 y.-o. males). Nonetheless, this allows us to conclude that this kind of reform, in countries with a similar demographic structure, is unlikely to make a significant difference in terms of the pool of jobs available to the young.

6 Conclusion

This paper uses a decrease in the age eligibility threshold for men in the old-age pension scheme of South Africa to study how public pensions interact with the informal sector in developing countries. 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.

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7 Appendix

7.1 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

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

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,

$$u_i(C^*(I), L^*(I)) < u_i(C^*(F), L^*(F)) \quad \text{(formal-sector workers)} \tag{3}$$

or

$$u_i(C^*(I), L^*(I)) > u_i(C^*(F), L^*(F)) \quad \text{(informal-sector workers)} \tag{4}$$

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:

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

Conditions (3) and (4) thus translate into:

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

Selection into sectors is thus determined by:

$$\mu_i < \alpha_i \ln\left(\frac{w_{i,F}}{w_{i,I}}\right) \quad \text{(formal-sector workers)} \tag{5}$$

or

$$\mu_i > \alpha_i \ln\left(\frac{w_{i,F}}{w_{i,I}}\right) \quad \text{(informal-sector workers)} \tag{6}$$

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 μ_i, or (iii) differences in α_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, $\alpha_i \ln(\frac{w_F}{w_I}) > 0$). 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 (6)

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

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

$$\mu_A > \alpha_A \ln\left(\frac{w_{A,F}}{w_{A,I}}\right) - \ln\left(\frac{w_{A,F}}{w_{A,I}} \times \frac{w_{A,I}T + m}{w_{A,F}T + m}\right) \tag{7}$$

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

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

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

$$\alpha_i \ln(\alpha_i) + (1 - \alpha_i)\ln(1 - \alpha_i) + \alpha_i \ln(w_{i,I}) + \ln(T + \frac{m}{w_{i,I}})$$
$$> \alpha_i \ln(\alpha_i) + (1 - \alpha_i)\ln(1 - \alpha_i) + \alpha_i \ln(w_{i,F}) + \ln(T + \frac{m}{w_{i,F}}) - \mu_i$$

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, (7). However, trivially, $\alpha_A \ln(\frac{w_{A,F}}{w_{A,I}}) > \alpha_A \ln(\frac{w_{A,F}}{w_{A,I}}) - \ln(\frac{w_{A,F}}{w_{A,I}} \times \frac{w_{A,I}T+m}{w_{A,F}T+m})$, 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 (6) implies (7), 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 (7), for these workers, becomes:

$$\mu_A > \alpha_A \ln\left(\frac{w_{A,F}}{w_{A,I}}\right) - \ln\left(\frac{w_{A,F}}{w_{A,I}} \times \frac{w_{A,I}T + m}{w_{A,F}T}\right) \qquad \text{(7bis)}$$

which is also trivially implied by (6).

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

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

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 (5)

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

And face a new inequality

$$\mu_B < \alpha_B \ln\left(\frac{w_{B,F}}{w_{B,I}}\right) - \ln\left(\frac{w_{B,F}}{w_{B,I}} \times \frac{w_{B,I}T + m}{w_{B,F}T + m}\right) \tag{8}$$

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

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

if condition (8) still holds, then these workers will stay in the formal sector and work less hours:

$$\Delta_m(l) = -\frac{1-\alpha_B}{\alpha_B}\frac{m}{w_{B,F}} < 0 \tag{9}$$

if condition (8) does not hold, then these workers will switch to the informal sector and still work less hours:

$$\Delta_m(l) = -\frac{1-\alpha_B}{\alpha_B}\frac{m}{w_{B,I}} < 0 \tag{10}$$

Note that whether the drop in labor supply for "stayers" (9) is larger than that of "switchers" (10) 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 α_B, or because of a smaller μ_B. All other things equal, if switching to informal work as a result of the pension m is due to a smaller α_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 (5)

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

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 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:

$$u_m^*(I) \lesseqgtr u(\bar{W} + m, T - l_{\bar{W}}, F) \tag{11}$$

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:

$$\mu_C \lesseqgtr \alpha_C \ln\left(\frac{w_{C,F}}{w_{C,I}}\right) - \ln\left(\frac{w_{C,F}}{w_{C,I}} \times \frac{w_{C,I}T + m}{w_{C,F}T + m}\right) \tag{12}$$

Note that (12) is the same as (8). 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:

$$\mu_C \gtreqless \alpha_C \ln\left(\frac{w_{C,F}}{w_{C,I}}\right) - \ln\left(\frac{w_{C,F}}{w_{C,I}} \times \frac{w_{C,I}T + m}{w_{C,F}T}\right) \tag{13}$$

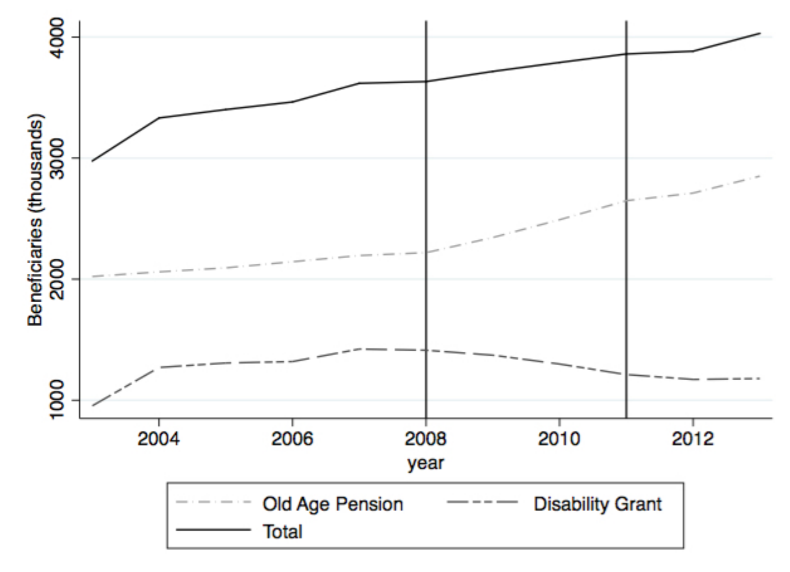
7.2 Tables and Figures

Table A1: Evolution of the *Old Age Pension*, 1993–2010

Date	Age threshold Men	Women	Amount	Amount (R '10)	Means-Test
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)	**63**	60	R 940	R 1049	R 26,928
2009(Q2)	**61**	60	R 1010	R 1051	R 27,552
2010(Q2)	**60**	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 (2013). Information on the means-test for 2009 and 2010 is collected from US (2015); Ranchhod (2006) for 2003; Case and Deaton (1998) for 1994; and from SASSA (2010, 2013, 2014, 2016) for all remaining years.

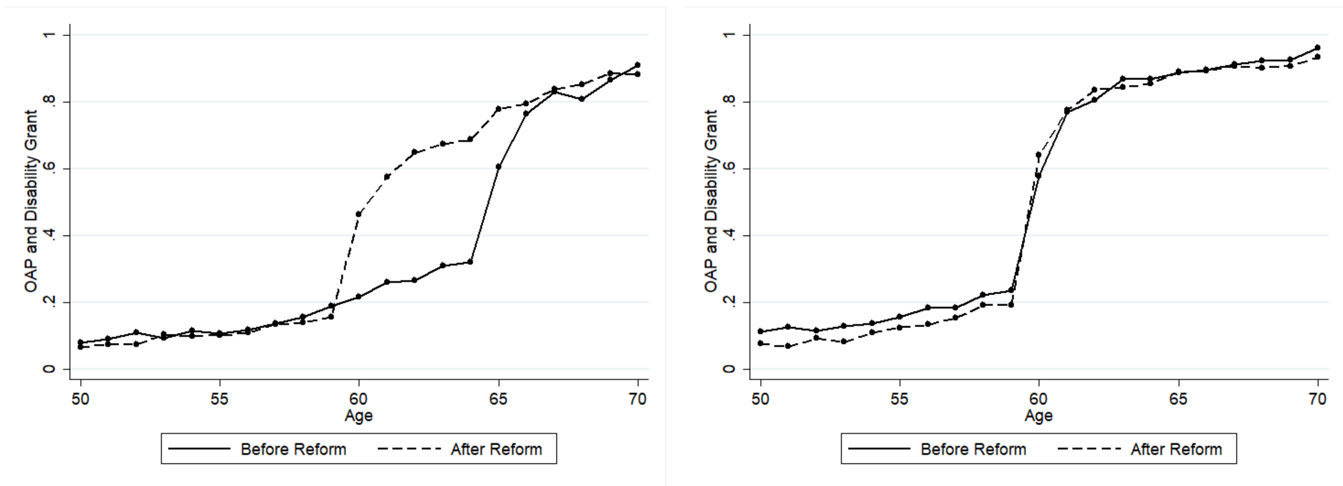
Figure A1: Number of *Old Age Pension* and *Disability Grant* Beneficiaries, 2003–2013



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 (2013). These figures come from administrative data (SOCPEN). When there are small discrepancies for the same year, the latest available estimate is used.

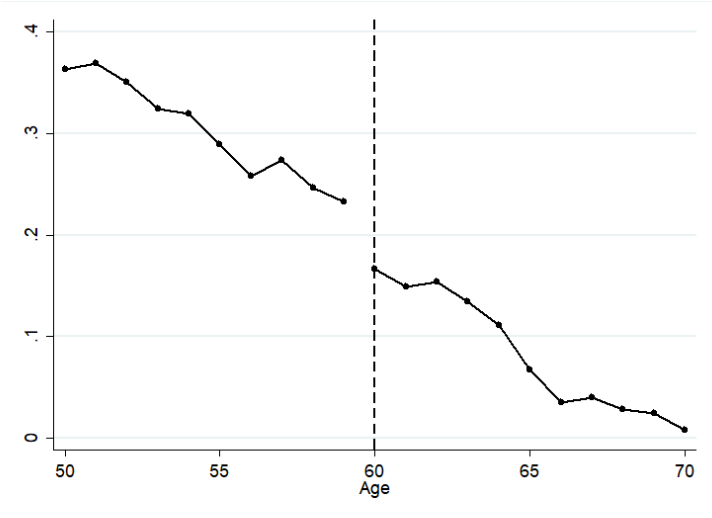
Figure A2: Share of People Receiving the *Old Age Pension* or *Disability Grant* by Age, **Before and After** Pension Reform, Black and Coloured Only

(a) Men (b) Women



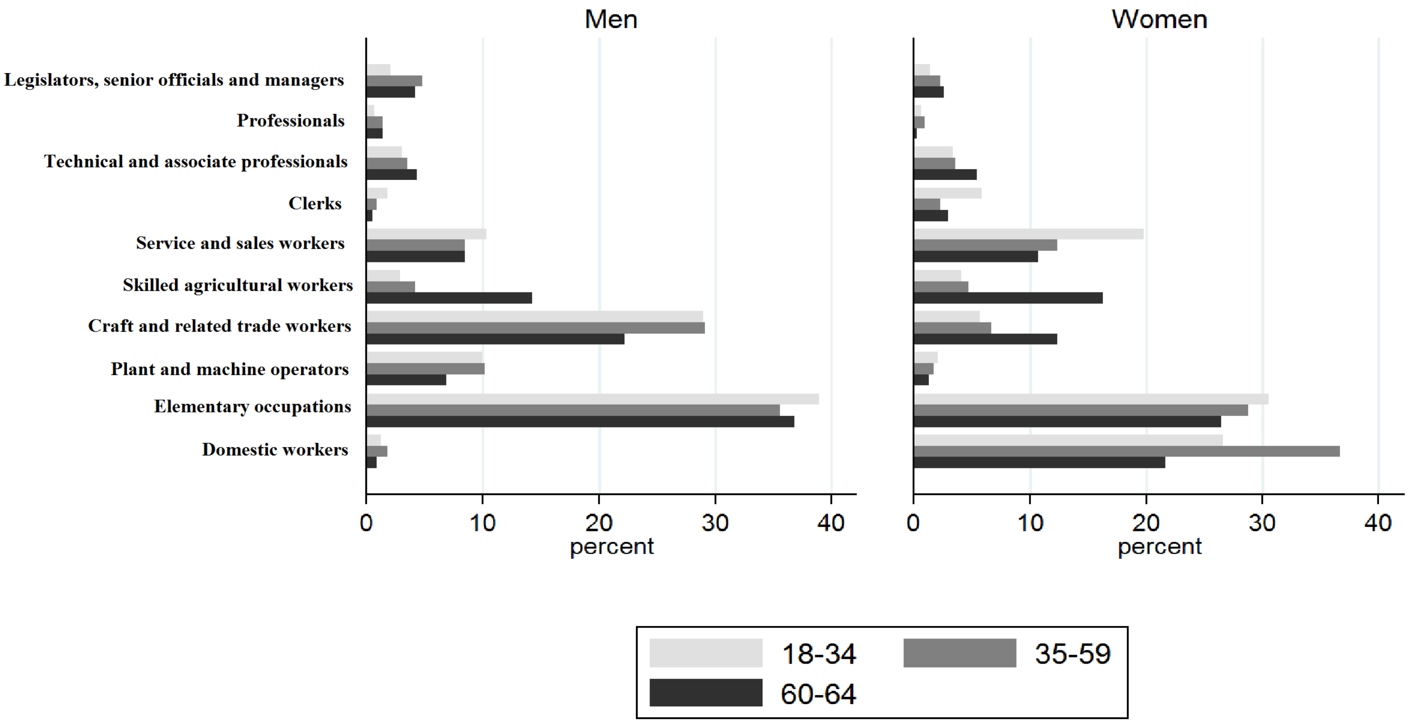
Note: These graphs plot the share of individuals receiving the *Old Age Pension* or the *Disability Grant* within each age bin, for men and women separately. The sample is restricted to the Black and Coloured population. The solid line is for the years before the reform (2002–2007), while the dotted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

Figure A3: 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).

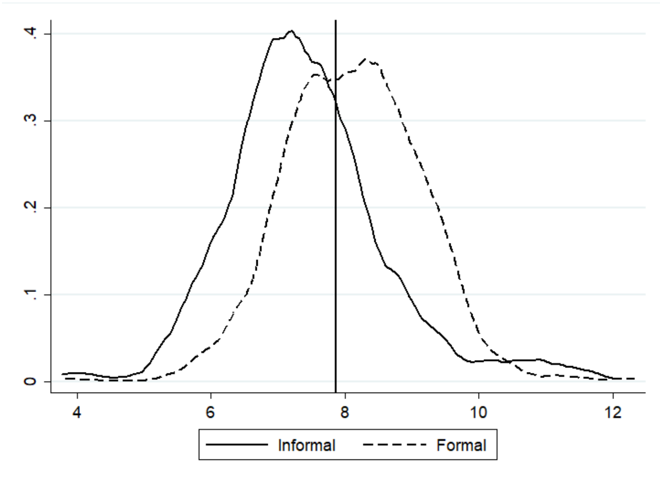
Figure A4: Informal Employment by Occupation and Age Groups, Men and Women, PALMS



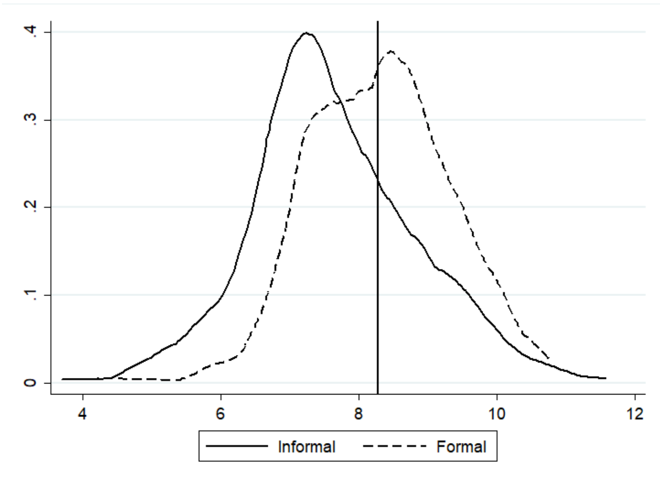
Note: These graphs plot the share of informal employment across occupational categories for different age groups, for men and women respectively. The age groups are 18–35 years old, 35–59, and 60–64.
Source: Authors' calculations on PALMS 3.2 (2006–2008).

Figure A5: Position of the Means-Test with Respect to Formal and Informal Monthly Earnings, 2010, 2012, 2014

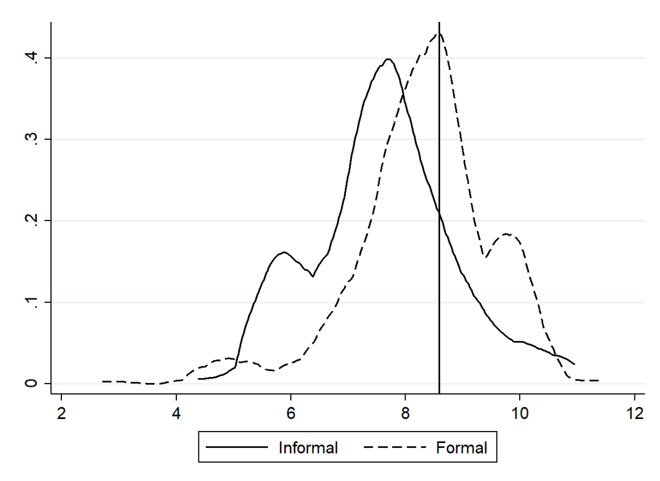
(a) 2010



(b) 2012



(c) 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 A2: Characteristics by Labor Market Status, Men aged 60–64, Before Reform, 2002–2007

Characteristics (pop %)	Informal	Formal	Non- Employed
Proportions	0.13	0.15	0.72
Socio-Demographics			
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
Job Characteristics			
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	

Note: 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 A3: Old Age Pension and Employment, RDD Results, Linear Fit 55–64, PALMS

	Before Reform			After Reform		
	(1)	(2)	(3)	(4)	(5)	(6)
	Employed	Informal	Formal	Employed	Informal	Formal
a. *Men*						
$Age_{(60+)}$	-0.0573***	-0.0007	-0.0566***	-0.1097***	-0.0471***	-0.0626***
	(0.0182)	(0.0140)	(0.0157)	(0.0107)	(0.0077)	(0.0100)
Mean Y 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. *Women*						
$Age_{(60+)}$	-0.0812***	-0.0510***	-0.0302***	-0.0720***	-0.0430***	-0.0290***
	(0.0124)	(0.0104)	(0.0084)	(0.0083)	(0.0061)	(0.0068)
Mean Y 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 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, β_{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).

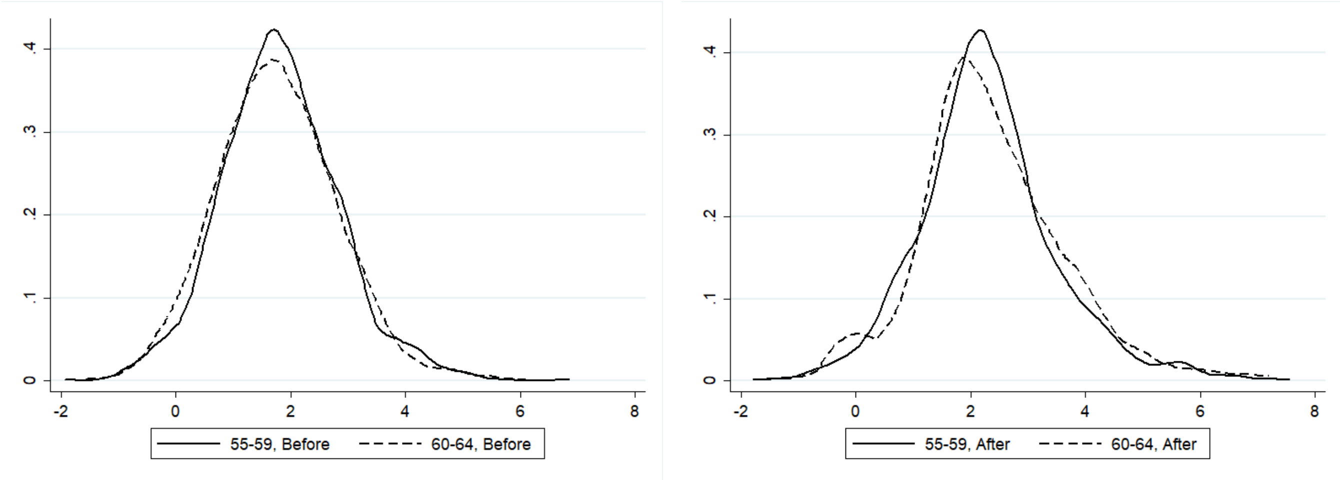
Table A4: Old Age Pension and Employment, Linear Fit 55–64, Diff-in-Disc, PALMS

	Employed	**Informal**	**Formal**	
			Extensive	Intensive
	(1)	(2)	(3)	(4)
a. *Men*				
$Post \times Age_{(60+)}$	-0.0539**	-0.0465**	-0.0074	-0.1027
	(0.0214)	(0.0179)	(0.0216)	(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. *Women*				
$Post \times Age_{(60+)}$	0.0092	0.0080	0.0012	0.1025
	(0.0129)	(0.0145)	(0.0116)	(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 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, β_{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 A6: Distribution of Informal Hourly Wages, 55–59 vs. 60–64, Before and After

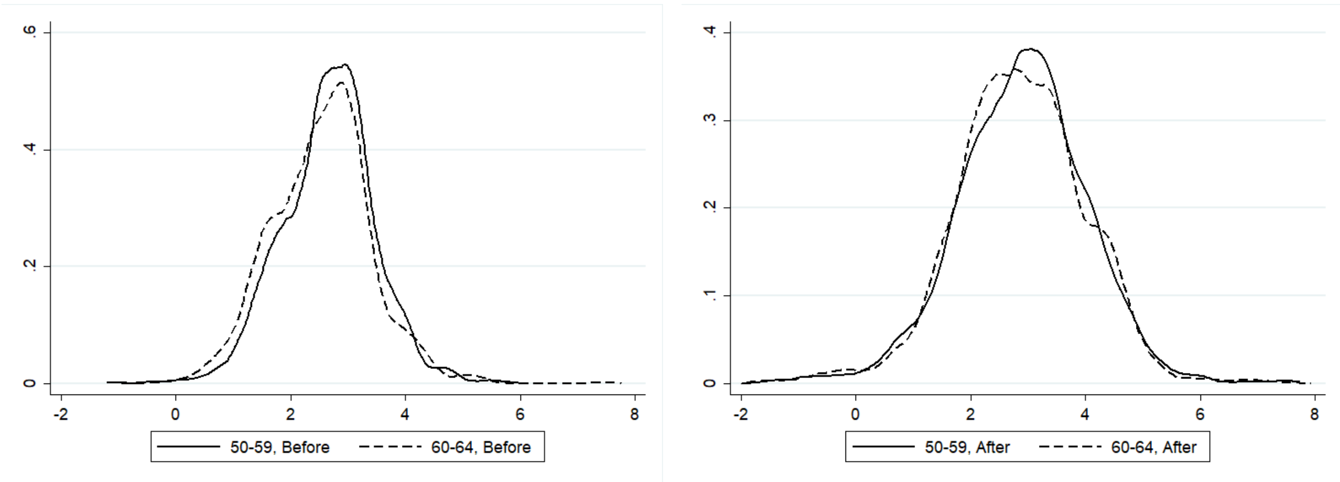
(a) Before (b) After



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 A7: Distribution of Formal Hourly Wage, 55–59 vs. 60–64, Before and After

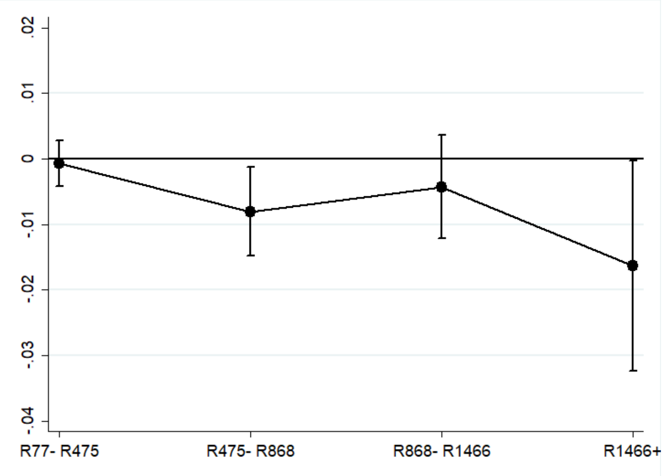
(a) Before (b) After



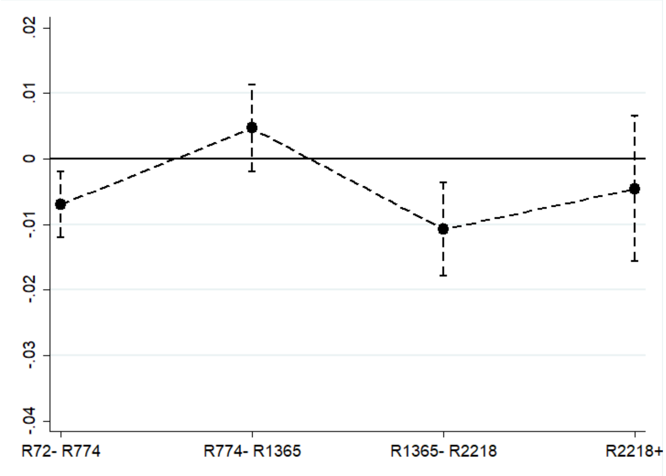
Note: These graphs plot the distribution of log-hourly wage for formal workers by age group before the reforms (2002–2007), panel (a), and after the reform (2010–2015), panel (b), adjusted for inflation. The sample includes Black and Coloured males only.
Source: Authors' calculations on PALMS 3.2 (2002–2007 and 2010–2015).

Figure A8: 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 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 A5: Density and Balancing Test, PALMS, Men Only

	Diff-in-Disc
	(1)
a. Density	
Log of Individuals	
	-0.1444
	(0.1085)
Observations	240
b. Balancing Test	
Y Variable	
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)
Observations	166,599

Note: *** p<0.01, ** p<0.05, * p<0.1. The sample is restricted to men. This table reports the coefficient β_{DiDRD} of Equation 2 with a quadratic function, on the age window 50 to 70. In the upper panel, observations are collapsed at age×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).

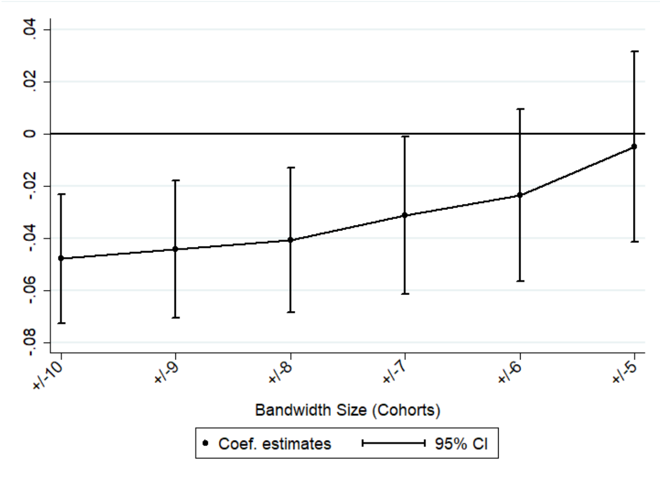
Table A6: Old Age Pension and Employment, Linear Fit 60–70, Diff-in-Disc with Threshold at 65, PALMS

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

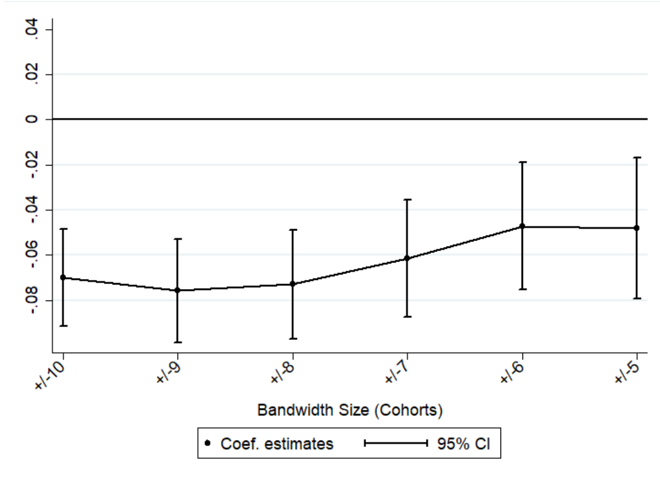
Note: *** p<0.01, ** p<0.05, * p<0.1. This table gives the results of Equation 2 with a linear function, for men (upper panel) and women (lower panel) on the age window 60–70. The threshold is set at 65, rather than 60. We only report the coefficient of interest, β_{DiDRD}. The dependent variables are 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 A9: 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 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).

Table A7: Old Age Pension and Employment, Quadratic Fit 50–70, Diff-in-Disc, PALMS

	Informal	**Informal**	
		Wage earners	Self-employed
	(1)	(2)	(3)
$Post \times Age_{(60+)}$	-0.0511**	-0.0314**	-0.0197
	(0.0195)	(0.0138)	(0.0127)
Mean Y at Age 59	0.16	0.09	0.07
Observations	140064	140064	140064
R-squared	0.0264	0.0205	0.0099

Note: *** p<0.01, ** p<0.05, * p<0.1. This table gives the results of Equation 2 with a **quadratic function**, for men on the age window 50–70. The sample is limited to Black and Coloured men. We only report the coefficient of interest, β_{DiDRD}. The dependent variables are binary variables for: (1) informally employed, (2) informally wage-employed, and (3) informally self-employed, such that (1)=(2)+(3). *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).

7.3 Studying the Market-Level Effects of the Pension

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 et al. (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 appendix 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 A10 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".[23] 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.

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 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 A12 displays the result on employment rate. With regards to 60–64 Black

[23] Weighting the distribution of coefficients by the size of the employment of each sector guarantees that treatment and control groups are of equal size.

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 deferentially 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 A11).

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

$$\mathrm{Y}_{c,t} = \kappa_c + \tau_t + \theta_1 T \times \{2010\text{q}2 < t < 2014\} + \theta_2 T \times \{2014 < t\} + \epsilon_{c,t} \tag{14}$$

where $Y_{c,t}$ is the outcome of interest in cell c at time t, κ_c and τ_t are cell and time-period fixed effects, T is and 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 θ to be flexible over time and split it in two components, a short- and a medium-term effect.

Results are displayed in Table A8. 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.[24] 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

[24]The small discrepancy could come from the fact that we are focusing here only on the two most extreme quartiles of sectors.

cell by its total size in employment in the pre-reform period does not qualitatively affect these conclusions.

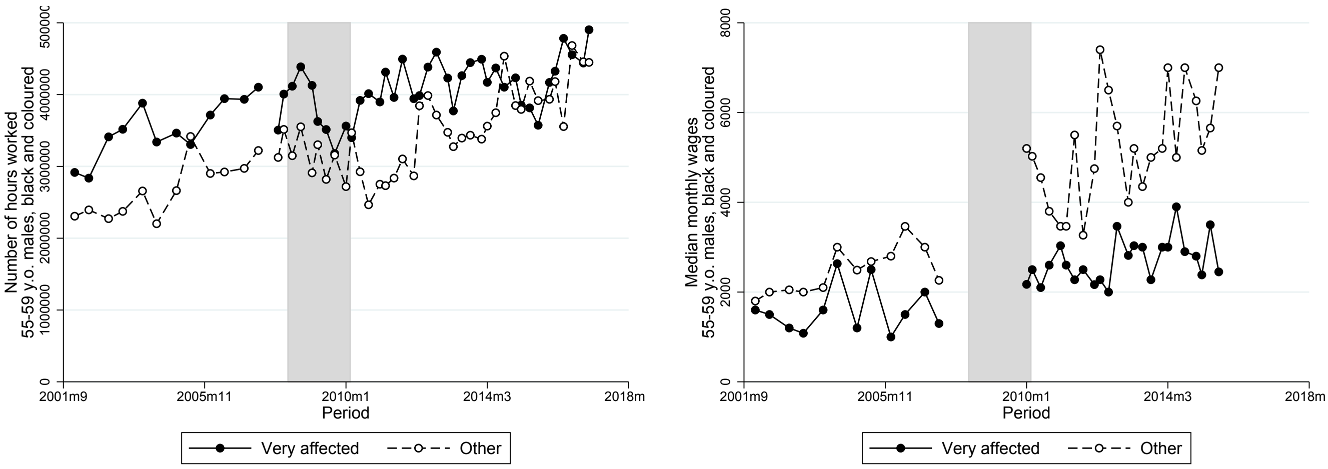
Estimates on the 55-59 y.-o. men do not show any sizable nor statistically significant effect of the reform in sectors where men aged 60–64 were more affected. The positive estimates in the unweighted case – which would be compatible with younger men picking up the jobs that the 60–64 are leaving – are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

Figure A10: Heterogeneous Effects of the Pension Reform by Industry × Occupation Cells

Note: This graph plots the point estimate of Equation 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).

Figure A11: Effect of the Reform on Hours Worked and Wages of 55–59 y.-o. Black and Coloured Men

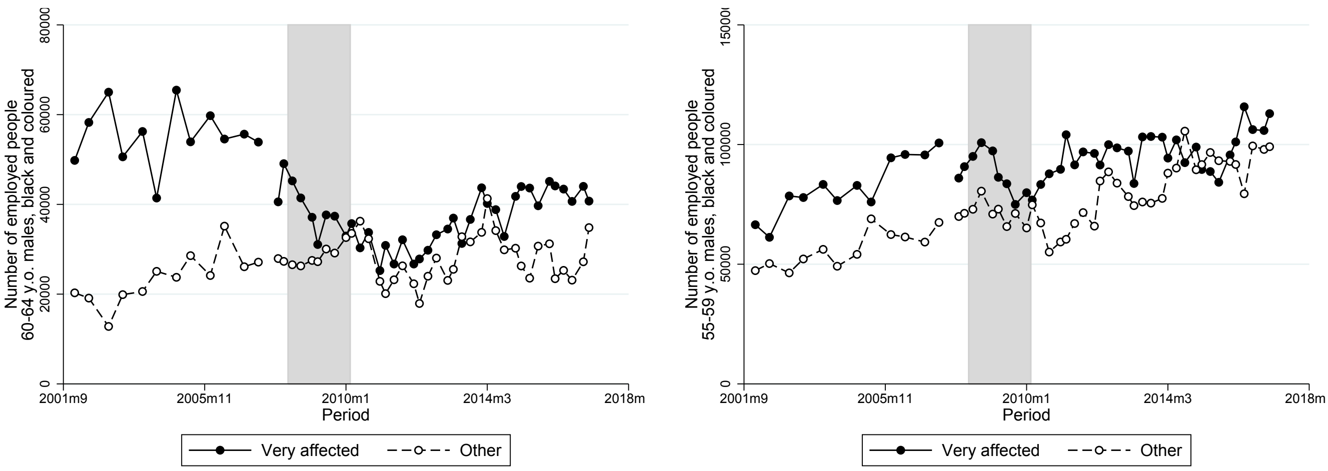
(a) Hours worked (b) Median wages



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 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).

Figure A12: Effect of the Reform on the Employment of 60–64 and 55–59, Top vs. Bottom Quartile

(a) 60–64 y.-o. men, Black and Coloured (a) 55–59 y.-o. men, Black and Coloured



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. 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).

Table A8: 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.
Panel A: Nb of employed people						
Treatment $\times$(2010q4 $\geqslant$ Year $<$ 2014)	-2043.8**	-2120.7*	630.9	-3314.8**	-4067.8**	-1624.8
	(990.4)	(1204.4)	(1130.5)	(1583.7)	(1856.4)	(1822.6)
Treatment $\times$(2014 $\geqslant$ Year $\geqslant$ 2017)	-1233.7	-1163.3	319.9	-2794.1*	-3461.0**	-2831.4
	(957.7)	(1134.0)	(1150.1)	(1454.9)	(1678.3)	(1921.9)
N	1764	1764	1764	1722	1722	1722
Panel B: Nb of hours worked						
Treatment $\times$(2010q4 $\geqslant$ Year $<$ 2014)	-84524.6**	-93976.7**	39030.4	-124636.7**	-166383.4**	-16145.1
	(33885.4)	(43854.1)	(42554.6)	(51283.5)	(63566.8)	(64835.9)
Treatment $\times$(2014 $\geqslant$ Year $\geqslant$ 2017)	-46549.3	-47047.1	21958.8	-98312.1**	-128169.1**	-79453.2
	(31997.0)	(40414.5)	(41289.6)	(43469.8)	(55193.4)	(65113.8)
N	1764	1764	1764	1722	1722	1722
Panel C: Median monthly wage						
Treatment $\times$(2010q4 $\geqslant$ Year $<$ 2014)	-277.9	-1308.2	-1839.3***	-199.0	-1744.2**	-1634.8***
	(1288.0)	(981.6)	(446.4)	(2079.9)	(796.0)	(442.3)
Treatment $\times$(2014 $\geqslant$ Year $\geqslant$ 2017)	3490.7	-208.5	-1677.2***	5719.4	-922.3	-1542.1***
	(3326.8)	(1064.7)	(506.5)	(5281.1)	(848.5)	(466.1)
N	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 14 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.

7.4 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: $D = \mathbb{1}\{X \geqslant 60 \cap T = 1\}$. $\tau = E[Y_{11} - Y_{10}|X = 60]$ 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:

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

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

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

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:

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

(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:

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

as documented for instance in (Grembi et al., 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 $\tau_{\bar{w}}$:

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

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

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

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

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[25]. 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 τ 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:

[25] In any case, the latter density is observed, and can thus be estimated.

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

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

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

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.