Unintended Labour Supply Effects of Cash Transfer Programmes: Evidence from South Africa's Old Age Pension

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Abstract

Employing South Africa's first nationally representative panel data set, I find that having old age pension recipients in the household adversely affects employment outcomes of prime-aged adults both by reducing the probability that the unemployed find work and by increasing the likelihood that the previously employed lose their job. These effects seems to operate through the income mechanism: an increase in pension resources increases the reservation wage and lowers labour force participation of prime-aged household members. By contrast I find evidence against the hypothesis that pensioners provide childcare which allows parents to work. Instead, gaining a pensioner lowers the probability that mothers are employed. Adverse employment effects are found for salaried work and self-employment while the pension does not affect casual work. Impact estimates are larger in metropolitan areas which questions previous studies that find that pension resources finance labour migration. Results are robust to a series of novel robustness tests to isolate the income effect of the pension exploiting institutional features of the disability grant.

1 Introduction

In the last decade, the role of social protection programmes in developing countries has received increased attention – in the words of a recent UN report, social protection experienced a "*renaissance in the global development discourse*" (UN 2012, p.4). Bourguignon and Ravallion (2004) argue that by reducing poverty and vulnerability social protection programmemes not only facilitate investment in human capital and correct market failures, but also foster social cohesion and reduce the likelihood of conflict. Recent evidence on the effects of large programmemes in developing countries like *Progresa* in Mexico or *Bolsa Familia* in Brazil confirms this positive view. These programmes are often found to increase educational attainment, improve child health, reduce child labour, or increase bargaining power of women (for a review see Parker, Rubalcava and Teruel 2007, for a meta-analysis see Saavedra and Garcia 2012). While many countries implemented conditional cash transfer programmes, recent studies from Morocco and Malawi question whether conditionalities are necessary to achieve these positive results (Baird, McIntosh and Ozler 2011, Benhassine, Devoto, Duflo, Dupas and Pouliquen 2013). *Unconditional* transfer cash transfer programmes that do not require extensive monitoring and targeting efforts may in fact be more cost-effective in achieving

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these goals (Caldes, Coady and Maluccio, 2006). This paper will contribute to this debate by looking at unintended labour supply consequences of South Africa's pension, Africa's largest cash transfer programme.

One of the legacies of South Africa's *apartheid* system is a generous social security system relative to other developing or middle income countries (Lund 1993, Case and Deaton 1998, van der Berg 1997). At the center of this welfare system is the old age pension (OAP), a non-contributory state pension that pays almost twice the national median per-capita income. Although the OAP is means-tested, more than 90% of black South Africans are eligible to receive the grant once they turn 60 years old and 86% of those eligible take it up (McEwen, Kannemeyer and Woolard 2009). With this high share of the population receiving the grant, the pension plays a central role in the government's redistribution efforts (Woolard, Harttgen and Klasen 2010). The OAP is in effect akin to an almost universal unconditional cash transfer programme targeted at seniors. However, in times of persistently high unemployment and stagnating tax revenues from the mining sector, the effects of the grant system that accounts for 3.2% of GDP has been at the center of the public and academic debate (Hagen-Zanker, Morgan and Meth 2011).

Due to the prevalence of multi-generational households in South Africa and the potential for spillover effects between household members, a large body of academic research has been devoted to understanding the impact of the pension on both the beneficiaries themselves and other household residents. Five themes in particular have emerged in the literature: the effect of the pension on (i) poverty and income inequality (Woolard and Leibbrandt 2010), (ii) household investment in human capital and child welfare (Case and Deaton 1998, Duflo 2003), (iii) gender equality (Holmes and Jones 2010), (iv) household composition (Klasen and Woolard 2009) and (v) labour supply (Bertrand, Miller and Mullainathan 2003, Ardington, Case and Hosegood 2009). This study will contribute to the literature on the effect of the pension on labour supply and employment of prime-aged adults living in the same household as pensioners.

Previous studies on this question reached contradictory conclusions about whether the effect of the pension on labour supply is positive or negative. In addition, causal mechanisms that led to the change in employment remained unexplored. I will address these issues by developing a theoretical framework to derive testable hypotheses on the direction of change and the underlying mechanisms. Exploring two waves of data from the National Income Dynamic Study (NIDS), South Africa's first nationally representative panel data, I find that the change in the number of pensioners is negatively correlated to employment outcomes of prime-aged adults living in the pensioner's household. Employing fixed effect estimation and instrumenting for pension take-up, I argue that this can be interpreted as a negative *causal* effect of the pension.

Adverse effects are observed for both salaried- and self-employment, while there is no change in the probability of being casually employed. This indicates that the pension affects the decision whether to join the labor force rather than the choice among different forms of work which vary in flexibility and job certainty. The negative effect of pension receipts on self-employment further suggests that it is unlikely that credit-constraints can explain the low levels of entrepreneurship in South Africa. This result contrasts to a recent study finding that cash grants had a significant positive effect on firm formation in Uganda (Blattman et al. 2013).

To isolate the income effect of the pension change, I employ two placebo tests. Almost 20% of the newly eligible pensioners previously received the disability grant, which pays exactly the same amount as the pension. The finding that gaining a pensioner who previously received the disability has a positive (insignificant) employment effect corroborates the interpretation that the negative effect of gaining a non-disabled pensioner is due to the income change rather than differences in the age structure of households. One concern with estimating the employment effect of losing a pensioner is that the actual income shock for the household is larger than the loss in pension income

due to the very high funeral costs in South Africa. To address this possibility, I compare the effect of losing pension-eligible to pension-ineligible household members. Under the assumption that the cost of funerals does not depend on the age of the deceased, findings suggests that employment effects are due to the loss in pension income instead of funeral expenses.

In testing different causal mechanisms, I find evidence against the hypothesis that pensioners affect employment outcomes of parents by providing childcare. Instead, an increase in resources from the pension is associated with a reduction of labour force participation among mothers. In contrast, I find support for the hypothesis that the pension operates through an income mechanism. A gain (loss) of an eligible pensioner leads prime-aged adults to withdraw from (join) the labour force and increases (reduces) their reservation wage. These results are stronger in metropolitan than rural areas which at least partly reconciles contradictory findings from previous studies. This also questions the importance of labour migration in explaining employment effects since adults in metropolitan areas already reside where most job opportunities are located.

Aggregate effects mask that the effect of the pension varies among different subgroups. I find that the pension affects employment of both previously employed and unemployed adults. Gaining a pensioner reduces the chances that unemployed adults find work and increases the probability that previously employed people lose their job. These effects further differ for women and men. Among respondents *unemployed* at the time of Wave 1, gaining a pension-eligible household member reduces the probability that women find a job by 6.5 percentage points (p.p.) while the effect for men is also negative but insignificant. Both unemployed men and women do not gain employment after losing a pensioner, possibly as a result of the limited number of vacancies in the South African labour market. Among previously *employed* adults, both men and women are about 10 p.p. less likely to lose employment after losing a pensioner. By contrast, gaining a pensioner does not significantly affect employment of employed women, but substantially increases the probability that men lose employment. However, the latter results should be interpreted with caution because effects can only be estimated imprecisely. These results are intent to treat estimates and thus present lower bounds of the true effect. Findings on the employment effect of the pension are robust to a range of sensitivity tests.

This study points to the importance of potential unintended effects of cash transfer programmes. However, the negative employment effects do not imply a negative welfare effect. In fact, a cash transfer does not distort behavior since adjustments only capture an income and not the substitution effect resulting from wage changes. For example, the finding that pension resources enable women with young children not to work should thus be seen as welfare improvement. Yet, a full welfare analysis needs to take into account the possibility that people fail to optimize behavior (for example due to hyperbolic discounting) as well as the distortionary effects of raising taxes that pay for the pension system.

The paper proceeds as follows: Section 2 provides background information on South Africa's old age pension system and reviews the literature on the pension's effect on employment of household members living with pensioners. Section 3 develops a framework on potential causal mechanisms and identifies testable hypotheses. Section 4 discusses the data and describes the identification strategy and analysis plan. Section 5 presents empirical results. Section 6 applies a battery of robustness tests and Section 7 concludes with a discussion of the results.

2 Background

2.1 South Africa's Old Age Pension

Case and Deaton (1998: 1334) conclude that South Africa's Old Age Pension scheme was a "*largely unintended consequence of the country's recent history*". It was first implemented in 1927 to support the movement of white people out of poverty. Since most whites were covered by private occupational pension schemes, the OAP was designed as a safety net for a relatively small number of workers without coverage (Lund 1993). Facing international pressure, the *apartheid* government started to gradually extend welfare benefits and improve the delivery to non-white communities, first to Coloureds and Indians starting around 1965 and a decade later also to Africans (Posel, Fairburn and Lund 2006, Woolard et al. 2010). When *apartheid* ended in 1994, social and political pressure led the newly-elected ANC government to continue the expansion of the generous welfare system to previously disadvantaged groups (Van der Berg 1997). As a result, South Africa has a notably well-developed social security system for a middle income country (Woolard et al. 2010).[1]

Figure 1 shows that South Africa's current social security system has two pillars: statutory and voluntary funds that comprise the social insurance pillar and social grants as the social assistance pillar. Only a small minority has access to the social insurance programmes as only people with continuous formal employment become eligible. In 2012, the OAP amount of 1,200 Rand per month (about USD 130 or USD 195 PPP adjusted) is about four times larger than the Child Support Grant (CSG).

Every South African citizen or permanent resident over the age of 60 with an annual income below R47,400 (about USD 5,000) and assets below R792,000 who is living in South Africa is eligible to receive the grant. In practice, the income but not the assets criterion is used in the means test due to difficulties with the valuation of assets (McEwen et al. 2009). Furthermore, eligibility requirements do not stipulate that recipients are not allowed to work. As a result of these low barriers to access and the substantial monetary value, more than 90% of Africans are eligible and take-up of the OAP is high: about 86% of eligible Africans and 82% of eligible Coloureds receive the pension (McEwen et al. 2009).

The OAP is delivered each month in cash at specific pay point locations or via electronic deposit if the recipient has a bank account. The amount of the grant has increased by 224% in nominal terms, but only 16.5% in inflation-adjusted terms since the end of *apartheid* in 1994. The monthly pension amount of R 1,200 corresponds to about 175% of the median wage in South Africa (Wooldard et al. 2010).[2] The contribution of the grant to household income is underscored by the fact that 85% of households receiving the pension name "pensions and grants" as their most important source of income (Sienaert 2008). There is a significant gender imbalance among recipients: 71.3% of grant recipients are women (McEwen et al. 2009). While longer life expectancy and lower labour force participation among women are two explanations for this phenomenon, they are unlikely to be the sole reasons for this large gender gap. At the time of the first wave of NIDS, close to 80% of women between the age of 65 and 69 received the grant compared to barely 60% among men. Why so many eligible men do not receive the grant remains a puzzle.

Given the size of the grant relative to other forms of social transfers, high pension take-up and prevalence of multi-generational households in South Africa (only 7 percent of pensioners live without at least one 19-50 year old (Sienaert 2008)), it is not surprising that the literature has found that

[1] South Africa spends about 15% of all government expenditure on social protection compared to around 4-8% of most other countries in southern Africa (SPII 2011).

[2] Jensen (2003) finds some evidence that the pension crowds out private transfers: every rand transferred reduces private transfers by 0.25 to 0.3 Rand.

the pension leads to intra-household resource transfers ranging from paying for children's health and education expenses (Duflo 2003) to financing job migration of adults (Ardington et al. 2009). Next, I will review this literature focusing on labour supply related intra-household spillovers of the pension.

2.2 Literature

The analysis on the effect of large social transfers on employment of other household members is related to two bodies of literature. Firstly, it relates to the literature of intra-household resource transfers and decision making. Previous research on South Africa's pension by Duflo (2003) and Ambler (2011) finds that an intra-household resource transfer takes place. However, these authors reject the unitary model of the household which predicts that resources get spent in the same way regardless of who earns the income or owns assets. Instead, these studies find evidence in favour of bargaining models. Households with female pension recipients allocate more resources to health and education of children, expenditures typically more closely aligned with preferences of women. Likewise, Ardington et al. (2009) conclude that female pensioners are more likely than male pensioners to finance labour migration of other household members.

Secondly, assuming that some of the pension is transferred to prime-aged adults, as is suggested by previous research, this study also relates to the literature on the effect of income on labour supply. However, given the limitation that no information on the actual resource transfer to each household member is collected in NIDS, computing individual labour supply elasticities is not possible without strong assumptions.[3] The employment effect of cash transfers is also linked to the theory of intra-household decision making and relates to how household members react differently to income shocks. In the context of this study, men and women may have different labour supply adjustments after gaining or losing pensioners who may have provided income and childcare.

The evidence from studies that estimate the effect of South Africa's old age pension on labour supply of prime-aged household members is mixed. Bertrand, Mullainathan and Miller (2003) analyse cross-sectional data from a nationally representative PSLSD survey of 9,000 households collected in 1993 and find that eligibility for the social grant is associated with strong negative employment effects for other household members, especially men. These results are supported by Ranchhod (2009) who finds that the cessation of pension receipt is associated with a statistically significant increase in employment rates of resident prime-aged household members.

Posel, Fairburn and Lund (2006) argue that Bertrand et al.'s (2003) results are misleading because the study only examines the effect of pension eligibility on household members who are resident at the time of the survey and thus neglects the potential positive labour supply effect through increased labour migration. Using the same cross-sectional data and methodology, the authors conclude that old age pension eligibility in fact increases female labour supply once labour migrants are included in the sample. They speculate that the pension has this effect because it alleviates the income constraints to migration and enables pensioners to look after grandchildren (Posel et al. 2006).

Ardington et al. (2009) use a panel dataset of about 25,000 individuals from one of the poorest districts in northern KwaZulu-Natal to assess migration patterns in pension-receiving household controlling for time-invariant individual characteristics. Similar to Posel et al. (2006) they find positive employment effects of about 3 p.p. for both men and women. They also conclude that labour migration is facilitated by alleviation of financial and child care constraints. Sienaert (2008) uses the labour Force Survey (LFS) to confirm significant positive migration impacts of the pension,

[3]Previous research by Bertrand et al. (2003) has computed labour elasticities at the household level. However, it is problematic that the authors simply assume that the pension gets equally divided among all prime-aged adults and that they compute individual income by dividing household income by the number of household members.

but finds modestly negative employment effects overall.

The existing literature suffers from a number of data limitations that I hope to address using NIDS, the first nationally representative panel data set in South Africa. Analyses of cross sectional data used in the studies by Bertrand et al. (2003) and Posel et al. (2006) are potentially biased by the fact that households with pension recipients are likely to differ from non-recipient households along unobservable characteristics. For example, Klaasen and Woolard (2009) find that households with pension income attract unemployed people to join the household. Longitudinal data analyses, as employed by Ardington et al. (2009), Sienaert (2008) and Ranchhod (2009) can help to alleviate this endogeneity problem. These studies, however, suffer from other data limitations: The study by Ardington et al. (2009) only employs data from one specific rural district in South Africa, which may limit the generalisability of the findings. Sienaert (2008) uses the rotating panel of the South African LFS, which only records household identifiers but not individual identifiers across waves. The analysis can thus only be done at the household level and so precludes an analysis of individual behaviour. Likewise, Ranchhod (2009) constructs an individual level panel data set from the South African Labour Force Survey (LFS). However, any individuals who migrated into or out of households are excluded from the panel. In addition, the LFS only asks unemployed persons whether they receive the pension and Ranchhod only looks at the effects of losing a pensioner.

In addition to data limitations, institutional details of the South African grant system have not been exploited. Most notably, while many studies have used the age eligibility cutoff rule to estimate the effect of the pension, none have discussed that about 15-20% of the newly eligible old age pension recipients previously received the disability grant[4], which is automatically converted into the old age pension once the individual becomes eligible. Since disability and old age grants have the same means test and pay the same amount, this group of new pensioners does not experience an income increase. This implies that (i) they should not be treated as new pension recipients as previous cross-sectional or longitudinal analyses do and (ii) they can serve as a control group in a robustness check to control for the effect of age.

3 Framework

Previous studies argue that the pension can affect employment through two mechanisms. First, the pension can be used to finance labour migration and other search costs. Second, pensioners can provide childcare at home thus allowing parents to find employment (Ardington et al. 2009, Posel et al. 2006). However, testing of these mechanisms has remained inconclusive, partly because intermediate outcomes and underlying assumptions have not been identified and tested.

Figure 2 disentangles the causal chain through which residing with a pensioner may affect employment outcomes of prime-aged adults. Assumptions underlying each step in the causal chain are listed in the grey boxes. This figure depicts the effect of a household *gaining* a pensioner. The reverse relationship for each causal chain holds for cases where households lose a pensioner.

Before analysing the separate causal chains in more detail, it is obvious from the intermediate outcome measures that the overall predicted effect of the household income change on *employed* prime-aged adults is negative on both the intensive (hours worked) and extensive (being employed) margin. Intuitively, adults can afford to consume more leasure and there is substition effect since the returns to work are unaffected. By contrast, the effect for prime-aged *unemployed* adults is ambiguous leading to an overall ambiguous employment effect for prime-aged adults: while gaining a pensioner may alleviate credit constraints and thus increase search activity among the unemployed,

[4]In 2008, there were 1.36 million disability grant recipients in South Africa, compared to 2.15 million old age pension recipients.

it could also reduce the pressure for people to find a job. This motivates an empirical investigation of this question.

Hypothesis 1: Pension resources reduce employment for adults with work both at the intensive and extensive margin. The overall employment effect of the pension on unemployed adults is ambiguous.

In order for the pension eligibility to have any effect on employment through the income channel, two basic assumptions need to hold. Firstly, the eligible person needs to file the paperwork to receive the pension. As discussed earlier, a significant share of pension-eligible people fail to do so. A second assumption, not discussed in the literature but required for the income channel to operate, is that the new pensioner experiences an income *increase*. If people had higher paying jobs that they quit upon becoming pension-eligible, households would experience a decrease in income. Of the newly eligible pensioners in NIDS, only 14.5% reported having a salary in Wave 1 that is larger than the old age pension amount. Of this group, 74% report not earning any income from employment in Wave 2. This means that a total of about 10.7% of all newly eligible pensioners experience an income reduction.

Dividing pensioners into the three groups of previous disability grant recipients, high income pensioners and low income pensioners has the additional advantage that it provides hypotheses on pensioner-subgroups for which we should not observe any employment effect. These hypotheses can be employed for a robustness test of our results.

Hypothesis 2: Having a newly eligible pensioner who:

i) previously received the disability grant does not affect employment outcomes as it does not alleviate the income or childcare constraint.

ii) previously received a higher income than the pension and stops working does not increase employment through the income channel as the household experiences an income decline.

Hypothesis 1 and 2 will be tested in section 5.1. Next, I will discuss the two main mechanisms in more detail and identify testable hypotheses. The **childcare mechanism**, depicted on the right side of Figure 2, implicitly assumes that newly eligible pensioners were i) previously employed and thus not available to take care of children and ii) quit their job after becoming eligible. One way to formally test the childcare constraint mechanism is to estimate how pension effects differ for respondents, and particularly women, with and without children and whether this effect is larger for mothers of young children who require more childcare. I will discuss results on this question in Section 5.2.

Hypothesis 3: Newly eligible pensioners that stop working provide childcare and thus allow prime-aged parents to work. This effect is larger for mothers of young children.

The left side of Figure 2 depicts the **income mechanism** of the pension. The first assumption in this causal chain is that pensioners with an income increase actually transfer money to prime-aged adults, whether explicitly as a cash transfer or implicitly by paying for a larger share of household expenses. As mentioned, I cannot test this assumption with the available data. However, previous research suggests that this assumption is plausible (Duflo 2003, Ambler 2011). The effect of a resource transfer from a pensioner may affect both employed and unemployed household members. The first channel, predominantly discussed in previous studies, assumes that unemployed household members have a preference for work and that the pension leads to increased search activity (both locally and through migration) by alleviating the search resource constraint (Ardington et al. 2009). This would result in an increase in overall labour force participation. However, unemployed adults may also use additional resources to consume more free time and substitute it for job search. This could reduce job search activity and lead to a decrease in labour force participation. In addition they may feel less pressure to accept low paying jobs. This would increase the reservation wage and

in turn reduce employment.[5]

A second potential channel through which the pension can affect employment is that an increase in resources may allow currently *employed* household members to substitute free time for work. On the implicit margin this could lead to a reduction in hours worked. On the explicit margin it could induce people to quit their job and either search for new jobs or leave the labour force. If this is true, we should also expect to see a decline in employment and an increase in reservation wage. The employment prediction is unambiguous because the pension has an income effect without changing returns to work, thus not leading to a substitution effect.

Hypothesis 4: The OAP increases household income. This leads to:

i) an increase in reservation wage of prime-aged household members,

ii) a decrease in the hours worked by previously employed household members.

The effect on job search and labour force participation is ambiguous.

I will empirically test Hypothesis 4 in Section 5.3. In sum, this system of causal mechanisms identifies the necessary assumptions and intermediate outcomes that can help to disentangle competing causal chains of how the pension may affect employment. Given data availability constraints, I will test these hypotheses to shed light on which mechanism is explaining any change in overall employment we may observe.

Two additional necessary assumptions for the pension to have an impact are that households are liquidity constrained and/or not forward looking. If both these assumptions fail, we would not expect to see any changes in behaviour from gaining a pensioner since the future pension income is predictable and households would adjust prior to receiving it. The change in pension eligibility would thus not pose a change to the permanent household income (Friedman 1957). To test this hypothesis, Edmonds (2006) compares the behaviour of eligible households to that of nearly eligible. He finds that pension receiving households spend significantly more on schooling investment. This discontinuity suggests that these households either face liquidity constraints or are not forward looking. I will also test whether one observes a change in household composition *in anticipation* to having a newly eligible pensioner which would suggest that people are forward looking with respect to the pension (Section 6.1).

4 Data and Empirical Strategy

4.1 Data

I use data from the National Income Dynamics Study (NIDS), South Africa's first nationally representative panel survey. NIDS uses a stratified, two-staged cluster sample design to identify households to be included in the initial sample. In the first stage, 400 primary sampling units (PSUs) were selected from South Africa's 3000 PSUs. PSUs were selected randomly within each district council (DC) strata proportionally to the population in order to be nationally representative (Leibbrandt et al. 2009).

NIDS Wave 1 started in 2008 with a nationally representative sample of about 28,000 interviewed individuals in 7,300 households. The surveys collect data on a wide range of indicators ranging from poverty and wellbeing to migration, labour outcomes, economic activity, education and health. In 2010 and 2011 the second wave of data was collected. 79 percent of respondents were successfully

[5] Assuming that the marginal benefit of consumption from income is declining and that the reservation wage, defined as the lowest pay a person would accept, is equal to the marginal cost of work then we would expect additional resources from the pension to lead to an increase in reservation wage.

interviewed. Wave 2 includes survey weights that combine Wave 1 sampling weights and attrition weights in order to account for non-random attrition.

Each household is administered a household questionnaire as well as individual questionnaires for each resident child and adult.[6] For household members not available for interviewing but typically residing at the household, a proxy questionnaire is administered that includes questions on key indicators such as employment status. By contrast, those members that do not typically reside in the household anymore are allocated a new household ID. They are followed to their new location, where the adult questionnaire is administered to them.

The question of how to define a household is central in the context of South Africa. A circular employment migration system was set up during *apartheid* in order to provide the urban industrial areas with cheap labour while keeping the African population in the rural homelands. Labour migration remains a common phenomenon in today's South Africa: 24% of households have one or more non-resident household members (Sienart 2008).

In the NIDS data, respondents keep a unique identification number across waves. Between waves a respondent may either be interviewed in the same household, be interviewed at a different household, or attrite. For the analysis it is important to decide whether to consider the change in number of pensioners aggregated in the old or new household for cases where pensioners or prime-aged adults change households between waves. My preferred specification is to aggregate changes in the number of pensioners based on the household roster in Wave 1. This has the advantage that it captures scenarios in which a prime-aged adult moves but is still engaged in some form of resource sharing with the original household. These cases may be particularly relevant in the context of this study since previous research found that the pension finances labour migration. I will explore how sensitive results are to the choice of household aggregation in the robustness section. One limitation is that the data does not capture the change in pension status of the households that attriters join, a topic I will address in the attrition section.

The previous literature on the effect of the old age pension defines prime-aged adults to be between the age of 18 and 50.[7] It is not immediately clear why the age cap was set at this threshold since it is plausible that the pension may affect the behaviour of household members between the age of 50 and 59. I will therefore define prime-aged adults to be between 18 and 59. To make results comparable to the existing literature, estimates using the age definition used in previous studies are also provided. I will follow the literature and restrict the main analysis to African households.

A key question for the analysis is how to measure the change in number of pensioners per household as pension up-take is incomplete and likely to be endogenous as discussed in the next section. Central to constructing a measure of the change in the number of pensioners in a household is how many people become pension-eligible, i.e. cross the age-threshold. It may therefore be concerning that in many data sets, particularly from developing countries, respondents often report their age rounded to the nearest multiple of five or ten. This phenomenon referred to as '*age heaping*' could be particularly problematic in the analysis of the old age pension since the age threshold is at 60 years (and 65 in the case of men in Wave 1). If age misreporting can be assumed to follow a classical measurement error, i.e. uncorrelated to other covariates, we would expect coefficients to be biased towards zero. The problem is even more serious if the measurement error is correlated to a covariate such as education that may affect the outcome of interest. However, in the NIDS data age heaping

[6] To identify household members, respondents were firstly asked to list all individuals that have lived under this "roof" or within the same compound/homestead at least 15 days during the last 12 months or who arrived in the last 15 days and this was now their usual residence (NIDS 2008).

[7] Bertrand et al. (2003) use 16 as the lower age cutoff for prime-aged household members based on the rationale that the legal minimum working age is 15. However, as noted by Sineart (2008), most 16 and 17 year old are still in secondary school.

does not appear to be a problem in the relevant age brackets (60 to 65) (Figure 3).

As shown in Figure 4, 259 people that were pension-eligible in Wave 1 died by the time of Wave 2. There are a total of 574 people that crossed the age-eligibility threshold between Wave 1 and Wave 2. As discussed in the framework section Section 3, it is useful to divide this group into three subgroups. 100 newly eligible pensioners previously received the disability grant and thus did not experience a change in income. Of the remaining 474 newly eligible pensioners, 405 (84.5%) previously earned an income from employment below the pension amount of 1,200 Rand, with 335 (70.7%) reporting to have earned no income from employment. This group, labeled '*new low income pensioners*', would experience an income increase through the pension. The remaining 69 (15.5%) of the new pensioners previously earned an income from employment higher than 1,200 Rand before becoming age eligible.

Table 1 reports how many of the 10,161 prime-aged adults surveyed in both waves are affected by changes in the number of reported, eligible and disabled pensioners. The table shows, for example, that 453 adults have an (aggregated) loss of one reported pensioner between 2008 and 2010. In total, 1,194 (11.75%) of the prime-aged adults in the sample are in treatment group in the sense that they experienced a change in the number of *reported* pensioners in their household. The treatment group size for the change in *eligible* pensioners is 778. An important caveat for the interpretation of results from this study is that this is a relatively small group of treated individuals. If I do not find a statistically significant result, I cannot rule out that there is an actual impact but that the study did not have statistical power to detect an effect.

Before exploiting econometric models to formally estimate the effect of the number of pensioners and employment, it is useful to summarise this relationship descriptively. Table 3 compares differences in demographic characteristics and changes in the number of pensioners between people that lose a job, gain employment and experience no change in employment. These simple cross-tabulations suggest a negative correlation between the number of pensioners and employment outcomes. The average increase in the number of eligible pensioners is much higher among adults that lose employment. Furthermore, a pensioner loss is more strongly associated with a gain in employment. Among those that find employment, 4.09% report to have lost a pensioner compared to 1.64% among respondents that lose employment.

In addition, we see that there is more transitioning between jobs for people that are more educated and live in metropolitan areas. This is particularly true for those that gained employment. The share of men that lose employment is disproportionally high. By contrast the population that did not change employment status is disproportionally drawn from women and respondents that are less educated. The next section will highlight factors that could result in a spurious correlation between pension receipt and employment and discuss identification strategies to mitigate these potential sources of bias.

4.2 Identification

Estimating the effect of the old age pension on employment can be viewed as a form of programme evaluation in which the behaviour of prime-aged adults in the 'treatment group' that experience a change in the number of pensioners in the household is compared to a 'control group' of adults that do not experience a pensioner change. There are two central identification challenges in estimating the effect of the pension: (i) unobserved heterogeneity between prime-aged adults in the treatment and control groups and (ii) endogenous selection into the pension among eligible people. I will next discuss how the empirical strategy will address these two potential identification problems.

Unobserved Heterogeneity: Analyses of cross-sectional data encounter the problem of distinguishing the effect of the pension from other possibly unobservable differences between the treatment

and comparison group. Most notably, the household composition may be endogenous to the key variable of interest, since pension-receiving households may attract people with lower employment prospects.

The availability of panel data allows for a comparison of the same individuals before and after a change in the number of pensioners in the household and thus control for unobservable individual and household characteristics that may affect employment outcomes. I will use a fixed effects model to account for the possibility that unobserved individual heterogeneity is correlated with the regressors, most notable the pension status. With data from two waves available, this fixed effects analysis can be implemented through a first differencing estimation in which changes in the outcome variable are regressed on changes in other characteristics that may determine employment.

For this fixed effect analysis to provide unbiased estimates of the impact of the pension, three assumptions need to hold true. First, the model assumes that unobserved individual-specific effects are additive and time-invariant. Second, we need to assume that 'treatment' and 'control' subjects would experience the same trend in outcome without the intervention. In the context of this study, prime-aged adults in household gaining and losing eligible pensioners (treatment group) may be different from those that do not experience a change (control group) along characteristics that may affect employment outcomes over time. In fact, when comparing baseline characteristics I find that while prime-aged adults in the control group have a similar education and gender profile, approximately 29% in the control group have salaried employment at baseline compared to about 19% in the treatment group. In addition, adults in the treatment group are on average one year younger. I will employ various robustness tests to assess the plausibility of the equal-trend assumption in Section 6.3.

A third important concern with fixed effects analyses is attrition – especially in the South African context where labour migration is common. Non-random attrition could affect the validity of the results. In particular, as previous research suggested, prime-aged adults may migrate in response to a change in the number of pensioners since the grant could finance relocation and search expenses. While NIDS tries to track people that relocate, the attrition rate among these movers is higher. In the robustness section (6.2), I will explore how sensitive results are with respect to assumptions about selective attrition.

Endogenous Selection: Endogenous selection refers to a situation where a non-random subset of eligible people take-up a programme. Figure 5 shows reported pension take-up rates by age and gender. While there is a clear discontinuity in pension take-up at the age threshold for men and women, some report to receive the pension before they reach the age threshold while others do not receive the pension despite being eligible.[8] This implies that while pension eligibility is exogenous (among the vast majority of Africans that pass the means test), the actual take-up is endogenous. Poorer households may feel more pressured to apply for the grant immediately after becoming eligible. Conversely, poorer households may be located further away from the public office where they can file for the pension. To account for this endogeneity, I will follow two identification strategies:

1. I will use the change in the number of pension-eligible household members as regressor of interest. Results are to be interpreted as intent-to-treat (ITT) estimates.

2. I will use the change in the number of pension-eligible household members as an instrumental

[8]Previous research on the OAP has also found that the take-up among eligible pensioners is far from complete. Particularly surprising is the significant share of people that report to receive the pension before reaching the eligibility threshold. There is no clear explanation for this phenomenon. It is not reported that there is widespread fraud in the pension system. It seems more likely that it may partly be result of respondents' misreporting of their actual age, even though we do not see 'age heaping' at age values of 60 or 65 in the NIDS data.

variable (IV) for the change in number of household members that reported to receive the pension. Bertrand et al.'s (2003) use a similar estimation strategy in a cross-sectional analysis. They instrument the pension amount with the number of eligible pensioners. The assumptions necessary for this IV to give valid estimates is that the instrument is correlated with the endogenous variable (relevance condition) and that the instrument affects the outcome of interest only through the endogenous variable (exclusion restriction). The relevance condition is met – the F-statistic of the first stage regression is 64.8. The exclusion restriction stipulates that the change in eligible pensioners only affects employment outcomes of prime-aged household members through the pension. While this is not directly testable, this is likely to hold for newly eligible pensioners since there is no other government programme with the same threshold. However, interpreting IV estimates of pensioner loss has the caveat that this event leads to funeral expenses and may cause distress which may violate the exclusion restriction.

While both of these estimation strategies attempt to address the problem of endogenous pension up-take, the estimates have different interpretations. The ITT estimate measures the effect of the household becoming pension-eligible regardless of whether the eligible person actually receives the pension. The IV estimate is capturing the local average treatment effect ($LATE$). It measures the effect of the pension for adults in '*complier households*' that changed the reported pension status due to a change in pension-eligible (Imbens and Angrist 1994).

The distinction between LATE and ITT estimates is important, not least because it has implications for testing economic theories such as alternative models of household resource pooling. A widely reported result in this literature is that the effect of the pension depends on the pension recipients' gender (Ardington et al. 2009, Posel et al. 2006). Ardington et al. (2009, p.30) report for example that "*The gain of a female pensioner ... is associated with greater labour migration for both men and women*". This is interpreted as evidence for the theory that female pensioners pool their income for both sexes. By contrast, the study does not find that male pensioners promote labour migration of female household members. However, the analysis uses the number of *pension-eligible* household members as the regressor of interest. The result that ITT estimates are smaller in magnitude for male pensioners compared to female pensioners could well be driven by the fact that pension take-up rates are substantially lower for men.

4.3 Analysis

Next, I will discuss the variables of interest and the econometric models I will estimate. The main specification to test hypothesis 1 through 4 is:

$$\Delta y_{ih} = \alpha + \delta \Delta Pens_h + \beta \Delta X_h + \pi \Delta DisabPens_h + \gamma \Delta 57.59yr_h + e_h \quad (1)$$

The variable y_{ih} measures the outcome of adult i, who is part of household h. For the different forms of employment, variable y_{ih} is a dummy for whether the respondent is employed. The dependent variable Δy_{ih} thus takes on the values -1, 0, and 1 for job loss, no change, and job gain respectively and captures the employment change between 2008 and 2010 (from NIDS Wave 1 to 2). My preferred outcome variable is salaried employment.[9] This is arguably the most important employment outcome from a welfare perspective: previous research shows that South Africans prefer salaried jobs over casual work and self-employment partly because it tends to be more stable and provides more certainty (Kingdon and Knight 2007). In addition, looking at the effect on any form of employment may mask welfare-affecting shifts between salaried and casual jobs. I will therefore also

[9]Salaried employment asks the question: "Are you currently being paid a wage or salary to work on a regular basis for an employer (that is not yourself) whether full time or part time?"

test whether the change in pensioners leads to a transition between different types of employment (Section 5.3).

In order to test the income mechanism (Hypothesis 3), I will estimate the effect of a change in pensioner on the i) reservation wage and ii) the change in the number of hours worked among people with jobs as a measure of employment on the intensive margin. If Hypothesis 3 is true we would expect the pension coefficient in the reservation wage regression to be positive and in the hours worked regression to be negative. I will further test for the pension's effect on labour force participation for which there was no clear theoretical prediction.

$\Delta Pension$ is the key independent variable of interest. Coefficient δ estimates the overall causal effect of the old age pension on employment outcomes. I will measure change in pension through four specifications:

- $\Delta Pension_1$: Change in number of pensioners in household based on reported pension receipt.
- $\Delta Pension_2$: Change in number of eligible pensioners in household based on reported age.
- $\Delta Pension_3$: Change in number of eligible pensioners in household based on reported age adjusted for previous disability grant receipt.
- $\Delta Pension_4$: Change in number of pensioners in household based on reported pension receipt instrumented by the change in number of age-eligible household members.

Estimates from these four pension specifications have different interpretations. While $\Delta Pension_1$ likely suffers from the endogeneity problem discussed, it is the outcome used in some previous studies and will serve as a benchmark and allow assessing the degree of endogeneity. $\Delta Pension_2$ measures the change in newly eligible pensioners. This is the variable most widely used in the existing literature. $\Delta Pension_3$ includes only newly eligible pensioners that have not previously received the disability grant or held a higher paying job since only this group experiences an income increase. $\Delta Pension_4$ uses the change in age eligibility as an instrument for change in reported grant receipt to address the fact that the take-up of the pension may vary due to unobserved factors. Both the instrument and endogenous variable are adjusted to exclude disabled pensioners. $\Delta Pension_3$ and $\Delta Pension_4$ are my preferred specifications with coefficients δ_3 and δ_4 as estimates of the ITT effect and LATE, respectively.

Measuring the change of pensioners in the household in one variable assumes that the there is a linear (opposite) relationship between losing and gaining a household member. To account for the possibility that the effect of gain and loss differs, I will follow Ardington et al. (2009) and also include specifications with separate (dummy) variables for household pension gain and pension loss.

ΔX_h is a vector of changes in time-variant household characteristics such as household size and number of children. Since I only measure employment outcomes of continuous sampling members (CSM) that were interviewed in both Wave 1 and 2, I am not concerned with endogenous selection of respondents into the household, such as the selection of people with poorer employment prospects into households that gained pension status, a phenomenon observed in some pervious studies (Klasen and Woolard 2009). It is a-priori unclear whether one should control for these variables or instead estimate the reduced form. On the one hand, a change in household size could still affect CSM's outcomes since new household members with low employment prospects may increase pressure to earn income or as new household members may provide childcare. On the other hand, including ΔX_h may run the risk of the 'bad control' problem (Angrist and Pischke 2009) as these variables are themselves affected by the treatment. A similar rationale holds for other individual covariates that change over time such as education levels which have the additional problem of being measured with much noise suggesting that β may capture false transitions. In my preferred specification, I

will therefore not control for the change in prime-aged household members and children as well as other covariates. However, results are not sensitive to including these variables.

The first robustness check is to compare how results differ depending on whether the pensioner previously received a disability grant (which automatically gets turned into the pension grant once the individual reaches pension eligibility age). I will estimate this by including a variable that measures the number of new pensioners that previously received the disability grant ($\Delta DisabPens$). Since both grants pay the same amount, this group will serve as a counterfactual for a gain in newly eligible pensioners as it accounts for having older household members while holding the income constant. One caveat for this test is that only 125 prime-aged adults have a new disabled pensioner. As a second robustness test, I will therefore estimate the changes in employment for the 771 adults that gain a new 57 to 59 year old person in the household between the two survey waves ($\Delta 57.59yr_h$). Similar to the first robustness test, this serves as a placebo test as it mimics a similar age structure of newly pension-eligible households without leading to a change in income or childcare providers. If employment effects of the pension are caused by the change in income or childcare provision and not by differential employment trends due to differences in the household age-structure between treatment and control group, we would expect the coefficients π_i and γ_i to be (close to) zero (Hypothesis 2).

To test if treatment effects vary across different characteristics of prime-aged adults and geography I will further estimate model (1) separately for different subgroups. To test the childcare mechanism (Hypothesis 3), I will test if the effect varies between mothers with and without children. If Hypothesis 3 is true, we would expect the coefficient δ to be positive and larger in magnitude for women with (young) children. Other subgroup specifications will be guided by the theoretical framework. Pension effect may vary by the gender of the pensioner (as intra-household transfers may differ by sex), age and gender of prime age household member (since women may be less inclined to work or migrate if it interferes with child-bearing plans), and whether the person lives in a metropolitan area (as this would largely rule out the labour migration channel).

Heteroskedasticity robust standard errors e_h are clustered at the sampling cluster level to account for correlation in shocks among respondents that are part of the same geographic cluster. Sampling weights are included to account for non-random attrition.

5 Results

This section presents results from estimating variations the main model (1). I will first follow the literature and estimate models using ordinary least square (OLS) and two-staged least square (2SLS) regressions. To understand how effects differ among subgroups and obtain marginal effects, I will re-estimate my preferred specifications using a fixed effect logit model. Section 8.1. tests the overall employment effects (Hypothesis 1) and robustness tests (Hypothesis 2). These regressions follow the majority of the existing literature on this topic by estimating reduced form models. To shed more light on the underlying mechanisms of the adverse employment effect found, Section 5.2. and 5.3. then proceed to explicitly test the childcare (Hypothesis 3) and income hypotheses (Hypothesis 4). Section 5.4 tests for heterogeneous treatment effects.

5.1 Employment effects

First, I estimate reduced form estimates of the pensions' effect on salaried employment using ordinary least square (OLS). The dependent variable takes on three distinct outcomes: loss of employment (value: -1), gain of employment (+1) and no change (0). Results in Table 4 report ITT

(Panel A) and IV (Panel B) estimates the following variation of the main specification (1).

$$\Delta y_{ih} = \alpha + \delta \Delta Pens_h + \beta \Delta X_h + e_h \tag{2}$$

The first two columns report findings from Model (2). Column 1 shows that an increase in the number of *reported* pensioners ($\Delta Pension_1$) is associated with a negative employment effect at statistically significant levels. As previously discussed, this pension receipt measure suffers from problems of endogenous selection. Column 2 addresses the endogeneity problem by using the number of *eligible* pensioners ($\Delta Pension_2$) as the independent variable of interest. This specification also finds a negative correlation between pension change and employment. However, compared to column 1 the coefficient is closer to zero and not statistically significant, thereby confirming that the endogeneity of the take-up decision can lead to a spurious correlation between a change in pensioners and employment.

$$\Delta y_{ih} = \alpha + \delta \Delta Pens_h + \beta \Delta X_h + \pi \Delta DisabPens_h + e_h \tag{3}$$

Model (3) divides the number of eligible pensioners into those that did not previously receive the disability grant ($\Delta Pension_3$) and those that did ($\Delta DisabPen$). Column 3 shows that the coefficient for a change in pensioners becomes statistically significant. It is noteworthy that previous studies that did not adjust for disability grant receipt would have incorrectly concluded based on Model (2) that the effect of the change in eligibility is not significant.

$$\Delta y_{ih} = \alpha + \delta_1 \Delta Pen^{elig}_{Loss} + \rho_1 \Delta Pen^{inelig}_{Loss} + \delta_2 \Delta Pen^{L}_{Gain} + \rho_2 \Delta Pen^{H}_{Gain} + \beta \Delta X_h + \pi \Delta DisabPen_h + \gamma \Delta 57.59 yr_h + e_h \tag{4}$$

Column 4 and 5 report results of model (4) which estimates treatment effect separately for gaining and losing pensioners. In order to isolate the income effect of the pension, these treatment groups are further divided into people losing pension-*eligible* (ΔPen^{elig}_{Loss}) and pension-*ineligible* household members ($\Delta Pen^{inelig}_{Loss}$), and gaining *low* income (ΔPen^{L}_{Gain}) and *high* income (ΔPen^{H}_{Gain}) pension-eligible household members. As expected, coefficients on my preferred measures of losing a pensioner (ΔPen^{elig}_{Loss}) and gaining a low income pensioner (ΔPen^{L}_{Gain}) have opposite signs and are significantly different at the 1% level.

The coefficient on the loss of a pension-*ineligible* household member (ρ_1) is close to zero and statistically insignificant. I can reject that the coefficient is equal to a loss of an pension-eligble member at the 5% level. This lends support to the interpretation that the effect of a pensioner loss is working through the income loss rather than other plausible channels like psychological effects of a death in the family or funeral expenses which tend to be very high in South Africa. Another possible explanation is that adults start working after a pensioner loss because they were previously occupied caring for their parent. This explanation predicts that we observe larger employment responses for family members of deceased with previous health issues. However, when I compare the effect of pensioner loss separately for those who reported to be in good vs. poor health at the time of Wave 1, I find that the coefficients on the loss of a pensioner in good health to be larger (results not reported).

As discussed, I estimate the effect of gaining a pensioner separately based on whether the baseline income of the newly eligble person was higher than the pension amount. People with a higher income (ΔPen^{H}_{h}) are less likely to pass the pension means test and any pension receipt constitutes a smaller relative income increase if they continue working or leads to a negative income change if they stop working. In fact, 73% of these high income pensioners report that their total Wave

2 income including the pension is lower than their Wave 1 total income. If we assume that the negative employment coefficient for a change in the number of low income pensioners is driven by the income *increase*, then we would expect the opposite effect in cases where pension eligibility leads to an income *decline*. However, estimating employment effects solely using pensioners with an actual income decline risks problems of reverse causality: it is plausible that these people may decide to retire from their job after prime-aged adults in the household find a job. ΔPen_h^H therefore includes the full group of pensioners with high incomes in Wave 1. The coefficient on this variable reported in column 4 is positive and statistically significantly different from the effect of gaining a low income pensioner which provides some support for the hypothesis that pensioner income change and employment of prime-aged adults are negatively correlated.

Model (4) also implements the two placebo tests for gaining a pensioner previously discussed. The effect of gaining a pension-eligible member ($DisabPen_h$) who previously received the disability grant and the effect of gaining an person just before age eligbility ($57.59yr_h$) are positive and insignificant. This corroborates the interpretation that the negative coefficient on gaining a pensioner (ΔPen_{Gain}^L) is the result of the pension and not due to some unobservable difference in the household age structure or composition. However, the difference in coefficient between treatment and placebo coefficients not significant at conventional levels (0.12 and 0.225 respectively).

Column 5 reports estimates from a district fixed effect regression. The fixed effect model allows the intercept for each of the 52 districts to vary. This strategy helps controlling for regional employment effects and thus increases the precision of the estimation. Results show that the negative effect of gaining a pensioner increases in magnitude and becomes statistically significant at the 10% level, whereas the coefficients on the other variables remain almost unchanged.

It is noteworthy that the coefficients of losing a pensioner are almost three times as large in absolute magnitude as the coefficients on gaining a pensioner. However, these results present ITT estimates that do not take into account the actual difference in pension receipt between treatment and control groups. Differences in the effect of the pension loss relative to gain may thus be driven by a lower compliance rate among newly eligible pensioners. To account for this possibility, Panel B reports instrumental variable results of model (2) to (4) estimated with two-staged least square (2SLS). Instrumenting changes in reported pension receipt with changes in eligibility provides measuring the local average treatment effect. Estimates thus capture treatment effects for members of households in which gaining eligibility induced people to receive the pension.

Coefficients are of the same sign than the non-instrumented pension eligibility measures and t-values are very similar. However, since LATE are in effect ITT estimates scaled by the difference in pension take-up rates between treatment and control households, coefficients are larger in absolute magnitude. The IV estimate of gaining a pensioner (column 8) is more than twice as large in magnitude than the equivalent ITT estimates (column 4) whereas the IV estimate on losing a pensioner is only 30% larger than the ITT estimate from the OLS estimation. In sum, effects of losing a pensioner remain larger in magnitude than the effect of gaining a pensioner. In addition coefficients on gaining a pensioner are only marginally significantly different from the control group and the placebo groups.

Marginal Effects: The analysis above follows the literature in measuring employment outcomes in three categories: job gain (+1), job loss (-1) and no change (0). However, the interpretation of the treatment coefficient is not straightforward. In addition, it is problematic that each person can only move by one value and in one direction depending on whether they have a job at the time of the baseline. To address this problem, I next re-estimate Model (4) using a fixed effect logit model. As before, I estimate fixed effect models through first differencing. In effect, this strategy separately estimates effects of a change in the number of pensioners on the probability of *finding* a job for previously unemployed and *losing* a job for those employed at Wave 1.

Each row in Table 5 reports results from a separate fixed effect logit regression. Panel A reports the pension's effects on the probability of losing employment for the subset of 2,042 adults employed at the time of Wave 1. Conversely, Panel B reports changes in the probability of gaining employment for the 6,396 adults unemployed at Wave 1. Column 1 reports coefficients of the fixed effect logit model. While the magnitude of the coefficients do not have a straightforward interpretation, the sign and significance suggest that gaining a pensioner both increases the probability that people lose employment and reduces the chances that the unemployed find employment. The effect of losing a pensioner are of opposite sign for employed and unemployed adults, although coefficients are not significant. Column 2 reports results from column 1 transformed to relative-risk ratios which are easier to interpret. Gaining a pensioner more than doubles (2.31) the probability that employed adults lose their job and almost halves (0.575) the chance that unemployed adults find employment. Relative risk estimates for losing a pensioner are also large in magnitude (0.60 and 1.17), but these coefficients cannot be estimated as precisely because of the smaller number of lost pensioners.

Column 3 and 4 report average partial effects separately for men and women. Estimates present the change in transition probability between employment stages associated with a change in the number of pensioners. Results in Panel B show that gaining a pension-eligible household member reduces the probability that women find a job by 6.5 percentage points (p.p.) while the effect for men is also negative but insigificant. Both unemployed men and women do not gain employment after losing a pensioner, possibly a result of the limited number of vacancies in the South African labour market. Among previously *employed* adults (Panel A), both men and women are about 10 p.p. less likely to lose employment after losing a pensioner. By contrast, gaining a pensioner does not significantly affect employment of employed women, but increases the probability that men lose employment by 33 p.p..

These results should be interpreted with caution. Each panel includes information on the percentage of observations correctly specified by the logit model. By comparing fitted probabilities to actual outcomes, this measure provides information on the goodness of fit of the fixed effect logit model. Both changes in pensioners and changes in employment are relatively rare event, so an imprecise model may provide unreliable estimate. In particular, the model's goodness of fit for the group of employed adults is only about 70%. This may explain the large marginal effects found in the upper panel. The model's goodness of fit is better for the group of previously unemployed adults (87%), which lends more credibility of these marginal effects.

With this caveat in mind, the picture that emerges from this analysis is that a change in pensioners may affect male and female household members differently. There is suggestive evidence that the effect of losing a pensioner has a similar effect for women and men: employed adults are less likely to stop working, whereas unemployed adults are not more likely to find employment, which could reflect the limited number of job opportunities in the South African economy. By contrast, results suggest that gaining a pensioner has particularly large adverse employment effects for previously employed men and unemployed women.

To sum evidence on Hypothesis 1, results from all of the models point towards a negative causal relationship between the change in the number of pensioners and (salaried) employment that is robust to different econometric specifications. Predictions on the effect of placebo groups (Hypothesis 2) were also confirmed. In the next two sections I will explore evidence on the childcare (Hypothesis 3) and income mechanism (Hypothesis 4).

5.2 Employment Effects through the Childcare Mechanism

To recap the theoretical framework, the underlying assumption for the hypothesis that a gain in pensioner affects employment through the childcare constraint is that pensioners retire from their

job after becoming age-eligible. Evidence for this assumption makes it *prima facie* unlikely that a pensioner gain will significantly affect employment through the childcare mechanism: of the people who become pension eligible in Wave 2, only 27.8% had salaried employment in Wave 1. This number increases to 40.4% once one measures broad employment that includes casual and self-employment, forms of work that may allow pensioners to provide childcare. After becoming pension-eligible the employment share drops by 9.7 percentage points and 16.9 percentage points for salaried and broad employment, respectively. This relatively small change may partly be due to the institutional regulation that pension receipt is not dependent on recipients' salary as long as it is below the relatively high cutoff of R47,400.

I will test for the childcare constraint by running separate regressions for women with and without children, both estimated with OLS and logit models to obtain marginal effects of gaining (Table 6, Panel A) and losing (Panel B) a pensioner on the probability of losing employment (LOSE) and finding employment (GAIN).[10] If the pension affects employment by alleviating the childcare constraint, we would expect the effect of a change in pensioner to be larger for women with children, particularly young ones that are not yet in school.[11] Columns 1 to 4 show regression results of model (4) with the change in salaried employment as the dependent variable and the adjusted change in eligible pensioners ($\Delta Pension_3$) as the independent variable of interest.

OLS estimates from column 1 and 2 indicate that gaining a pensioner is negatively correlated with employment outcomes for mothers whereas losing a pensioner is positively correlated with employment for women without children. Tests of equal coefficients for gaining and losing a pension across regressions for mothers and non-mothers can be rejected at marginal significance levels (p-values of 0.16 and 0.17 for gain and loss respectively). Looking at the marginal effects of gaining a pensioner (mfx, Panel A) we find that this difference is driven by differential effects among *employed* women: gaining a pensioner reduces the probability of losing work for women without children, but increases this probability for women with children. The effect of gaining a pensioner is similar for *unemployed* mothers and non-mothers. Panel B shows that both employed and unemployed women without children are *more* responsive to a change in pensioners than mothers, although coefficients are not significantly different.

Column 3 and 4 divide mothers living with biological children according to whether their youngest child is between 1 and 7 or 8 and 17 at the time of Wave 2 to account for the fact that childcare is more important for children that are not yet in school. While I cannot reject that coefficients of gaining and losing a pensioner are equal for mothers with young and older children (p-values: 0.75 and 0.66 respectively), results suggest that mothers with older children are, if at all, *more* responsive to pensioner changes.

As a last test of the childcare hypothesis, I construct an indicator variable for prime-aged respondents that claim that childcare was a constraint to working in Wave 2 but not in Wave 1. I use this indicator as the dependent variable in a probit fixed effects regression with indicator dummies for gaining and losing pensioners as the main independent variables of interest (Model 5). If the childcare constraint hypothesis was true we would expect the coefficient on the pensioner gain variable to be negative, while the coefficient for losing a pensioner would be positive. Instead column 5 shows that only the coefficients for gaining a pensioner is positive, (statistically insignificant).

In sum, Hypothesis 3 is clearly refuted by the data. If new pensioners would take care of children

[10] I focus on women because they are traditionally in charge of raising children in my sample of African households. This is reflected by the fact that of the 778 respondents that say that childcare is a constraint to work, 97.4% are women. To categorize women I used the NIDS question on whether any biological children under the age of 18 are living in the same household.

[11] This analysis defines women with children as those that have given birth to a child that is currently living with them. Estimates are almost identical when I extend the definition of mothers to include women with children not living in the same household.

and thus allow mothers to work or, conversely, the loss of a pension would require mothers to quit their job to take care of the children, we would expect the exact opposite results from what we observe. Instead, it seems more plausible that pension resources allow mothers not to work and provide childcare themselves. The adverse employment effects of the pension can thus not be explained through the childcare mechanism. Next, I will test Hypothesis 4 on the effect of the pension through the income mechanism.

5.3 Employment Effects through the Income Mechanism

Assuming that some of the grant money gets transferred within the household to prime-aged adults, Hypothesis 3 states that the pension will i) increase the reservation wage and ii) decrease the number of hours that people with employment work. The overall effect on labour force participation and the level of search activity is ambiguous.

The theoretical framework suggests that prime-aged adults use pension resources to substitute free time for either job search or actual work. As a result, the share of people in the labour force defined as the economically active population that is either employed or actively searching for work should decline. Conversely, the pension may finance job search of the previously economically inactive unemployed and thus increase labour force participation. Results from OLS and IV estimations reported in columns 1 to 3 in Table 7 show that the share of people in the labour force is actually negatively correlated with the change in the number of pensioners, although estimates are not statistically significant. This result is driven by both the previously reported negative effect of the pension on employment (significant) as well as a negative effect of the probability that the unemployed have a desire to work (not significant, estimates not reported).

Results reported in Column 4 to 6 provide some evidence in favour of Hypothesis 4i). The change in the number of pensioners is positively correlated with the change in reservation wage among people with any form of employment in both waves.[12] To reduce the noisiness in the data, I winsorize reservation wage data (at the 1% level). The effect is almost symmetrical: gaining a pensioner leads to a 252 Rand per month increase in reservation wage while the loss of a pensioner decreases the reservation wage by 279 Rand. The local average treatment effect is 389 Rand (p-value: 0.15). While these effects are at about 10% of the baseline mean value economically meaningful they are not significant at conventional levels, arguably because of the relatively small subset of the sample that provided information on reservation wages in both waves.

Results reported in Column 7 to 9 provide suggestive evidence in favour of Hypothesis 4ii) that the pension leads to a decrease in employment on the intensive margin. The dependent variable is the difference in hours worked in a typical week (winsorized at the 1% level) for those that have any form of employment in both waves. Results show that among adults with work in both waves, those that lose a pensioner increase their weekly time worked on average by 0.87 hours. By contrast, those that gained a pensioner reduce it by 3.9 hours, about 13% of the baseline number of hours worked (column 8). This difference may be explained by the well-established fact that it is easier for workers to adjust hours worked downward than upward (Card, Chetty and Weber 2007). However, these estimates are not statistically significant, possibly because the reported hours worked is a noisy measure with large variation.

In sum, I find suggestive evidence in favour of Hypothesis 4: a positive change in the number of

[12]One data constraint for the analysis of the reservation wage is that the question differs between waves. In NIDS wave 1 only unemployed people willing to start work in the next four weeks are asked what they think a "reasonable take-home monthly wage for you, given your age, education, and skills" would be. By contrast, NIDS wave 2 asks all adults that the lowest monthly wage would be that they would accept if they were to become unemployed. As a result I can only compute a proxy for the change in reservation wage and I can only do it for the subset of 1549 searching unemployed at the time of wave 1.

eligible pensioners is associated with an increase in the reservation wage and a decline in the number of hours worked. I further find that the pension has a negative effect on labour force participation. While all estimates have the sign predicted by the theoretical framework, data and sample size limitations preclude a definite answer. Data limitations also prohibit estimating the effect of the pension on search activities of unemployed adults. Only 270 adults reported how much money they spend on job search and this measure is very noisy. Once additional waves of NIDS is available, the analysis should also be extended to look at the salary and employment spell as these are considered proxies for job match quality. This would allow testing whether pension resources enable people to search for jobs for which they are better qualified (Chetty 2006, Card et al. 2007).

Forms of Employment: The observed substitution of free time for salaried work or job search can also result in a shift in types of employment. Pension resources may induce people to switch from salaried employment to casual work or being self-employed as these forms of employment provide more flexibility to reduce working hours. If we observed an increase in these alternative employment outcomes, the potential adverse employment effect of the pension found in Section 5.1 would be diminished. Table 9 reports results from OLS and IV regressions on salaried employment, self-employment and casual work as well as a broad employment measure that indicates having any form of work.

Column 2 reports previous results (model (4)) on salaried employment, estimated with OLS and 2SLS. Column 3 finds that a change in the number of pensioners is negatively correlated with the probability of being self-employed. Similar to salaried employment, this relationship is mainly driven by the effect of losing a pensioner. Interestingly, the ratio of the effect of gaining and losing a pensioner is almost identical to estimates on salaried employment. The fact that people do not seem to use pension resources to start their own business suggests that, in the context of South Africa, the relatively low rates of entrepreneurship are not the result of credit constrained. This finding stands in contrast to evidence from other (African) countries where cash grants led to the creation of businesses (Blattman, Fiala and Martinez 2013). Results in column 4 imply that casual work is not affected by a change in the number of pensioners. In sum, there is no evidence that pension resources induce people to transition from salaried to other forms of employment. Instead, we also see a reduction in self-employment although this effect is smaller in magnitude than for salaried employment. Overall, the negative correlation between pension and employment also holds if we measure employment as any type of work (Column 1).

In conclusion, the previous result sections tested the hypotheses identified in Section 3. I find that the pension receipt has adverse employment effects. This relationship is mainly driven by a positive employment effect of losing a pensioner. Testing different causal mechanisms, I find that this effect is not operating by affecting the childcare constraint. Instead, there is evidence that it is driven through the income mechanism. Getting pension resources increases prime-aged adults' reservation wage and reduces the hours they are working and their overall labour force participation.

5.4 Subgroup Analysis

This section estimates heterogeneous treatment effects by running separate OLS regressions (Model 4) for different subgroups of prime-aged adults. The limitation of this type of analysis is that the identification assumptions discussed in Section 6, in particular the equal trend assumption, are less likely to hold with smaller subgroups. With this caveat in mind, Table 8 reports which subgroups are particularly affected by the pension. Testing the robustness of these results and exploring why pension effects vary between subgroups is subject of future research.

Gender: Panel A reports how effects differ by gender. As discussed in the previous sections, men are more responsive to changes in pensioners which is driven by a larger employment response

to losing a pensioner. This may partly be explained by the fact that more men are in salaried employment at the time of Wave 1 (33.3% vs. 19.4%). One important caveat for the analysis of gender effects is that we do not observe in the data how much of the pension gets transferred to each adult. It may well be that men and women have the same labour supply elasticity, but that men receive a larger share of the grant. One could explore this question by testing whether estimates are the same in households in which all prime-aged adults are of the same sex. Also, one could test for whether effects are larger if the pensioner is of the same sex as the prime-aged adult as this may result in larger resource transfers. Panel B confirms results from 5.2 showing that employment responds stronger to pension changes for women with children.

Age: Panel C reports how treatment effects vary by age. I divide prime-aged adults into four age brackets and estimate pension effects in separate regressions. OLS coefficients in the top row suggest that the negative relationship between the change in pensioners and employment outcomes is weak and statistically insignificant for adults aged 30 to 39. Results reported in the next two rows suggest that the young (18-29) and old (50-59) adults are more responsive to gaining a pensioner whereas middle-aged adults seem more likely to increase labour supply after losing a pensioner. In the robustness section, I will show evidence against the explanation that the negative employment effect for young people is due to increased schooling. Interestingly, the coefficient on gaining a pensioner is positive (albeit insignificant) for this middle-age group.

Income: Panel D documents how effects vary by per-capita income quintile. Results show that the correlation between employment and the pension is only significant for adults in income quintile 2 and 3 (which range from about R240-420 and R420-750, respectively). One theory discussed in the literature that may explain this pattern is *income targeting* which predicts that people reduce labour supply once they reach a certain income level regardless of the marginal returns to work (Camerer et al. 1997). In the context of the pension this could mean that people above a certain income level do not respond to a change in pension resources. Likewise, very poor people may be below their income target even after gaining a pensioner which could explain why we do not observe an employment reduction for this group. However, this explanation remains speculative and cannot explain, for example, why the coefficient of losing a pensioner is small for people in quintile 1.

6 Test of Identification Assumptions and Robustness

This section explores how sensitive results are with regard to the identification assumptions discussed in section 4.2 such as endogenous household formation, non-random attrition, and the equal trend assumption. I find that the main results are robust to various tests of these assumptions. I will then proceed to test how sensitive results are to the choice of defining households and prime-aged adults.

6.1 Household Composition

Previous research shows that the pension affects the household composition which could potentially bias the results. In particular, receiving the old age pension increases the probability of attracting unemployed people to the household (Klasen and Woolard 2000, Ardington et al 2009). Edmonds, Mammen and Miller (2005) estimate that household sizes increase by about 10% after receiving the pension. Cross sectional analyses may thus exaggerate the negative labour supply effect of the pension.

Panel data allows me to address this problem by restricting the analysis to prime-aged adults that were already present at the time of Wave 1. I thus sidestep the potential problem that adults with

worse employment prospects may join the household in response to a new pensioner. As discussed, these additional household members may still affect employment of adults in my sample. While this may be regarded as an indirect effect of the pension, controlling for the effect for a change in the number of children and other adults residing in the same household does not change results qualitatively. One potential problem that I cannot address with this strategy is that hard-to-employ adults may join households that will have a newly eligible pension in the near future. While previous research has not found evidence for these kind of 'anticipation effects' with regard to the pension receipt, it is still a possible confounder.

NIDS collects data on temporary survey members (TSM) who joined the household between the first and second wave. This allows me to directly test for these anticipatory endogenous household formation effects. Figure 6 shows the average number of new adult and child household members for each age bracket of continuous survey members. A few results stand out. First, there is a general increase in both new children and adults among newly eligible pensioners. Secondly, the highest increase is recorded for the 62 year olds which may be explained by the fact that new children and adults had time relocate into the pension-receiving household. Third, there is no evidence for anticipation effects: the figure shows that there is no discontinuity in the number of new children or adults for people at age 58 and 59 who will soon be eligible to receive the pension.

A concern related to endogenous household formation prior to a change in pensioner status is that households likely anticipate the change in eligibility, particularly with new pensioners. If they are not liquidity constrained, they may change behaviour *before* the actual pension receipt. As discussed above, there is little evidence in support of people changing their behaviour in anticipation to receiving the grant. In any case, such behaviour changes would bias any effects I may find towards zero.

6.2 Selective Attrition

As previously discussed, while non-random attrition is an important problem in panel data analysis in general, it is particularly concerning in analysing effects of South Africa's old age pension. Maitra and Inder (2004) and Ardington et al. (2009) find that the pension increases the probability that household members migrate. To assess the extent of selective attrition in the NIDS data it is useful to divide attriters into two categories. In some cases the entire household attrites whereas in other cases only certain individuals leave the household (and thus a proxy questionnaire was not administered). This distinction is useful because for the latter group of attriters we can actually measure the change in the number of eligible pensioners in the original household and thus test if attrition rates differ along this 'treatment indicator'.

As a first pass test for selective attrition, Table 10 shows results of probit regressions with a dummy for attrition of prime-aged adults (excluding cases of death) as the dependent variable. Results in column 1 confirm that attrition is clearly non-random: younger, less educated, non-African men with employment are more likely to attrite. Outes-Leon and Dercon (2008) point out that non-random attrition does not necessarily lead to biased inference. Whether it does depends on the particular model one is estimating. In my model attrition is most concerning if attrition rates are related to the treatment status, which is determined by the change in the number of eligible pensioners.

Regression results in column 2 show that the coefficient on the change in number of eligible pensioners ($\Delta Pension_3$) is not a significant determinant of attrition. However, column 3 shows that attrition rates are lower for households with a pensioner change, especially those that lose a pensioner. Column 4 estimates results with a range of covariates. It is noteworthy that the coefficient on the pensioner gain dummy is negative or very close to zero once I control for covariates. This pro-

vides evidence in support of internal validity of our results and against the concern that a spurious negative correlation between pensioners and employment resulted from excluding attrited labour migrants from the sample.

There are two caveats to the conclusion that attrition does not affect the validity of my analysis. First, selective attrition on observables may affect the external validity of results. Since younger, less educated people attrite disproportionally from both treatment and control groups, this may limit the generalisability of the estimates. Second, while coefficients on the treatment variables in the attrition regression are statistically insignificant, this may be due to the fact that there is a relatively small group of attriters for which we know the change in pensioners. Next, I will discuss different strategies to test how robust results are with respect to attrition.

First, differential attrition can be addressed by including (inverse probability) weights which re-weight the sample to better resemble the initial population.[13] Sampling weights were included in all regressions in Section 5. The share of variation in attrition explained in the probit regressions used to compute sampling weights can serve as an indicator for how well observable covariates can address attrition.[14] However, computing attrition weights in this way does not necessarily solve the problem since they are based solely on observable characteristics.

A second strategy is to restrict the analyses to subsets of the sample. First, I restrict the sample to households that did not have attrition of any household member except for cases of attrition through death. This may cause bias if excluded households are systematically different between treatment and control groups. However, the share of households without attrition is relatively similar between pension gaining (74.3%), no change (77.3%) households and pensioner losing (82.1%) households. Table 11 shows that results are robust to limiting the sample to households that did not experience any attrition both in the OLS (column 1 and 2) and IV regressions (column 3 and 4). I cannot reject that coefficients of the change in eligible pensioners are equal across column 1 and 2 (p-value: 0.49).

A third strategy to test the effect of attrition is to estimate how the effect varies between metropolitan and non-metropolitan areas. The rationale for this test is that while attrition is higher in metropolitan areas (28% vs. 23%), it is unlikely that respondents attrite to search for work since they already reside in areas where most job opportunities are located. Columns 5 and 6 in Table 11 show that the coefficient for non-metropolitan areas is robust to excluding households with attrition. What is more, the employment effect of a change in pensioners is *larger* in metropolitan areas. These estimates corroborate the earlier conclusion that results are unlikely to be driven by selective attrition due to labour migration.

6.3 Equal Trend Assumption

As discussed before, the validity of the fixed effects analysis depends on the assumption that the treatment and control group would have experienced the same trend in outcome without the intervention. In the context of this study this means that adults living in households with a change in the number of pensioners would have had the same change in employment as adults living in households without a pensioner change.

To test for the equal trend assumption, section Section 5 looked at the effect of two placebo treat-

[13] Computing inverse probability weights includes estimating two probit regressions with an attrition dummy as the dependent variable. One regression includes variables that are significantly associated with attrition, the second does not. The ratio of predicted probabilities from these regressions are used to reweight the sample (Baluch and Quisumbing 2010).

[14] NIDS provides survey weights but no information on the share of variation explained of the covariates used to compute weights.

ments to disentangle the effect of crossing the pension age threshold from the income effect of the pension. To recap, I found that the coefficient on the change in disabled pensioners is positive and insignificant whereas the effect of gaining a non-disabled pensioner is negative. However, one may argue that adults living with disability grant recipients differ from other adults in ways that affect changes in employment over time. To address this concern, I run a second placebo test in which I estimate the effect of a change in the number of 57 to 59 year olds on employment outcomes of adults between the age of 18 and 56. The effect of an increase in people just below the eligibility cutoff is very close to zero and not significant. This supports the claim that gaining a person just above the age threshold would not affect employment outcomes of prime-aged adults where it not for the effect of the pension.

Table 12 compares baseline characteristics of the treatment groups of pensioner gain and pensioner loss, control group, and the two placebo groups. Descriptive statistics confirm that the characteristics of adults in the control group ('No change') differs from the treatment groups ('gain' and 'loss) which raises doubts about the equal trend assumption. In particular, the control group has a higher level of baseline employment and is more likely to live in a metropolis. However, people with a new almost pensioner are very similar in observable characteristics to the group with a newly eligible pensioner. For the almost pensioner we estimated a precise zero effect. This provides some evidence in favour of the assumption that employment of adults in the treatment group would have behaved similarly to the control were it not for the change in the number of pensioners. However, the equal trend assumption is fundamentally untestable which is an important caveat when interpreting the results of this study.

As a last test of the equal-trend assumption, I redo the analysis for the subset of prime-aged adults that live in households with at least one person older than 50 years. Similar to the strategy of previous research (Bertrand et al. 2003, Posel et al. 2006) that restrict the sample to multi-generational households, this ensures that the age-structure between treatment and control households are more comparable. Results (not reported) show that estimates are robust to this specification. The coefficient on the change in pensioners (Model 4) drops in magnitude from -0.059 to -0.046 but remains statistically significant. This change is only driven by the coefficient of losing a pensioner that drops from 0.103 to 0.087 (results not reported).

Definition of prime-aged adults: One addition factor that could affect the equal trend assumption is that there may be more people near the retirement age in households that experience a change in the number of pensioners. As a result, a larger share of people in the treatment group would stop working even without the change in pension status. To control for this possibility, I re-do the analysis defining prime-aged adults between the age of 18 and 50. This has the additional advantage that these results are more comparable to the existing literature on the old age pension since most previous studies defined prime-aged adults through this age bracket. Results (not reported) shows that previous estimates are very robust to this specification: the coefficient of the change in the number of pensioners drops from -0.059 to -0.058 and remains statistically significant.

One may also be concerned that defining prime-aged adults to be between 18 and 59 at the time of Wave 2 means including youths that were as young as age 16 at the time of Wave 1. This may confound our results since we would count youths that are staying longer in school as not being employed. While the question of the pension's effect on schooling warrants a separate investigation that goes beyond the scope of this study, we can test whether results are robust to this possibility by re-estimating the analyses with prime-aged adults defined to be older than 22. I find that results are robust to this specification – the pension change coefficients in model 4 and 5 change by 0.001 or less (results not reported).

Choice of HH aggregation: The previous analysis was based on an aggregation of pensioner changes based on the original household roster to reflect the possibility that pension resources

may be used to finance labour migration. By contrast, aggregating changes in pensioners at the household roster collected in Wave 2 has the potential advantage that it reflects that pensioners may leave the household and thus stopped supporting members of the household they were part of in Wave 1. It may further account for cases of reverse migration where the pension induces prime-aged adults to move back to the household they were part of before Wave 1. When I redo the empirical analysis with this alternative aggregation of household pensioner changes, the coefficient on gaining a pensioner changes from -0.036 to -0.067 presenting an even stronger negative correlation whereas the coefficient on losing a pensioner remains almost unchanged (results not reported).

7 Concluding Remarks

Using the first nationally representative panel data for South Africa, I estimate the effect of a change in the number of pensioners on employment of prime-aged adults residing in the same household and test potential causal mechanisms. Overall, I find evidence suggesting that the availability of pension resources reduces salaried and self-employment of other household members. A preliminary analysis of within household effects suggests that it is employed men and unemployed mothers who reduce their labour supply most strongly in response to a change in household pension income. The data does not allow us to distinguish whether this is because these groups have a larger labour supply elasticity or receive a larger share of the pension than other household members.

In testing mechanisms, the analysis fails to find support for the hypothesis that pensioners affect employment by providing childcare in their household. Employed women with children are less likely to lose their job after a pensioner dies and unemployed mothers are less likely to find work after their household gains a newly eligible pensioner. This result is unsurprising in the South African context. Given high unemployment, low labour force participation rates, and the prevalence of multi-generational households, it seems unlikely that people do not work due to a lack of adults available for child-minding. Instead, the pension seems to affect employment through an increase in household income. In line with theoretical predictions, an increase in pension resources reduces labour force participation, increases the reservation wage of unemployed adults, and reduces the hours that employed people work. However, data size limitations preclude conclusive support for this mechanism.

The effect of losing a pensioner is larger in magnitude than of gaining a pensioner, even once we control for incomplete take-up among newly eligible pensioners through the IV estimation. There are a number of possible explanations for this result. First, the death of a pensioner is more likely to be an unexpected income shock than the gain of a newly eligible pensioner to which households may adjust in advance. In fact, I find that the effect of losing a pensioner who reported being in good health at the time of Wave 1 is about 50% larger than the effect of losing a pensioner who was in poor health (results not reported). This suggests that unexpected income shocks have a larger effect. Second, the death of a pensioner may in fact be a larger income shock than just the loss of the pension amount since funerals are very costly in South Africa. However, the fact that the death of non-eligible household members has no employment effect provides evidence against this hypothesis.[15] Third, a negative income shock may lead to larger adjustments than a gain in income since the household may have essential running expenses that were paid for by the pension.

As discussed in the literature review, previous evidence on the labour supply effects of the pension is mixed. My findings contrast with those of Ardington et al. (2009) who find a positive employment effect largely due to the pension financing labour migration. However, while I analyse nationally

[15] One of the reasons why we may not see an employment effect in response to the substantial costs of a funeral is that funeral insurance is very common in South African.

representative data, Ardington et al. use a sample from a rural district in KwaZulu-Natal, an area that has traditionally provided labour migrants. It may be that in these regions pension resources are indeed used to alleviate credit constraints and finance labour migration, especially since job search is more costly for people living in rural areas. While I find that employment effects are larger in metropolitan areas, a more careful analysis on a subset of the NIDS data from similar rural areas could shed light on the comparability of these studies. My findings support previous results from Sienaert (2008) and Ranchhod (2009), although the (ITT) estimates are smaller in magnitude. For example, I estimate that losing a pension-eligible household member makes adults in that household 7.9 p.p. less likely to lose their job, and increases the chance of unemployed adults finding a job by 1.2 p.p.. In comparison, Ranchhod (2009) reports that the probability of finding or maintaining a job after such an event increases by 8 to 10 p.p..

Posel et al. (2006, p.8) argue that the negative employment effect found by Bertrand et al. (2003) could be used as a general critique of social assistance programmes as "*fostering a culture of dependency*". This interpretation seems somewhat narrow from a economic welfare perspective. For example, pension resources that allow mothers *not* to work and instead provide childcare themselves can be interpreted as a positive effect of the pension. The premise that people know best how to spend resources to maximise their welfare is a central argument in favour of *unconditional* cash transfer programmes. Conditionalities set by policymakers not only entail monitoring and enforcement costs but also limit the choice set of recipients. In addition, it is not obvious how the pension could be designed to avoid affecting the labour supply decisions of other adult household members since resources within the household are likely to be fungible.

Overall, it is unclear if we can make any inferences about the impact of the observed employment effects on individual and household welfare as the pension does not alter the returns to work and thus should not distort labour supply decisions. Adverse employment effects may thus simply reflect increase in leisure consumption due to an income increase. However, the pension may reduce long-term welfare if prime-aged adults are unaware of the labour market benefits of additional work experience. Alternatively, if people have time-inconsistent preferences, pension resources may induce them to procrastinate in the job search (O'Donoghue and Rabin 1999). Reasons for *why* adults reduce their labour supply in response to the receipt of pension resources should be explored in future research. A comprehensive welfare analysis also needs to quantify efficiency costs from raising the tax revenues to finance the pension system.

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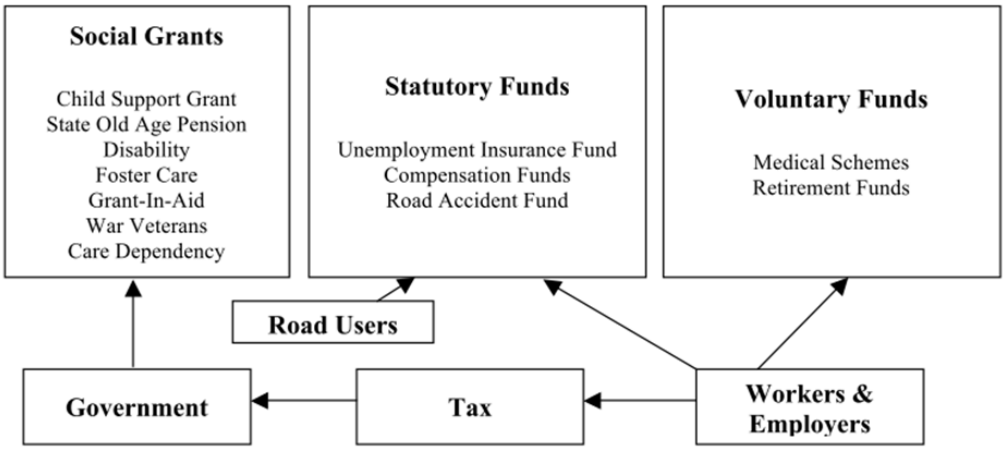
Appendix

Figure 1: Social Security System in South Africa



Source: Woolard et al. (2010)

Figure 2: Causal Mechanisms

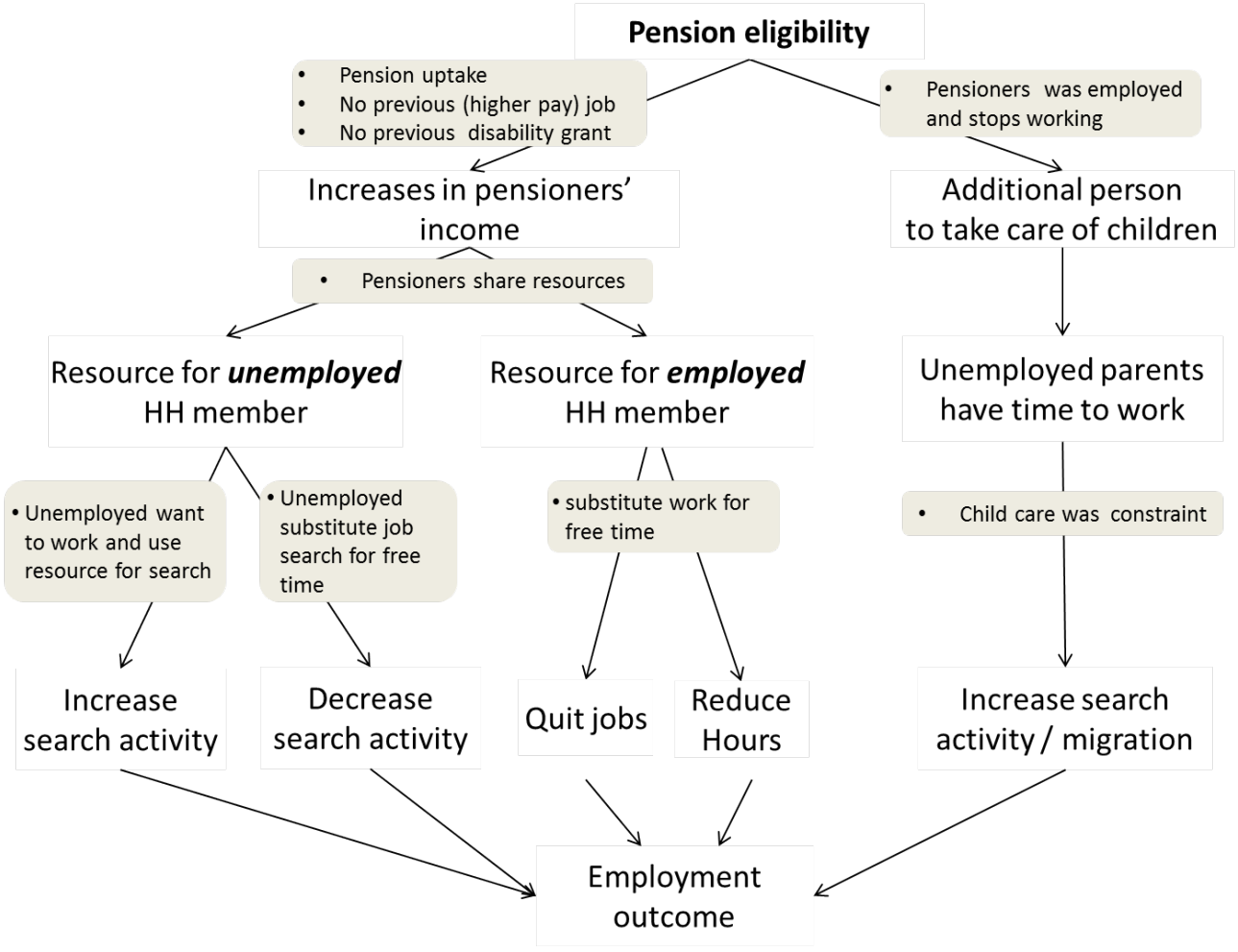


Figure 3: Age Distribution of Respondents

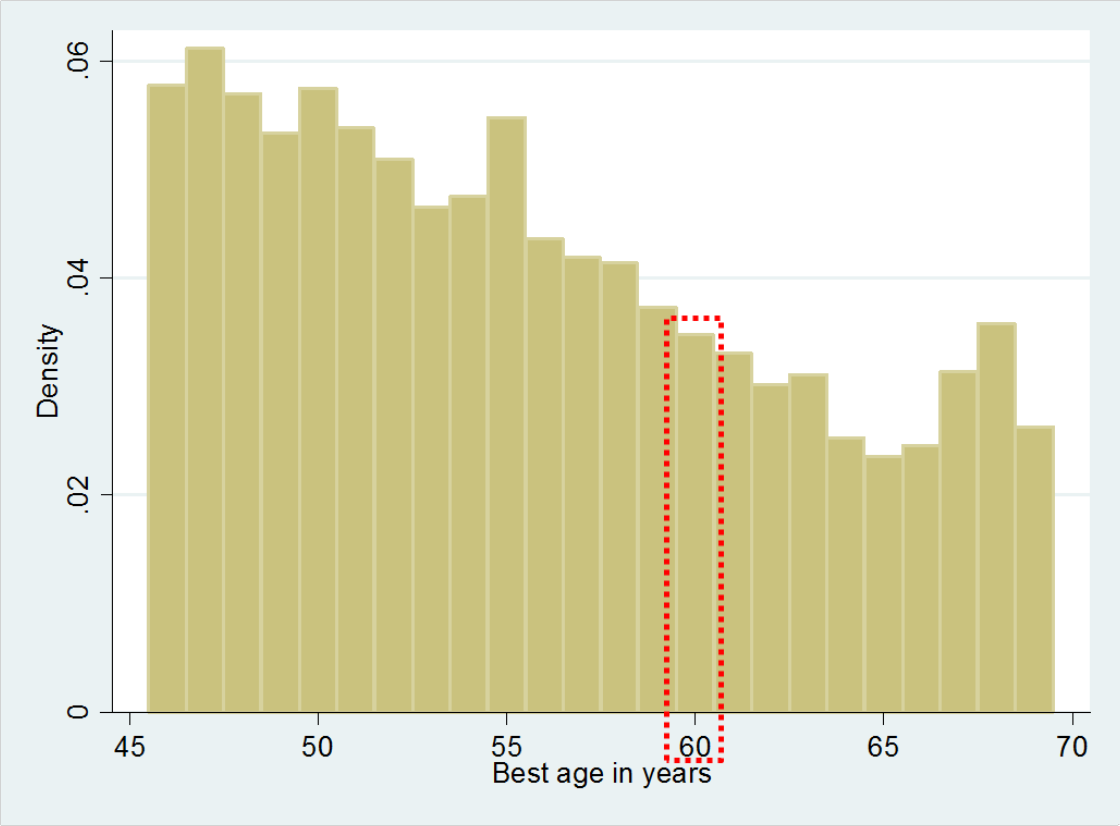


Figure 4: Treatment, Control and Placebo Groups

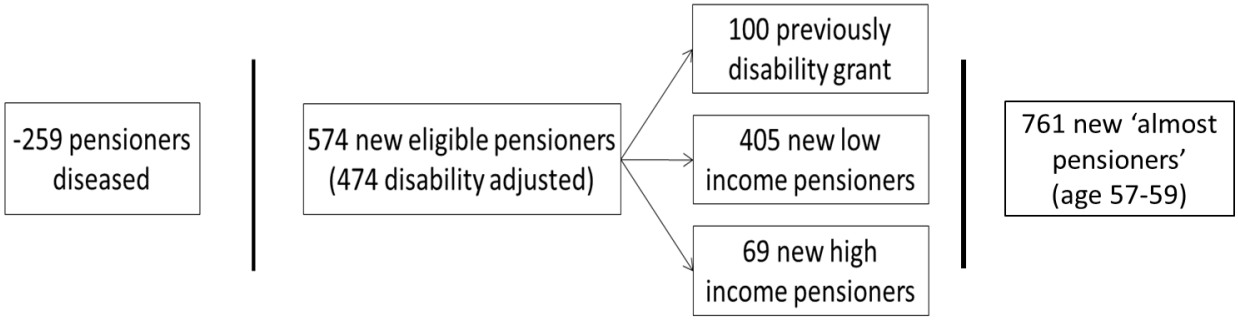


Figure 5: Pension Take-up

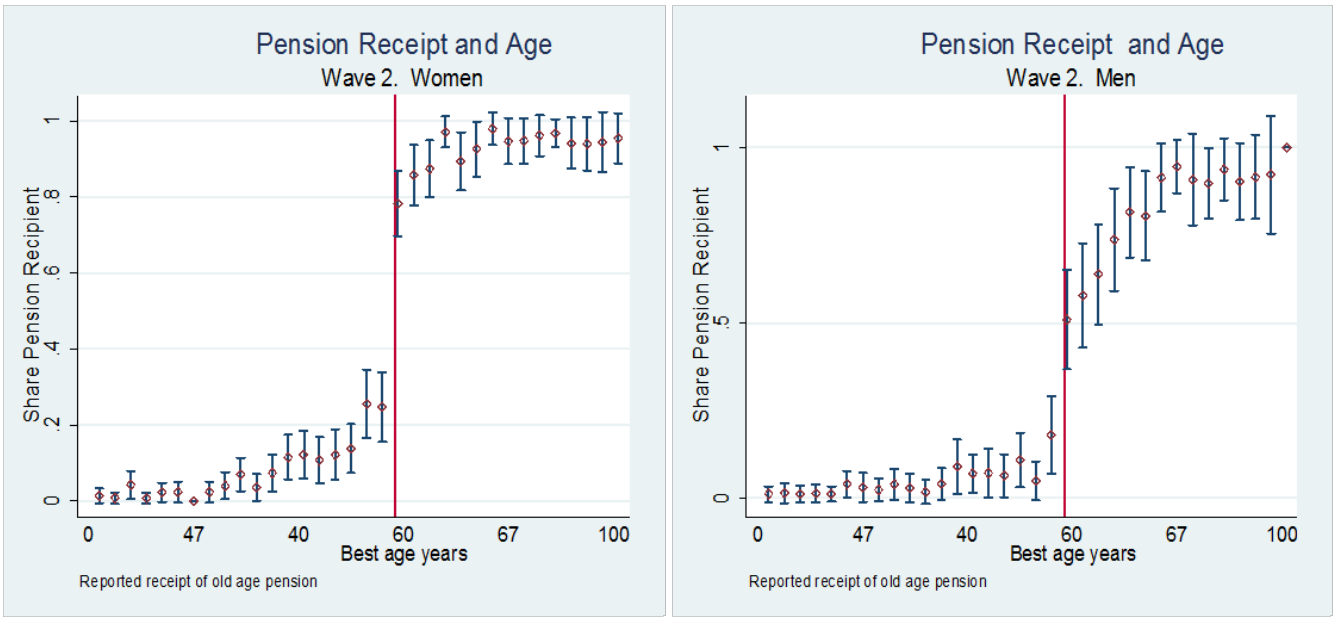


Figure 6: Household Composition Effect

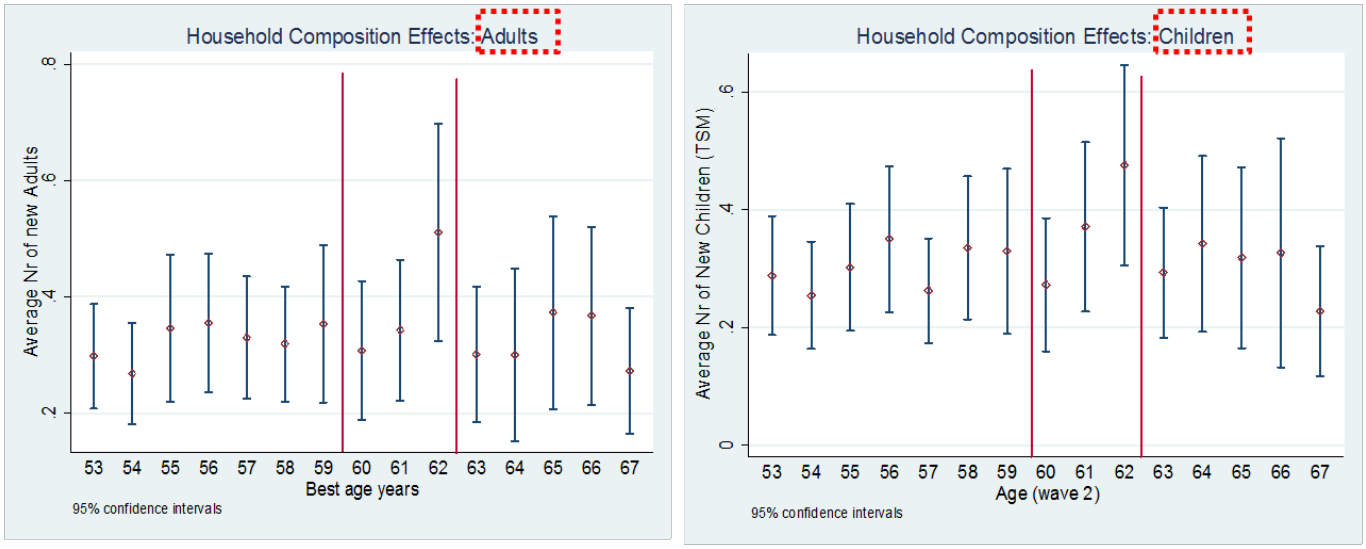


Table 1: Number of Prime-aged Adults (18-59) with Change in Pensioners

△ reported (adjusted)			△ eligible (adjusted)			△ disab. pensioners		
△ pens.	# prime	Percent	△ pens.	# prime	Percent	△ pens.	# prime	Percent
-3	4	0.04%						
-2	21	0.21%	-2	5	0.05%			
-1	453	4.46%	-1	301	2.96%			
0	8,967	88.25%	0	9,383	92.34%	0	10,036	98.77%
1	651	6.41%	1	441	4.34%	1	115	1.13%
2	63	0.62%	2	29	0.29%	2	10	0.10%
3	2	0.02%	3	2	0.02%			
	10,161	100%		10,161	100%		10,161	100%

Note: △ pens. only reflect the change in eligible and reported pensioners that did not previously receive the . disability grant. # prime measures the number of prime-aged adults affected by the change in pensioners. The sample is restricted to Africans and Coloureds between age 18 and 59.

Table 3: Characteristics of Prime-aged Adults by Employment Change

		△ Pensioners (elig)						
△ Employment	N	Change	Gain	Lose	Educ.	Age	Male	Metro
Loss	785	0.039	5.16%	1.63%	8.48	35.05	48.7%	12.8%
No Change	8,303	0.022	5.31%	3.39%	8.22	32.02	40.2%	12.3%
Gain	1,049	0.009	4.57%	4.09%	9.15	30.79	42.9%	15.9%

Note: △ pens. only reflect the change in eligible and reported pensioners that did not previously receive the disability grant. 'Change' measurs the average change in the number of pensioners between waves.

Table 4: Effect of the Old Age Pension on Salaried Employment, (OLS and IV)

		Panel A: *Pension Eligibility*				Panel B: *Instrumental Variable*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δ pensioners (reported)	-0.039***							
	[0.015]							
Δ pensioners (eligible)		-0.027				-0.051		
		[0.024]				[0.039]		
Δ pensioners (eligible), adj.			-0.048**				-0.087**	
			[0.022]				[0.038]	
HH lost pensioner (eligible)				0.105***	0.104***			0.133***
				[0.037]	[0.039]			[0.050]
HH lost pensioner (ineligible)				0.019	0.023			0.016
				[0.021]	[0.020]			[0.020]
HH gained pens. (low inc)				-0.041	-0.046*			-0.084
				[0.029]	[0.028]			[0.062]
HH gained pens. (high inc)				0.053	0.056			0.059
				[0.062]	[0.051]			[0.052]
HH gained 'disabililty' pens.				0.092	0.092			0.088
				[0.081]	[0.076]			[0.084]
HH gained 57-59 year olds				0.004	0.009			-0.010
				[0.021]	[0.021]			[0.019]
Disability Adjustment	N	N	Y	Y	Y	N	Y	Y
Age Placebo	N	N	N	Y	Y	N	N	Y
District Dummies	N	N	N	N	Y	N	N	N
p-value:Pen^{elig}_{Loss}=Pen^{L}_{Gain}				0.003	0.003			0.005
p-value:Pen^{elig}_{Loss}=Pen^{inelig}_{Loss}				0.039	0.060			0.029
p-value:Pen^{L}_{Gain}=$Disab$				0.120	0.085			0.087
p-value:Pen^{L}_{Gain}=$57.59yr$				0.225	0.140			0.328
R-squared	0.001	0.001	0.001	0.002	0.013	0.001	0.001	0.002
N	8638	8638	8638	8258	8248	8638	8638	8258

Notes: T-values in parentheses. Significance levels: *0.10, **0.05, ***0.01.

Regressions are estimated using OLS. Standard errors are clustered at the household level and sampling weights

Instrumental Variable regressions are estimated using 2 Staged Least Square (2SLS). The F statistic for the first stage is 68.4.

The sample is restricted to Africans between the age of 18 and 59 at Wave 2. District fixed effect model (7) includes dummies for South Africa's 52 districts.

Table 5: Fixed Effect Logit: Effect of the Old Age Pension on Salaried Employment.

	y=△ salaried employment			
	(1)	(2)	(3)	(4)
	△ elig	relative risk	mfx (wom)	mfx (men)
Panel A: y=LOSE EMPLOYMENT (-1)				
Δ Eligible Pensioners, adj	0.772***	2.16**	0.042	0.239***
	[2.65]	[2.65]	[0.41]	[4.09]
HH gained eligible pensioner, adj.	0.83**	2.31**	0.042	0.33**
	[2.37]	[2.37]	[0.40]	[2.76]
HH lost eligible pensioner, adj.	-0.507	0.60	-0.092	-0.107
	[-0.80]	[-0.80]	[-0.71]	[-1.03]
N	2042	2042	955	1087
% correctly specified by model	70.37%		69.01%	71.85%
Panel B: y=GAIN EMPLOYMENT (1)				
Δ Eligible Pensioners, adj	-0.335**	0.715**	-0.049**	-0.045
	[-2.25]	[-2.25]	[-2.41]	[-0.99]
HH gained eligible pensioner, adj.	-0.552**	0.575**	-0.065***	-0.047
	[-2.16]	[-2.16]	[-3.04]	[-1.01]
HH lost eligible pensioner, adj.	0.021	1.17	0.025	0.015
	[0.62]	[0.62]	[0.63]	[0.28]
N	6396	6396	4126	2470
% correctly specified by model	86.92%		88.22%	84.74%

Notes: T-values in parentheses. Significance levels: *0.10, **0.05, ***0.01. Mfx reports average marginal effects from a first difference logit model. Standard errors are clustered at the household level and sampling weights are included. The sample is restricted to Africans between the age of 18 and 59 at Wave 2.

Table 6: Effect of the Pension through the Chilcare Mechanism

	y= △employment				y=△childconst.
	(1)	(2)	(3)	(4)	(5)
	w/o child	with child	1-7 yr	8-17 yr	Probit
Panel A: HH *gained* pensioner	-0.008	-0.080*	-0.057	-0.088	0.121
	[0.028]	[0.049]	[0.028]	[0.053]	[0.130]
mfx: LOSE employment (-1)	-0.192**	0.178			
	[0.064]	[0.116]			
mfx: GAIN employment (+1)	-0.059*	-0.063**			
	[0.032]	[0.029]			
Panel B: HH *lost* pensioner	0.110*	0.047	0.012	0.053	-0.009
	[0.065]	[0.049]	[0.068]	[0.049]	[0.173]
mfx: LOSE employment (-1)	-0.118	-0.008			
	[0.149]	[0.16]			
mfx: GAIN employment (+1)	0.110	-0.033			
	[0.074]	[0.044]			
N	1998	3083	1552	1345	5077

Notes: Standard in parentheses. Significance levels: *0.10, **0.05, ***0.01.
Standard errors are clustered at the survey cluster level and sampling weights are included.
Mfx reports marginal effects from a multinomial logit model computed at the mean values of age and education. △childconst. is an indicator variable equal to 1 if the respondent listed childcare as a constraint to work in Wave 2 but not in Wave 1. The sample is restricted to African women between the age of 18 and 59.

Table 7: Effect of the Pension on labour Force Participation, Reservation Wage, Hours worked

	Labour Force Participation			Reservation Wage			Hours Worked (broad)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	△ elig	△gain/lose	IV	△ elig	△gain/lose	IV	△ elig	△gain/lose	IV
Δ pensioner(eligible)	-0.036			271.8			-2.814		
	[0.041]			[191.9]			[4.08]		
Δ pensioners (reported), adj.			-0.055			388.5			-5.639
			[0.064]			[272.8]			[8.24]
HH gained pensioner, pos inc		-0.019			251.9			-3.898	
		[0.057]			[305.5]			[5.60]	
HH lost pensioner (eligible)		0.045			-278.5			0.874	
		[0.045]			[307.2]			[2.901]	
p-value:Pen^{elig}_{Loss}=Pen^{L}_{Gain}		0.371			0.198			0.365	
Mean (baseline)	0.504	0.504	0.504	2537	2537	2537	32.8	32.8	32.8
R-squared	0.005	0.004	0.004	0.002	0.002	0.002	0.006	0.006	0.002
N	8671	8671	8671	1549	1549	1549	1668	1668	1668

Notes: Standard in parentheses. Significance levels: *0.10, **0.05, ***0.01. Standard errors are clustered at the survey cluster level and sampling weights are included. Columns 1-3 are restricted to Africans (aged 18 to 59). Columns 4-6 are restricted to Africans (aged 18 to 59) that were searching for jobs in both waves. Columns 7-9 are restricted to Africans (aged 18 to 59) that have any employment in both waves. Variables measuring reservation wage and hours worked are winsorized at the 1% levels. Results do not change qualitatively when I winsorize variables at other levels.

Table 8: Heterogeneous Treatment Effects: Gender, Age, Income

	y = Δsalaried employment			
	A. **Gender**		B. **Women**	
	men	women	w/ child	w/o child
Δ pensioners (eligible),adj.	-0.080**	-0.047**	-0.056*	-0.041
	[-2.16]	[-2.31]	[-1.75]	[-1.55]
1=HH gained pensioner	-0.040	-0.042	-0.080*	-0.008
	[-0.74]	[-1.47]	[-1.68]	[-0.27]
1=HH lost pensioner	0.134***	0.079**	0.050	0.110*
	[2.71]	[2.11]	[1.03]	[1.68]
N	3557	5081	3083	1998
Mean employment (Wave1)	33.3%	19.4%	21.5%	16.3%
#pensioners reported (Wave1)	0.25	0.27	0.252	0.294

	C. **Age**			
	18-29	30-39	40-49	50-59
Δ pensioners (eligible),adj.	-0.064***	-0.029	-0.100	-0.063*
	[-3.00]	[-0.52]	[-1.61]	[-1.69]
1=HH gained pensioner	-0.068**	0.042	0.029	-0.076
	[-2.18]	[0.47]	[0.41]	[-1.59]
1=HH lost pensioner	0.066*	0.124*	0.192**	0.012
	[1.86]	[1.72]	[2.24]	[0.75]
N	3853	1823	1619	1343
Mean employment (Wave1)	11.9%	38.1%	38.5%	31.6%
#pensioners reported (Wave1)	0.306	0.259	0.211	0.187

	D. **Income per-capita** (Quintile)				
	Q 1	Q2	Q3	Q4	Q5
Δ pensioners (eligible),adj.	-0.017	-0.056*	-0.138***	-0.000	-0.049
	[-0.49]	[-1.71]	[-3.73]	[-0.01]	[-0.63]
1=HH gained pensioner	-0.012	-0.050	-0.145**	0.051	-0.058
	[-0.24]	[-1.10]	[-2.48]	[0.80]	[-0.56]
1=HH lost pensioner	0.032	0.114**	0.140*	0.073	0.026
	[0.56]	[2.01]	[1.95]	[1.19]	[0.20]
N	2037	2184	1953	1707	757
Mean employment (Wave1)	4.30%	11.8%	25.1%	46.3%	65.1%
#pensioners reported (Wave1)	0.151	0.413	0.363	0.189	0.069

Notes: T-values in parentheses. Significance levels: *0.10, **0.05, ***0.01.

Regressions are estimated using OLS. Standard errors are clustered at the household level and sampling weights are included. 'HH gained pensioner' is an indicator variable for when the household gained at least one low income pensioner. '*w/ child*' refers to group of women that have at least one biological child living in the household. Per capita *Income quintiles* were constructed from a derived household income variable that aggregates all sources of income. '*# pensioners*' measures the number of reported pensioners in the household at the time of Wave 1.

The sample is restricted to Africans between the age of 18 and 59 at the time of Wave 2.

Table 9: Effect of the Pension on Forms of Employment

	Employment Type			
	(1)	(2)	(3)	(4)
	broad	salaried	self-emp.	casual
Δ pensioners (eligible)	-0.075***	-0.056**	-0.019*	0.001
	[0.028]	[0.023]	[0.010]	[0.013]
IV: Δ pensioners (reported)	-0.117***	-0.087**	-0.029*	0.001
	[0.048]	[0.038]	[0.016]	[0.02]
HH gained pensioner (eligible)	-0.049	-0.036	-0.010	-0.002
	[0.035]	[0.028]	[0.016]	[0.017]
HH lost pensioner (eligible)	0.136***	0.103***	0.034**	-0.003
	[0.046]	[0.036]	[0.016]	[0.028]
p-value:Pen^{elig}_{Loss}=Pen^{L}_{Gain}	0.001	0.003	0.038	0.975
Share employed (baseline)	0.362	0.253	0.056	0.048
N	8638	8638	8638	8638

Notes: Standard errors in parentheses. Significance levels: *0.10, **0.05, ***0.01.
Standard errors are clustered at the survey cluster level and sampling weights are included.
Coefficients reported in each column are from separate regressions.

Table 10: Probit Regression Analysis. y=Attrition Dummy

	(1)	(2)	(3)	(4)
	covariates	△ elig	△gain/lose	△g/l +cov
Δ pensioners (eligible), adj.		0.002		
		[0.04]		
1=HH gained pensioner (eligible)			-0.055	0.002
			[-1.05]	[0.04]
1=HH lost pensioner (eligible)			-0.173**	-0.129
			[-2.01]	[-1.48]
Education (years)	-0.012***			-0.012***
	[-2.83]			[-2.82]
Age (years)	-0.008***			-0.008***
	[-6.13]			[-6.07]
Employed (wave 1)	0.185***			0.182***
	[5.97]			[5.86]
Male	0.190***			0.190***
	[6.71]			[6.71]
African	-0.147***			-0.146***
	[-3.91]			[-3.89]
Pseudo R-square	0.0137	0.000	0.001	0.0139
N	10785	10795	10795	10785

Notes: T-values in parentheses. Significance levels: *0.10, **0.05, ***0.01. Standard errors are clustered at the household level. No sampling weights are used. The depdent variable is an indicator for whether the prime-aged adults attrited between Wave 1 and 2. The sample is restricted to Africans and Coloureds aged 18 to 59.

Table 11: Robustness Analysis - Sample Restriction to Non-attrition HH

	(1)	(2)	(3)	(4)	(5)	(6)
	△ elig	no attrit	IV	no attrit	metro	no attrit
Δ pensioners (eligible)	-0.048**	-0.054**			-0.026	-0.031
	[0.019]	[0.023]			[0.017]	[0.019]
Δ pensioners (report.)			-0.073**	-0.081**		
			[0.030]	[0.036]		
Δ pens. eligible x Metro					-0.090	-0.100
					[0.054]	[0.065]
R-squared	0.001	0.001	0.001	0.000	0.001	0.001
N	10161	8211	10161	8211	10161	8211

Notes: T-values in parentheses. Significance levels: *0.10, **0.05, ***0.01. Standard errors are clustered at the household level and sampling weights are included. The sample is restricted to Africans aged 18 to 59. 'no attrit' regressions restrict the sample to adults that did not have any attrition in their households.

Table 12: Baseline Characteristics of Treatment, Control, and Placebo Groups

	Control	Treatment		Placebo		TOTAL
	'no change'	gain	loss	disability	57-59 yr	
Male	44.3%	38.7%	43.7%	46.6%	42.4%	44.8%
Education	8.59	8.81	8.36	8.18	8.78	8.60
Age	30.7	27.7	28.7	27.7	27.2	30.2
Employment	38.3%	24.1%	18.4%	20.9%	23.4%	35.9%
Nr of Children	2.20	3.12	2.88	3.29	2.96	2.32
Metropolis	15.1%	10.1%	8.1%	11.8%	14.8%	14.7%
N	9,250	444	325	135	783	