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Stellenbosch Economic Working Papers: 02/14

KEYWORDS: WAGE SUBSIDIES; YOUTH EMPLOYMENT; SOUTH AFRICA; IMPACT
EVALUATION; RANDOMISED CONTROL TRIALS; EMPLOYMENT INCENTIVES.

JEL: C93, D22, H25, J22, J23, J58

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A WORKING PAPER OF THE DEPARTMENT OF ECONOMICS AND THE
BUREAU FOR ECONOMIC RESEARCH AT THE UNIVERSITY OF STELLENBOSCH

Wage subsidies and youth employment in South Africa: Evidence from a randomised control trial¹

JAMES LEVINSOHN, NEIL RANKIN, GARETH ROBERTS AND VOLKER SCHÖER

ABSTRACT

- Youth unemployment in South Africa is high, differs substantially by race group and is increasing. In 2012, close to two-thirds of young Africans were broadly unemployed. Over the four years prior to this the unemployment rate had increased by almost ten percentage points.
- A wage subsidy is one type of intervention which aims to reduce youth unemployment by providing a subsidy to firms which covers part of the cost of employing young people. The outline of a youth employment incentive was provided by the National Treasury in 2011.
- Evidence from other countries suggests that the success of a wage subsidy can be context specific and depends on the nature of the intervention and the structure of the labour market amongst other things. Thus, in order to understand how a wage subsidy may affect youth unemployment it is useful to know how South African young people and firms may react to a wage subsidy. A randomised control trial (RCT) is one way to investigate this.
- In an RCT the participants in the study are randomly divided into two groups – one which received the intervention, in this case a voucher for a wage subsidy which a firm who employs the individual could claim for six months (called the treatment group), and the second group which does not receive anything (called the control group). Since allocation to the groups is random and both groups share similar characteristics, any observed changes on average should be the result of the wage subsidy voucher. We can thus attach a causal interpretation to our results.
- The key finding of the paper is that those who were allocated a wage subsidy voucher were more likely to be in wage employment both one year and two years after allocation. The impact of the voucher thus persisted even after it was no longer valid. The magnitude of these effects was relatively large –

Research discussed in this publication has been funded by: the International Initiative for Impact Evaluation, Inc. (3ie) through the Global Development Network (GDN); the South African National Treasury and Department of Labour; the European Union through the Programme to Support Pro-Poor Policy Development (PSPPD); and the World Bank through the Strategic Impact Evaluation Fund (SIEF). The views expressed in this paper are not necessarily those of any of these funders. We thank Haroon Borat, Murray Leibbrandt, Dori Posel, David McKenzie, Kamilla Gumede, anonymous referees for 3ie and SIEF, and participants at the University of Oxford's Centre for the Study of African Economies' 2012 conference, the IZA/World Bank conference in New Delhi, and seminar participants at the Universities of the Witwatersrand, Cape Town, Stellenbosch, the Western Cape and Pretoria as well as the project steering committee and especially David Faulkner, Marlé van Niekerk and Monet Durieux for useful comments

those in the voucher group were 7.4 percentage points (approximately 25 percent) more likely to be in wage employment one year after allocation and of similar magnitude two years later. This impact was not driven by changes in the sample composition.

- This suggests that those young people who entered jobs earlier than they would have because of the voucher were more likely to stay in jobs. This confirms the important dynamic impacts of youth employment. It also suggests that government interventions which successfully create youth employment are important and can virtuous longer-term effects.
- Relatively few firms actually claimed the voucher. Interviews with firms and young people suggest that this was for a number of reasons: the young people did not even get a chance to show the voucher to someone who makes hiring decision; the administrative burden associated with claiming the money, although not onerous, could not be overcome (for example, larger firms did not have a process for accepting subsidy money, human resource functions were centralised and HR had little incentive to engage in the process of claiming the voucher); or managers or firm owners questioned the legitimacy of the voucher. This suggests that a national wage subsidy policy would need to be widely advertised and information and support provided to firms who would like to claim the subsidy.
- However, the impact of the voucher among those individuals who were employed in firms who claimed or enquired about the subsidy was much larger than the broader estimated effect. In this paper we are unable to ascertain whether these jobs were new or not.
- Even after controlling for firm take up and enquiry there is still a difference in the probability of wage employment between the group with a voucher and the group without. This indicates that part of the impact of the voucher is through supply side responses of those allocated the voucher.
- The results indicate that the observed impact of the voucher is not driven by changes in search, increases in search intensity or movement either to look for jobs or to take up employment.
- Rather it seems that part of the impact may be driven by people turning down job offers. Those in the control group, especially those in households with other employed members, were more likely to turn down job offers than those in the treatment group. This suggests that there is some queuing in the South African youth labour market as young people who can wait for better paid jobs do.
- We can only speculate about why those in the treatment group did not engage in this behaviour. One explanation is that the voucher changed their perceptions of potential success in the job market and thus they were more willing, or able, to go to these jobs, since they thought the voucher advantaged them or they were able to borrow money from their households to travel and incur the initial costs associated with accepting a job. It may also be that households which contained voucher holders were more likely to encourage the holder to take up the job since it was perceived as part of a special programme, or it may be that more information about jobs was passed onto the households of voucher holders with employees since they were linked into firms and people in their network may have known about the voucher.

- These results confirm that the structure of the household is important for success in the labour market. Research on South African labour markets shows that networks are the main channel through which information about jobs is transmitted. Households with working members are thus advantaged since members receive more information about jobs. These types of households can also provide intra-household cash transfers to help pay for transport costs or other costs associated with taking up a job.
- However, there can also be relatively negative implications for young people in households with other earners – they can afford to turn down jobs as they wait for potentially better paying or better matched jobs. The consequences of this may not necessarily be negative if these types of jobs eventually arrive but if they do not then these young people have sacrificed both earnings and work experience as they wait.

Keywords: Wage subsidies; youth employment; South Africa; impact evaluation; randomised control trials; employment incentives

JEL codes: C93, D22, H25, J22, J23, J58

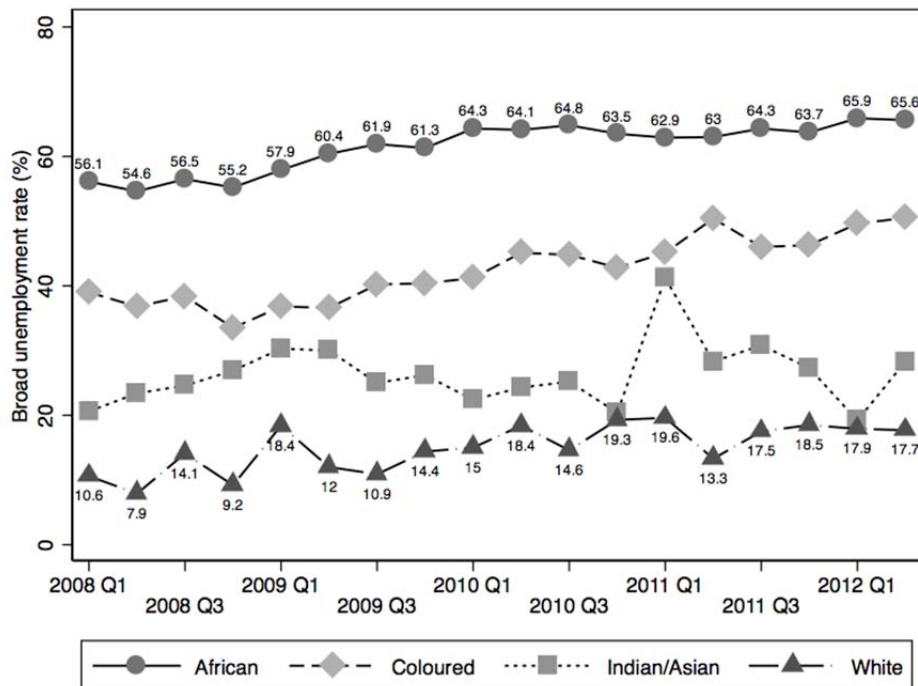
1. Introduction

High rates of youth unemployment when compared to older adults are a global phenomenon. However, unemployment among young South Africans is particularly high, differs substantially by race group and is increasing. In the second quarter of 2012, the broad unemployment rate for those aged 20 – 24 was above 60%.² For young Africans this figure is even higher – close to two-thirds are unemployed – and is in sharp contrast to white youth where less than a fifth of young people are unemployed. Since the beginning of the global financial crisis youth unemployment has also increased substantially. Over the four year period since the beginning of 2008 the unemployment rate among young Africans increased by close to ten percentage points. Unemployment is thus a particularly acute problem among an already marginalised group of South Africans.

Their twenties is also a crucial period which shapes the labour market trajectories of most South Africans. Many finish school, or further education, search for jobs and have their initial substantive work experience during this period. The age period of 20-24 seems particularly important. As Figure 2 shows, employment rates increase substantially between this period and the 25-29 age group as young people enter jobs. Despite this, unemployment rates remain high as young people approach 30. Previous work experience is highly correlated with whether someone currently has a job or not (Banerjee, et al., 2008) and thus finding a first job is crucial for the lifetime work trajectory of people. Since this often occurs in the 20-29 age group this is a key age group for government policy to target when aiming to reduce unemployment.

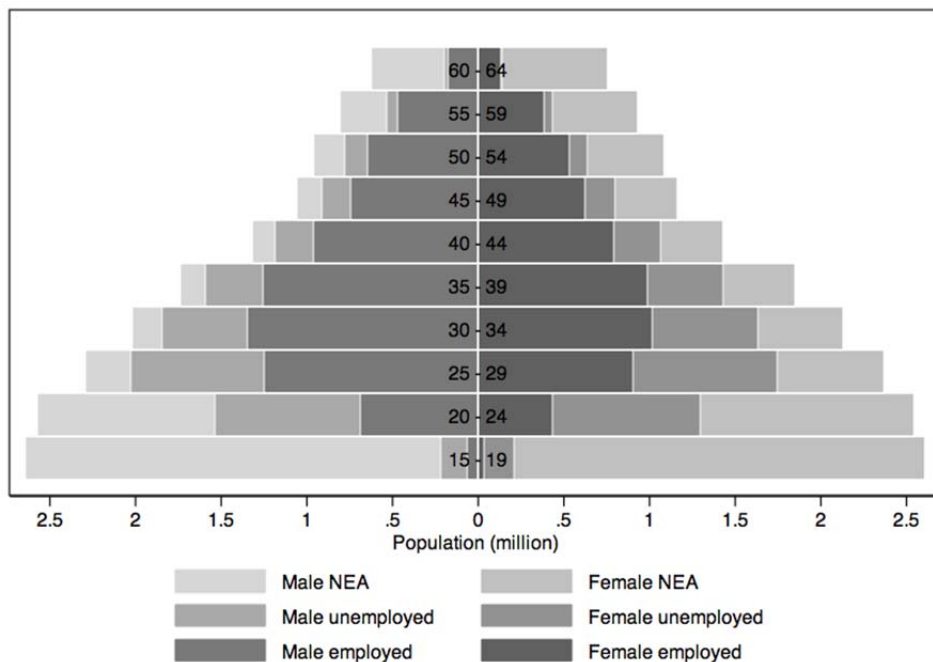
² We use the broad unemployment definition, rather than the official of narrow definition, throughout this paper. The broad definition differs from the narrow definition in that it includes people who have given up searching for a job – the so-called discouraged job-seekers. Since job search entails non-negligible costs, which young people may be less likely to overcome since they lack savings and networks, and there is evidence that the discouraged are actually worse off than those who search (see for example Kingdon & Knight, 2004) arguably the broad definition is more appropriate in the context of youth unemployment.

Figure 1. Broad youth unemployment rates by race



Note: These figures are calculated from Statistics South Africa's Quarterly Labour Force Survey (QLFS). The final period, quarter 2 of 2012, corresponds to the final survey period in this trial.

Figure 2. Labour market status by age, Africans



Note: These figures are calculated from Statistics South Africa's Quarterly Labour Force Survey (QLFS). The period used, quarter 2 of 2012, corresponds to the final survey period in this trial.

Given the high unemployment rates among this age group and the importance of this period for transitions into work there are a number of government interventions which aim to help young people enter jobs and acquire skills. Employers contribute funds through the Skills Development Levy towards workplace training. Sector Education and Training Authorities (SETAs) are funded from these contributions to provide training programmes that are tailored to the needs of specific sectors. Employers can employ workers through a Learnership contract in order to train workers while they receive practical work experience. Employers can also claim a tax allowance for workers who are trained through a recognised learnership or apprenticeship programme. The National Youth Development Agency runs job placement programmes, provides skills training (including training in life skills) and supports entrepreneurs through loans and training. The Department of Labour's Labour Centres help with job search, career guidance and curriculum vita development. The Expanded Public Works Programme (EPWP) provides short-term job opportunities, often requiring low skill levels, on government projects whereas the Community Works Programme (CWP) has a broader focus and aims to empower communities through a more holistic approach to job creation. Both of these programmes also have a training component. The current set of government interventions focuses mostly on the supply side of the labour market – through training and job search assistance. Demand side interventions are mostly from the government sector, through the EPWP, although the National Treasury's Jobs Fund does also fund innovative projects to create jobs.

An additional proposed intervention, designed to change the relative cost of hiring young people but not through directly reducing the wage they earn, is a temporary wage subsidy, in the form of a hiring voucher. Unlike existing policies, this approach would not dictate how firms should use the money, but merely make it cheaper to hire young people for a limited period. It would also be exclusively targeted at private firms. The South African National Treasury suggested an employment tax incentive and/or hiring voucher as possible policy options to boost job creation in 2009, and President Jacob Zuma formally announced the tabling of such a policy for consideration in his State of the Nation address in February 2010. Subsequent to this, the National Treasury has set out a proposal of how the policy would actually be structured in a discussion document (in February 2011). Most recently, the National Planning Commission has supported greater use of active labour market policies including a tax incentive to employers to reduce the initial cost of hiring young labour market entrants and cabinet passed the Employment Tax Incentives Bill, which creates tax breaks for firms who hire young people, in September 2013.

Evidence from other countries suggests that the success of a wage subsidy can be context specific and depends on the nature of the intervention and the structure of the labour market amongst other things (see Betcherman, Godfrey, Puerto, Rother, & Stavreska, 2007, for a summary of interventions to support young people). Given the context specificity, a randomised control trial (RCT) of a wage subsidy can provide an indication of how young people and firms may react. This paper discusses the results from an RCT of a hiring voucher among young Africans. It specifically investigates whether the allocation of a wage subsidy voucher to a group of young people affected their employment probabilities in the short-term (one year after allocation) and one year later once the eligibility for the voucher had lapsed. In an RCT the participants in the study are randomly divided into two groups – one which received the intervention, in this case a voucher for a wage subsidy which a firm who employs the individual could claim for six months (called the treatment group), and the second group which does not receive anything (called the control group). Since allocation to the groups is random and both groups share similar characteristics, any observed changes on average should be the result of the wage subsidy voucher. We can thus attach a causal interpretation to our results and can conclude that allocation of the voucher had a causal impact in increasing the probability of employment and that this impact persists even after the voucher lapses. Our results further suggest that one mechanism through which this worked was that young people in the control group in households with other members who were employed were more likely to turn down job offers. The persistence of an effect after the intervention ended shows that programmes which enable young people to become employed sooner have important positive longer term employment consequences.

2. The South African wage subsidy and how it could lead to more youth employment

In 2006 the National Treasury of the Republic of South Africa together with the Center for International Development, Harvard University, convened a panel of local and international experts to investigate growth constraints and opportunities. One proposal emerging from this exercise was a youth wage subsidy (Levinsohn, 2008). This proposal was intended to address two market imperfections preventing young school leavers from entering the labour market and thus contributing to low levels of economic growth and negative externalities (such as crime) associated with wide-spread unemployment. The first is the uncertainty about the productivity of young workers which firms face when hiring. School leaving certificates are often not a good indication of

the skills which firms require and young people usually have no previous work experience to act as a productivity signal. This uncertainty also means that firms are reluctant to invest in training. Dismissal costs, both monetary but also importantly administrative costs, and the perceived rigidity of labour regulations, mean that firms are unable or reluctant to hire young workers for a test period since they are concerned that it is difficult to get rid of bad worker matches. The second market imperfection is that a wage floor exists which prevents wages from falling and the market from clearing. In a more flexible market wages for young people could fall to compensate for the uncertainty of their productivity and lack of experience. However, institutional factors in the South African labour market, including bargaining arrangements, and historic factors, such as long commuting distances as a result of apartheid, mean that there is a floor in the labour market below which wages do not fall.

These two market imperfections result in *“a troubling equilibrium... [where]... the demand for labor is lower than it would be if workers’ types were observable (since firms hire based on expected skill levels), the incentives to obtain skills are diminished (since workers cannot be sure they will reap the benefits of their acquired skills), and racial inequality worsens (since firms’ best guess as to worker quality may involve race)”* (Levinsohn, 2008). Levinsohn argues that a subsidy would address these by lowering these costs associated with matching firms to workers by breaking *“this self-reinforcing and troubling equilibrium by, in effect, subsidizing search (by workers) and experimentation (by firms.)”* and would likely serve *“to increase employment, reduce discrimination, increase skill acquisition, and increases investment (because mobile capital is complementary to labor.)”*

The National Treasury’s proposed youth employment incentive, developed from Levinsohn’s proposal, would make it cheaper for firms to hire young people by providing a tax incentive which firms could claim if they hired young people. This would change the relative price firms face for young people, and encourage businesses to hire more. Since the subsidy is paid directly to firms the increase in demand is not driven by a fall in the wages young people earn but rather the cost of young people to the firm. In fact, *a priori*, it is not certain what will happen to the take home wages of young people under a wage subsidy – there could be certain circumstances where wages may actually rise as firms and workers receive some of the rents arising from the tax incentive.

In order to better understand the changes in behaviour and employment outcomes a wage subsidy could cause among young people, the African Micro-Economic Research Unit (AMERU) at the University of the Witwatersrand undertook a randomised control trial (RCT) of a wage subsidy voucher. This voucher was based on the Levinsohn (2008) proposal and although similar to, is not the same as the National Treasury (2011) proposal nor the Employment Tax Incentive Bill. As such, it does not provide conclusive evidence on whether the youth employment incentive proposed by the National Treasury would work or not but rather provides an indication of how young people may react to the implementation of a subsidy.

How would a wage subsidy lead to increased employment? The theory of change can be summarized in the following steps:

1. A young person is allocated a voucher that enables any firm (subject to the firm being registered for tax and paying unemployment insurance) that decides to employ this worker to claim back a portion of the wage that the firm pays to the worker.
2. This young person searches for a job through the channels that are available to them including networks, formal application procedures, and informal methods such as approaching firms directly.
3. The firm chooses to experiment with an additional worker who is unable to signal their productivity, knowing that the cost of employing this worker is temporarily reduced by the amount of the subsidy (less the administrative cost of claiming the subsidy).
4. Through this employment, the worker gains skills and references which increase their productivity and ability to signal this productivity, which in turn raises their income and the likelihood of being retained in employment or finding another job.
5. The firm not only increases the productivity of its workforce, but also raises the productivity of a young worker and reduces the uncertainty associated with the available pool of young workers.

The central premise on which this theory of change is based is that there are a group of young unemployed workers who, when given the opportunity, would be able to contribute to the output of South Africa and that firms would hire these workers if the relative cost of hiring, or the information

available about the productivity, of these workers changed. The complexity of the labour market, however, suggests that there are multiple points at which this theory of change may be challenged. For example, school-leavers may not believe the subsidy will increase their chances of finding employment, and may subsequently under-invest in search. The search channels available to a younger worker may also restrict the chance of finding firms that would be interested in hiring someone with the voucher. Similarly, the amount covered by the subsidy may be insufficient to induce the firm to experiment with an additional worker, or even if they do the consequent employment may be restricted to jobs that do not improve the skills of the worker or their ability to signal these skills (even to their own employer). Finally, there are also limits to the evaluation methodologies available to answer many of the questions that may arise from this theory and the multiple channels and linkages through which such a programme will effect change, particularly those relating to externalities and the broader economy wide impacts.

A wage subsidy may also have potential negative consequences. These include:

1. “Destructive Churning,” where firms dismiss workers once they are no longer subsidised and recruit subsidised workers to replace them;
2. Substitution of (older) workers without subsidies by those with;
3. Young people could under-invest in education by entering the labour market instead of completing their education or carrying on with further education;
4. The intervention could stigmatise workers who are subsidised;
5. The implementation of a subsidy covering a significant proportion of the population could lead to higher levels of inflation;
6. There could be potential for fraud.

An RCT, although unable to answer all these questions, provides some insight into some of them. In particular this paper attempts to answer the following questions:

- 1) Are those with a wage subsidy voucher more likely to be in employment as a result of the allocation of the voucher?
- 2) If yes, what are the mechanisms through which this effect works?

3) If there is an effect of the voucher does this effect persist after the voucher lapses and are there any discernible differences in the employment probabilities between these two groups two years after voucher allocation?

3. Global evidence on wage subsidies

Job subsidies, recruitment incentives and schemes to reduce non-wage labour costs are relatively common labour market policies in both developed and increasingly developing countries. A number of academic studies investigate the impact of these in a variety of contexts.

Gerfin, Lechner, & Steiger (2005) investigates the differential effects of two different Swiss employment subsidies – one a non-profit employment programme and another a subsidy for temporary jobs in private and public firms. Using matching methods they find positive effects of the subsidy relative to the employment programme. Both programmes are effective in raising reemployment probabilities for the unemployed having substantial difficulties in the labour market.

Galasso, Ravallion, & Salvia (2004) consider the impact of a wage subsidy and a wage subsidy coupled with training for participants on a workfare programme in Argentina. They do this through the use of a randomised experiment and correct for incomplete compliance.³ They find that voucher holders have a significantly higher probability of being in private sector employment 18 months after the baseline survey but training made very little additional difference.⁴ The difference in employment probabilities was largely driven by women and younger workers. Take up of the subsidy by firms was low which suggests that the voucher triggered a supply-side response. They conclude that given these low take-up rates the cost to the government was relatively low. However, they are not able to quantify potential displacement effects.

Betcherman, Daysal, & Pagés (2010) examines two employment subsidy schemes in Turkey. They use administrative data at the province level and staggered expansion of the programmes to identify

³ Incomplete compliance is when, although someone has been assigned to the treatment group, they do not take part in the programme or intervention.

⁴ The wage subsidy resulted in a 5 percentage point increase in the probability of being employment (from 9 percent in the control group to 14 percent in the treatment group).

the impact of these schemes. Using a difference-in-differences methodology they find that these programmes did lead to significant increases in net registered jobs (of between 5 and 15%). Most of this increase was within existing firms. However, there does seem to have been significant deadweight losses (jobs that were subsidised that would have been created anyway). There is some evidence, although based on limited data, that the dominant effect of the subsidies was to increase formal registration of firms and workers rather than boosting total employment and GDP.

More recently Groh, et al., (2012) examine the impact of a wage subsidy, soft skills training or a combination of the two on employment of female community college graduates in Jordan. They find that in the short-term the wage subsidy leads to a 40 percentage point increase in employment but the size of the impact falls and is no longer statistically significant four months after the voucher period ended. The effect does seem to persist outside of the capital city of Amman and they argue that this may be the result of displacement of other job seekers. They also find that employability training does not increase the probability of employment. These results suggest that one of the reasons for high unemployment among educated young Jordanians is that the minimum wage is too high.

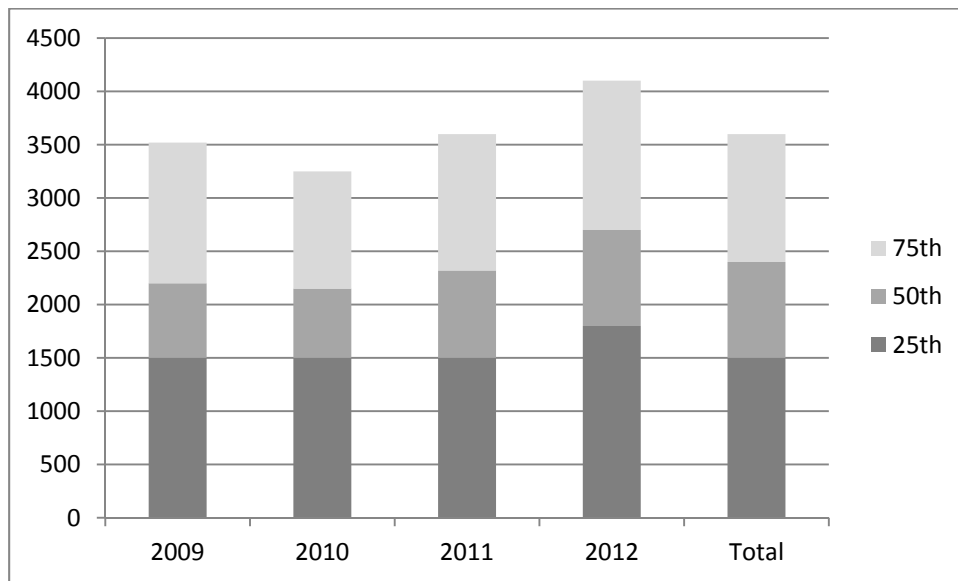
There are also a number of papers that attempt to model the impact of a wage subsidy in South Africa. Both Go, Kearney, Korman, Robinson, & Thierfelder (2010) and Burns, Edwards, & Pauw (2010) use computable general equilibrium models to evaluate the economy wide effects of a wage subsidy in South Africa. Both show a relatively large range of potential impact that is driven by assumptions about the wage elasticity. Pugatch & Levinsohn (2012) develop a structural search model to better understand the impact of a potential wage subsidy among Cape Town youth. They suggest that a R1000/month wage subsidy paid to employers leads to an increase of R660 in mean accepted wages and a decrease of 15 percentage points in the share of youth experiencing long-term unemployment. This happens because the wage offers made by firms increase since they can afford to pay young workers more as they receive a subsidy for these workers. In this best-case scenario the amount of the subsidy is fully passed through to job seekers in the form of higher wage offers. Should employers have some market power in the youth labour market then this pass-through will be incomplete and the impact will be lower. All these South African specific studies are simulations rather than field experiments.

4. Methodology

The wage subsidy evaluated in this study is broadly based on Levinsohn's 2008 proposal – Africans aged between 20 and 24 were randomly allocated a voucher which would allow firms that employed them to be compensated for a portion of the wages they paid to these individuals. The subsidy amount was capped at half the wage or R833 per month (whichever was lower) and could be claimed for a minimum of six months or until the R5,000 that each subsidised individual was allocated ran out. As Figure 3 shows median wages in the sample varied between R2,150 and R2,700 over the study period and for the first three rounds of the survey (2009-11) at least 25 percent of those working were earning R1,500 or less per month. The subsidy amount of R833 per month was thus almost 40 percent of median monthly wages when vouchers were allocated in 2010 and half the wage for at least a quarter of the working individuals.

In order for a firm to claim the voucher they needed to demonstrate that they employed the person with the voucher, through for example a contract, and this was also checked through telephonic contact. Firms had to be officially registered for tax and be paying unemployment insurance. They were required to submit a formal invoice to Wits Enterprise, the entity within the university which managed the project. Subsidies were transferable between companies – an individual took the unclaimed subsidy with them should they leave a firm – and individuals needed to be employed full-time in a formal non-government business. The study was run as an RCT with baseline surveys in 2009 and 2010, the allocation of the vouchers in 2010, and follow up surveys in 2011 and 2012.

Figure 3 Within sample monthly wages (in current Rands) by percentile, conditional on working



In 2009 a baseline sample of 4,009 young people was undertaken. These individuals were aged between 20 and 24 at the time of interview and were drawn from random clusters sampled with a probability weight based on the proportion of young Africans living in them based on the 2001 census. We call this the enumeration area (EA) sample. In addition to this, a second sample of young people who had registered at the Department of Labour’s Labour Centres were interviewed. These individuals were selected randomly from those visiting the Centres and from the individual Centre databases. The chosen Labour Centres were those located in the EA clusters or those closest to the EA clusters. Approximately 2,500 young people were part of the EA sample with the balance drawn from the Labour Centres.

Sampling was done in three regions – the Johannesburg metropolitan area in Gauteng province; the eThekweni (greater Durban) metropolitan area of KwaZulu-Natal province (which although classified as a metropolitan area did include some rural areas within the boundaries of the metro); and the urban area of Polokwane and surrounding rural areas of the Limpopo province. A structured survey which captured demographic and household characteristics, education levels, and previous and current labour market experiences was administered to these individuals. In 2010, these individuals were re-interviewed and a random selection of these were given wage subsidy vouchers and had the process of claiming these vouchers explained to them. These people were then re-interviewed in 2011 and 2012 and their labour market outcomes recorded.

The treatment and control groups were determined using pair-wise matching and this was done by researchers using processes that ensured that no individual was treated in a biased fashion. The respondents were assigned to separate groups based on where the enumeration areas were located and gender. Pairs were then identified using a Mahalanobis matching algorithm (an approach which Bruhn & McKenzie, 2009, suggest is appropriate when outcome variables are persistent). This algorithm identified pairs based on age, whether the respondent had a Matric (the South African school leaving certificate), a degree or diploma, the number of earners in the household, and the self reported activity of the respondent at the time of the interview in 2009. Individuals in matched pairs were randomly allocated to either the treatment or control group. In some cases three respondents had identical Mahalanobis scores. In these cases one treatment observation was randomly selected and the remaining two were assigned to the control group.

The composition of the sample is shown in more detail in Appendix A as are descriptive statistics of key variables, balance and the change in sample composition over the various rounds of the survey.

5. Results

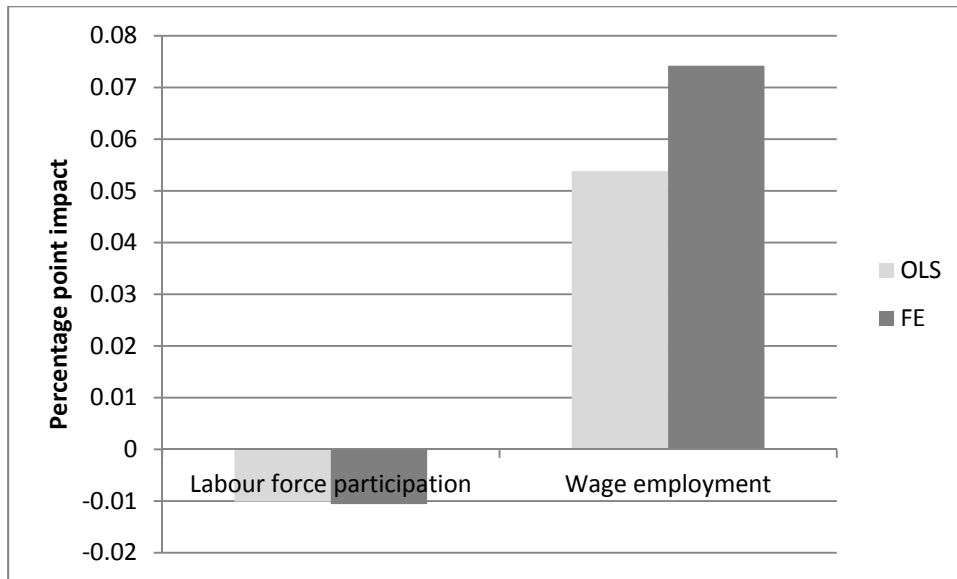
5.1 One year after allocation

The wage subsidy voucher led to a higher employment one year after receiving the voucher. This impact is relatively large – between 5.4 and 7.4 percentage points.⁵ Given that only 24 percent of those not in wage employment in the control group in 2010 transitioned into employment in 2011 and overall only 31 percent of those in the control group were in wage employment in 2011 these results suggest that the voucher increased employment by 24 percent.⁶

⁵ These results come from two different estimators. The first is ordinary least squares (OLS). The second is a matched fixed effects (FE) estimation since we used pair-wise matching to group individuals who were then randomly assigned between the treatment and control (similar to the suggestion by Bruhn & McKenzie, 2008). This also helps to control for attrition based on these observable characteristics since the treatment coefficient is identified only in those matched pairs where both a representative from the treatment group and the control group remains in the sample. We report both estimates for completeness but our preferred estimator is FE.

⁶The probability of being in employment is measured as a percentage (for example a 50 percent probability of being in employment among a group would mean that half are expected to be in employment). A change, or difference, in this probability is referred to as a percentage point change, or difference. The probability of

Figure 4. The impact of the subsidy voucher, in percentage points, on labour force participation and wage employment one year after allocation



Note: These are the coefficient estimates take from the regression results in Table 2

The impact of the voucher on employment is not driven by more people entering the labour force since estimates on labour force participation are not significantly different to zero. This indicates that, at least in this sample, the voucher did not cause young people to exit education and begin looking for a job.

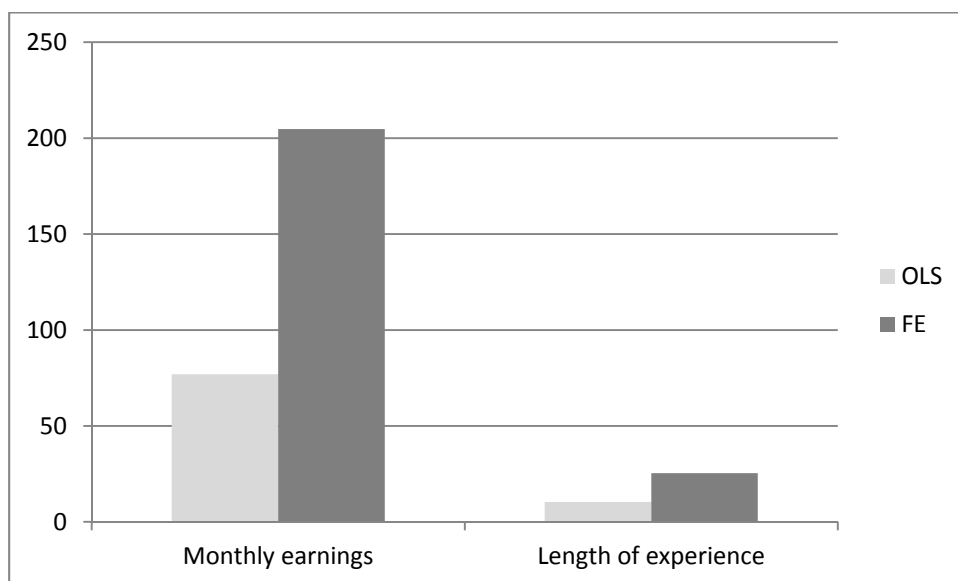
The wage subsidy voucher also seems to have had a positive impact on monthly earnings. The fixed effects estimation suggests that those in the treatment group earn approximately R200 more per month (14 percent) than those in the control group.⁷ This effect is not significantly different from zero and is also due to the fact that those in the treatment group are more likely to be in jobs and thus earning wages. There is a positive and significant effect on the length of employment too. Those in the voucher group had almost one month more work experience than those in the control group – a 21 percent increase in employment length.⁸

those in the control group being in employment in 2011 is 31 percent and the probability of those in the treatment group being in employment is 7.4 percentage points higher. To represent this as a percentage change requires 7.4 to be divided by 31.

⁷ These estimates include everyone in the sample and are not conditional on being in wage employment. Those without jobs are given a monthly wage of 0.

⁸ Employment length and earnings are both dependent on whether a person is employed or not (and thus affected by the voucher). Those not in employment are assigned 0s for both of these. If we only consider those

Figure 5. The impact of the subsidy voucher on monthly earnings (rand) and length of employment (days) one year after allocation



Note: These are the coefficient estimates take from the regression results in Table 2

5.2 Two years after allocation

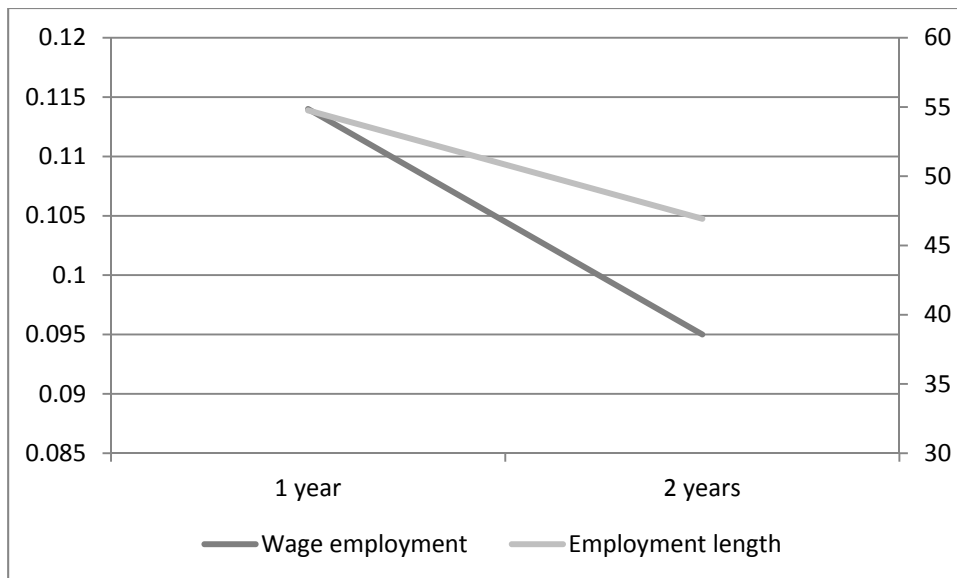
The impact of the voucher on the probability of wage employment persists two years after allocation of the voucher (estimation results can be found in Table 3). The fixed effects estimate suggests that those in the voucher group are almost ten percentage points more likely to be in wage employment than those who were in the control group. The impact on employment length also persists – individuals in the treatment group have a month and a half more work experience on average compared to those in the control. As with the one year estimation results there is no statistically significant effect on labour force participation or monthly earnings.⁹

who were in wage employment in 2011, monthly earnings are approximately R325 lower for those with the voucher and employment length is approximately 22 days less (from the OLS estimate). Both these estimates are not significantly different from zero. These lower estimates are consistent with more individuals in the treatment group finding new jobs and hence having less time in these jobs and being paid lower wages as they start these jobs.

⁹ The negative, but not significant, point estimate on wages is consistent with an explanation that those with the voucher took lower paid jobs than they would ordinarily have, or because more entered employment, and these entry level wages were low, compared to the wages for those in the control group who may have been in employment for some time.

The sample of individuals changes over the two year period as some people could not be located or refused to participate in the follow up survey of 2012. In order to compare magnitudes of these effects over time we confine our sample to those who were present in both years. Figure 6 indicates that the effects are positive after both 1 and 2 years. As expected, the effects on both employment and the length of employment spell are larger one year after allocation compared to two years later. This indicates that the voucher had no further effect after it lapsed and that some of those who became employed as a result of the voucher were not able to retain their jobs or transition into new ones.

Figure 6. Treatment coefficient estimates for wage employment probabilities (left hand scale) and employment length in days (right hand scale) 1 and 2 years after allocation. (Consistent sample.)



6. The mechanisms through which the impact worked

The voucher had a causal impact on being in wage employment both one year and also a second year after allocation when the voucher had lapsed. The magnitudes of these effects are relatively large and suggest that interventions which get young people into jobs earlier can have positive medium term effects on employment which could potentially lead to further positive long-term effects. However, these results do not indicate anything about the mechanism through which the impact happens.

These results might be driven by a host of factors. The observed higher employment probabilities may be due to firms employing people with vouchers or it may be due to supply-side responses or other factors or a combination of these. These supply side responses include: individuals searching harder or differently; those with vouchers hearing about more jobs; or people accepting different types of jobs which they would not normally have accepted. The voucher could also have given the impression that they were endorsed by the University (reverse stigmatisation); allowed them to get into the pool of applications; or have gotten them passed the security guard at the entrance of the company. The observed impact could also driven by changes in the sample composition, including non-random attrition. In this section we consider these various mechanisms which may be driving these results.

6.1 Understanding the voucher

The first issue we investigate is that of incomplete compliance and understanding the voucher. In the general RCT literature incomplete compliance refers to individuals who are assigned to the intervention or programme but do not end up taking part. In this trial we use understanding the voucher and actually using the voucher to proxy for incomplete compliance. In the follow up round of 2011 it was clear that some of those who had received a voucher did not fully understand it. Table 1 shows that 63.5% of those with the voucher who were interviewed in 2011 understood how it worked.¹⁰ Amongst the group who understood the voucher transitions into wage employment were three percentage points higher than in the group that did not understand the voucher. Furthermore, not everyone actually given the voucher used it to approach firms. 54 percent of those in the voucher group approached businesses with the voucher. Those who were in wage employment in 2011 are less likely to have approached firms with the voucher. This may, but does not necessarily, mean that it is not the firm's response to the voucher that is driving the observed impact. There is also a large degree of correlation between understanding and using the voucher. Of those who understood the voucher 60 percent used it to approach firms compared to only 44 percent of those who did not understand the voucher.

¹⁰ In the survey round of 2011 a number of questions were asked to respondents about what they understood the voucher to mean. These were then used to classify the treatment group into those who understood the voucher and those that did not.

In Table 4 we present results using a measure of whether an individual understood the voucher and whether the individual used the voucher to approach firms. In 2011 individuals were asked about what they thought the voucher meant. They were also asked about whether they had approached any businesses with the voucher. These answers are used to create binary variables for those who understood the voucher and those who used the voucher. These results are presented in the first four columns. In the next four columns we use an Instrumental Variable estimator, where allocation to the treatment group is used as an instrument for whether the individual understood the voucher in the 5th and 6th columns or whether the individual used the voucher in the 7th and 8th columns. In the last two columns we interact understanding and using the voucher but do not instrument.

When instrumenting with allocation to the treatment group, both understanding the voucher and using it are significant in the separate regressions. The coefficient estimates are also larger than those on allocation to the treatment group. This suggests that it is not merely being allocated to the voucher group but actually understanding the voucher or using it which is driving the increase in employment probability. When both understanding and using the voucher, and the interaction between these two, are included in the regressions but not instrumented since we have only allocation to the treatment group as an exogenous instrument, this suggests that the positive observed impact is due to understanding the voucher. There is no significant difference in employment probability between those who do not understand the voucher but use it, or those that understand it and use it, and the control group.¹¹

¹¹ One explanation for this finding is that understanding of the voucher is a proxy for an unobservable characteristic or set of characteristics which are correlated with finding a job. Those who understand the voucher may be better at assimilating and communicating information or have better cognitive skills. Since we only ask those with a voucher about their understanding of it we cannot use this variable to compare across the two groups. Instead we asked a series of six maths questions which differed randomly between respondents (so that they did not receive help from the enumerators). Arguably these could proxy for the ability to understand and solve a problem. We also use the confidence of the person in speaking English (the dominant language of employers in these areas in South Africa) in 2009 before assignment of the voucher. This may proxy for the ability to communicate. Even when we control for the number of these questions successfully answered and confidence in English the results remain unchanged. Furthermore, these characteristics are orthogonal to assignment to the treatment group which we use as the instrument for understanding.

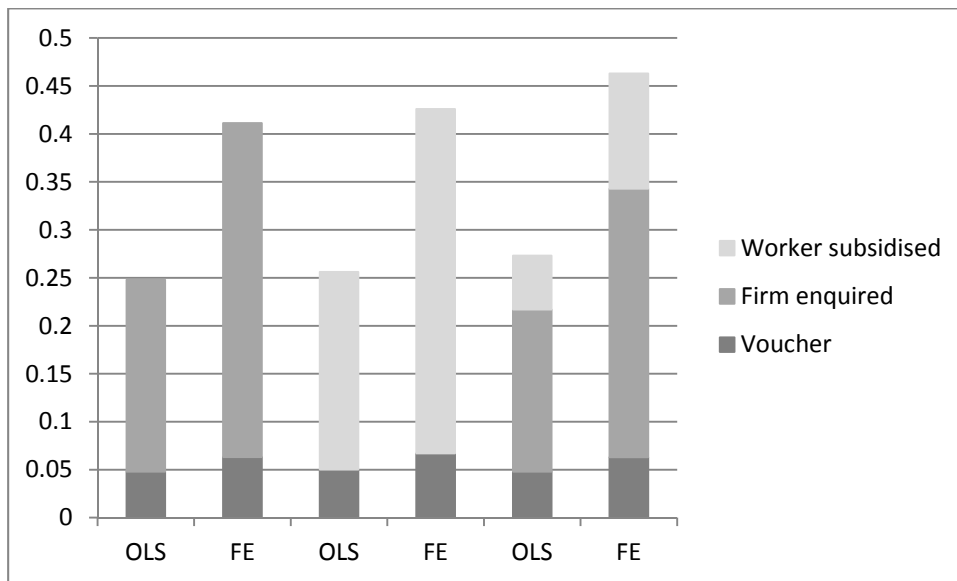
6.2 Firm responses

In the trial very few firms actually took up the voucher (22 firms), although a larger number enquired about the voucher (an additional 16).¹² Enquiring about the voucher may be a signal of its efficacy since firms may have hired individuals based on the voucher but not have taken it up once they learnt of the process they had to go through. Interviews with firms and young people suggest that take up by firms was low for a number of reasons: the young people did not even get a chance to show the voucher to someone who makes hiring decision; the administrative burden associated with claiming the money, although not onerous, could not be overcome (for example, larger firms did not have a process for accepting subsidy money, human resource functions were centralised and HR had little incentive to engage in the process of claiming the voucher); or managers or firm owners questioned the legitimacy of the voucher.

In Table 5 we investigate whether it is the firms claiming the voucher or those who enquired that are driving the observed treatment effect. Assignment to the treatment group remains significant even after controlling for pair-wise matching. An individual whose employer has claimed the subsidy is a further 36 percentage points more likely to be employed. The magnitude of the effect of a firm enquiring about the subsidy is similar. If both claiming the subsidy and enquiring about the subsidy are included both measures become insignificant, however this is due to the high correlation between the two measures. These results indicate that the observed treatment effect is not wholly due to firm responses, although the magnitude of the impact is much larger for those firms who have taken up the subsidy. This is understandable given that at the time of interview some of the firms were still drawing the subsidy.

¹² We were able to capture information from firms that enquired about 36 individuals in the sample, however there were some firms that enquired but that did not provide names or voucher numbers and thus we are unable to match them with individuals in the sample. Overall enquiries about the project numbered more than 100.

Figure 7. The magnitude of the impact for firm take up or enquiry on the probability of being in wage employment. (2011)



Note: These are the coefficient estimates take from the regression results Table 5. These are percentage point changes. These are stacked to indicate the cumulative impact, thus for example on average those in the voucher group who are employed by a firm which enquired about the voucher and subsequently took it up were 46 percentage points more likely to be in wage employment in 2011 than someone without a voucher (based on the point estimates from the fixed-effects estimation).

These results are similar for the two year follow-up survey. Allocation to the treatment group remains significant even after controlling for firm enquiries or a firm claiming the subsidy. People employed by firms which enquired about the voucher are a further 35 percentage points more likely to be employed and those whose firms claimed the subsidy a further 27.5 percentage points more likely compared to those in the treatment group, however the coefficient on the subsidy variable is not significant, due to a small number of individuals in this category in the sample.

For both one year and two years after allocation the coefficient estimate on the treatment variable remains significant but reduces in magnitude. This suggests that there are other factors, not merely firm take up, which contribute to the observed impact.

6.3 Search and supply side responses

The evidence above suggests that even after controlling for firm level responses to the voucher there is still an effect from having been allocated a wage subsidy voucher. This suggests that the

effect may be driven by a supply side response. There are well known challenges with estimating the impact of a treatment on an outcome variable through a mediator, such as search behavior in our case (see for example the discussion in Gerber & Green, 2012). Treating the mediator as a right hand side regressor may introduce bias and typically it will seem that most of the effect happens through the mediator (Gerber & Green, 2012).¹³

In order to avoid this issue we treat search, and other potential mediator variables, as an outcome variable and investigate whether there are any differences in search behavior between the treatment and control groups. In the 2011 interview, respondents were asked whether and how they changed their search behaviour in the month after the 2010 interview. They also indicated in which months between the two interviews they searched the most intensively. Table 7 – Table 9 show the differences in search behavior between the treatment and control group. We also instrument for understanding the voucher. Surprisingly these results show some evidence of a negative relationship between having or understanding the voucher and changing search behavior to search more or approach more firms, although these results are only weakly significant. Although not significant there is also a positive relationship between having the voucher and changing the search approach. These regressions show no relationship between the voucher and searching most intensively one month after the interview or moving (which people might do to look for or take up jobs).

What does emerge from these results though is a relationship between the voucher and turning down job offers. In the fixed effects estimations the coefficients on the voucher group and understanding the voucher are negative and significant at the 5 percent level – those in the voucher group are almost 3 percentage points less likely to have turned down a job in the period between the allocation of the voucher in 2010 and the follow up interview in 2011 compared to the control group. Those who understood the voucher are over four percentage points less likely to have turned down a job offer. Given the proportion of those turning down a job offer in the treatment group, it suggests that those in the control group are more than twice as likely to turn down a job offer.

¹³ This is indeed the case if search intensity, and search intensity interacted with the treatment are included on the right hand side. In the fixed effects estimations the point estimate on the voucher variable is 0.

This seems perverse – why would young people in a high unemployment environment turn down plausible job offers and how do these individuals support themselves given they are unemployed? In Table 10 we investigate this by dividing the control and treatment group between those who are (or were) in households with other employed members in 2010. In these types of households there may be transfers between members to help with search or living expenses. Given smaller cell sizes many of the coefficients are no longer significant but the point estimates give an indication of a plausible explanation. Those in the control group with employed members in the household are most likely to turn down job offers. These types of individuals are also least likely to be in wage employment and if they are in wage employment then they earn the highest wages on average. These results suggest that these individuals may be queuing for higher income jobs and can afford to do this since there are other earners within the household who can support them. Why does this not happen for those with vouchers? One explanation may be to do with the flow of information. Most information on job availability comes through networks with links into firms. Individuals in households with employed family members are thus likely to hear about more jobs than those without (it is this flow of available jobs which respondents interpreted as job offers). However, people will only follow up on this information if they believe that they stand a good chance of getting the job since applying and getting to the job is expensive and often requires the individual to incur initial costs for things such as transport which can only get repaid a few days or weeks later when the individual receives their first pay cheque. The wage voucher may have changed the perceived probability of successfully getting a job and thus those in the voucher group may have decided to follow up on this information more often. It may also have changed the perception of the individual's household and thus they would have been more willing to lend money for transport and/or more likely to insist that the young people with vouchers follow up on this information.

6.4 Attrition and sample composition

The presence of individuals in subsequent rounds of the surveys may be correlated with the assignment of the voucher and the outcome variables of interest. Those with the voucher may be more likely to participate in follow up rounds if they believe that contact with the researchers and the voucher advantages them in the labour market. Those in wage employment may be less likely to participate since the opportunity cost of their time is now higher and they may believe that they no longer need to participate in the research since they already have a job.

Table 11 presents the factors associated with attrition from the sample in a future round. Between the 2010 wave (the round where the voucher was allocated) and the 2011 round, neither being in the voucher group nor being currently employed is related to whether the person participated in the 2011 wave. This suggests that the results for the first year follow-up after allocation are not driven by attrition associated with being assigned the voucher or being in employment in the 2010 round of the survey. However, being in the voucher group is negatively associated with attrition between 2011 and 2012 – those with the voucher are more likely to have been interviewed in 2012, and being employed is positively associated with attrition. This suggests that the results for 2012 may overstate the impact since those in the control group who were employed in 2011, and subsequently would be more likely to be employed in 2012, are more likely to attrite.

In order to understand how this correlation between attrition in 2012, treatment and employment status may affect the coefficient estimates on the treatment group for wage employment we re-estimate the fixed effects regression under a number of different hypothetical scenarios. First we assume that all of those who left the sample in 2012 maintained the same employment status as they had in 2011. Under this assumption the estimate on the treatment group become 0.082 which is slightly lower, but not significantly different from, the initial estimate of 0.095. In the second scenario we assume that those from the treatment who left the sample in 2012 also left wage employment whilst those in the control group maintained the same employment status as in 2011. The coefficient estimate is now 0.03. Thirdly we assume that those in the treatment group who left maintained the same employment status as in 2011 whilst those who left from the control transitioned into wage employment. In this scenario the point estimate on the treatment group variable is 0.021. In the last scenario we make the extreme assumption that all those who left from the treatment group were not in wage employment whilst all those who left from the control were in wage employment. In this case the point estimate is -0.028.

In column 5 of Table 11 we interact being in the treatment group and wage employment in order to determine which of the four groups – wage employed in the treatment group, wage employed in the control group, non wage employed in the treatment group, and non wage employed in the control group – are most likely to attrite in 2012. This gives an indication of which of the scenarios above may be most likely. Of the four groups, those in the treatment group with jobs in 2011 are most likely to attrite (0.051 percentage points more likely than those in the control group without jobs).

The next most likely are those in the control group with jobs (0.041 percentage points) and the least likely are those in the treatment group without wage employment (-0.057 percentage points). Based on this it suggests that the treatment group which remains in 2012 is composed of individuals less likely to have jobs than the group in 2011. Although this composition change is also present for the control group it is relatively smaller. Since those who are most likely to leave in both groups are those with jobs and this is more likely in the treatment group this suggests that the scenarios where both groups are more likely to transition into wage employment or maintain the current status are most likely. Thus the impact of the voucher two years on is most likely to be towards the top of the range given in the scenarios.

7. Conclusions and implication for policy

This paper discusses a randomised control trial of a wage subsidy voucher for young Africans in South Africa. Unlike the South African National Treasury's proposal for the broader wage subsidy and the Employment Incentive Tax Bill, these vouchers were given directly to young people, and firms had to claim the subsidy, rather than directly to firms. The structure of the experiment and concerns from the firms about the legitimacy of the voucher and their willingness to claim the voucher mean that this study is not a direct test of the likely success of the wage subsidy policy. Despite these limitations, the study does provide a wealth of information on how young people and firms may react to the subsidy, the impact of these types of interventions in the youth labour market and the mechanisms through which these impacts may occur.

The results suggest that at least one concern about the impact of the wage subsidy – that young people would exit school to pursue search or employment – is unfounded. We find no significant impact of the voucher on labour force participation indicating that young people did not give up education and enter the labour force.

The key finding of the paper is that those who were allocated a wage subsidy voucher were more likely to be in wage employment both one year and two years after allocation. The impact of the voucher thus persisted even after it was no longer valid. The magnitude of these effects was relatively large – those in the voucher group were 7.4 percentage points (approximately 25 percent) more likely to be in wage employment one year after allocation and of similar magnitude two years

later. This suggests that those young people who entered jobs earlier than they would have because of the voucher were more likely to stay in jobs. This confirms the important dynamic impacts of youth employment. It also suggests that government interventions which successfully create youth employment are important and can have virtuous long-term effects.

Relatively few firms actually claimed the voucher. Interviews with firms and young people suggest that this was for a number of reasons: the young people did not even get a chance to show the voucher to someone who makes hiring decision; the administrative burden associated with claiming the money, although not onerous, could not be overcome (for example, larger firms did not have a process for accepting subsidy money, human resource functions were centralised and HR had little incentive to engage in the process of claiming the voucher); or managers or firm owners questioned the legitimacy of the voucher. This suggests that any wage subsidy policy at a national level would need to be widely advertised and information and support provided to firms to would like to claim the subsidy. However, the impact of the voucher among those individuals who were employed in firms who claimed or enquired about the subsidy was much larger than the broader estimated effect. In this paper we are unable to ascertain whether these jobs were new or not. Even after controlling for firm take up and enquiry there is still a difference in the probability of wage employment between the group with a voucher and the group without. This indicates that part of the impact of the voucher is through supply side responses of those allocated the voucher.

The results indicate that the observed impact of the voucher is not driven by changes in search or increases in search intensity which we were able to monitor in the survey, or movement either to look for jobs or to take up employment. Rather it seems that part of the impact may be the result of people turning down job offers. Those in the control group, especially those in households with other employed members, were more likely to turn down job offers than those in the treatment group. This suggests that there may be some queuing in the South African youth labour market as young people who can wait for better paid jobs do. We can only speculate about why those in the treatment group did not engage in this behaviour. One explanation is that the voucher changed their perceptions of potential success in the job market and thus they were more willing, or able, to go to these jobs, since they thought the voucher advantaged them or they were able to borrow money from their households to travel and incur the initial costs associated with accepting a job. It may also be that household which contained voucher holders were more likely to encourage the holder to

take up the job since it was perceived as part of a special programme, or it may be that more information about jobs was passed onto the households of voucher holders with employees since they were linked into firms and people in their network may have know about the voucher.

In terms of policy implications these results confirm at least two things. First, that the structure of the household is important for success in the labour market. Research on South African labour markets shows that networks are the main channel through which information about jobs is transmitted (Rankin & Schöer, 2011). Households with working members are thus advantaged since members receive more information about jobs. These types of households can also provide intra-household cash transfers to help pay for transport costs or other costs associated with taking up a job. However, there can also be relatively negative implications for young people in households with other earners – they can afford to turn down jobs as they wait for potentially better paying or better matched jobs. The consequences of this may not necessarily be negative if these types of jobs eventually arrive but if they do not then these young people have sacrificed both earnings and work experience. Second, getting young people into jobs earlier than they ordinarily would have been employed without an intervention has positive effects that outlast the intervention. Two years after the voucher was allocated those in the voucher group still had a higher probability of being employed than those that in the control group. Interventions in the labour market which assist young early on can have virtuous dynamic impacts and thus need to be prioritised.

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Tables

Table 1 Understanding and using the voucher, 2011 sample

		Wage employed		
		No	Yes	Total
Understood the voucher				
No	n	285	151	436
	% row	65.37	34.63	100
	% col	37.65	34.4	36.45
Yes	n	472	288	760
	% row	62.11	37.89	100
	% col	62.35	65.6	63.55
Total	n	757	439	1,196
	% row	63.29	36.71	100
	% col	100	100	100
Approached businesses with the voucher				
No	n	294	218	512
	% row	57.42	42.58	100
	% col	41.35	52.91	45.59
Yes	n	417	194	611
	% row	68.25	31.75	100
	% col	58.65	47.09	54.41
Total	n	711	412	1,123
	% row	63.31	36.69	100
	% col	100	100	100

Table 2. Impacts on different dimensions of employment – one year after voucher allocation (2011)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	Labour force participation		Wage employment		Monthly earnings		Employment length	
Estimator:	OLS	FE	OLS	FE	OLS	FE	OLS	FE
Independent variables:								
Voucher	-0.0101 (0.0173)	-0.0106 (0.0213)	0.0538*** (0.0187)	0.0742*** (0.0233)	76.93 (135.3)	204.7 (171.6)	10.38 (10.97)	25.42* (13.90)
Male	0.0374** (0.0181)		0.107*** (0.0206)		782.5*** (133.1)		26.08** (11.59)	
Earners in household	-0.000670 (0.00202)	-0.00465 (0.00303)	2.48e-05 (0.00179)	0.000892 (0.00331)	12.07 (10.17)	15.34 (24.40)	-0.0779 (0.638)	-0.0447 (1.969)
Grade 11	0.0541 (0.0413)	0.0267 (0.0763)	0.0266 (0.0362)	0.0160 (0.0833)	212.6 (140.8)	62.00 (614.3)	-7.221 (19.30)	29.13 (49.96)
Matric without endorsement	0.107*** (0.0371)	0.0945 (0.0870)	0.114*** (0.0337)	0.0566 (0.0949)	831.8*** (150.8)	655.9 (699.8)	57.01*** (19.96)	39.04 (56.67)
Matric with endorsement	0.0803* (0.0429)	0.104 (0.0930)	0.179*** (0.0416)	0.176* (0.101)	1,645*** (247.4)	1,379* (748.2)	65.69*** (22.63)	77.47 (60.58)
	0.0541	0.0267	0.0266	0.0160	212.6	62.00	-7.221	29.13
Sampling cluster fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,358	2,358	2,358	2,358	2,358	2,358	2,354	2,354
R-squared	0.060	0.089	0.067	0.086	0.072	0.062	0.047	0.071
Number of pairs		1,598		1,598		1,598		1,598

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 3. Impacts on different dimensions of employment – two years after voucher allocation (2012)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	Labour Force Participation		Wage employment		Monthly earnings		Employment length	
Estimator:	OLS	FE	OLS	FE	OLS	FE	OLS	FE
Independent variables:								
Voucher	0.0130 (0.0195)	0.0247 (0.0278)	0.0514** (0.0216)	0.0952*** (0.0303)	-173.5 (213.7)	-274.0 (394.0)	3.124 (16.06)	46.93** (21.30)
Male	0.0741*** (0.0197)		0.110*** (0.0227)		461.6* (246.0)		36.50** (15.32)	
Earners in household 2009	-0.000105 (0.00196)	-0.000355 (0.00634)	-0.000740 (0.000738)	-0.000195 (0.00692)	1.332 (7.298)	6.083 (58.50)	0.112 (0.553)	0.0742 (4.729)
Controls for school:	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sampling cluster fixed effects:	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,862	1,862	1,862	1,862	1,862	1,862	1,803	1,803
R-squared	0.085	0.116	0.115	0.156	0.038	0.012	0.137	0.205
Number of matched pairs		1,386		1,386		1,386		1,352

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 4. Impacts on wage employment taking into account the understanding of the voucher and understanding the voucher– one year after voucher allocation (2011)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable:	Wage employment									
Estimator:	OLS	FE	OLS	FE	IV	IV-FE	IV	IV-FE	OLS	FE
Independent variables:										
Understood the voucher	0.0585*** (0.0210)	0.0800*** (0.0297)			0.0846*** (0.0294)	0.120*** (0.0378)			0.138*** (0.0312)	0.160*** (0.0479)
Used the voucher			-0.0270 (0.0219)	0.0306 (0.0327)			0.0999*** (0.0351)	0.127*** (0.0435)	-0.0254 (0.0381)	0.0342 (0.0598)
Understood × used the voucher									-0.108** (0.0521)	-0.166* (0.0863)
Sampling cluster fixed	Yes		Yes		Yes		Yes		Yes	
Observations	2,358	2,358	2,285	2,285	2,358	2,358	2,285	2,285	2,285	2,285
R-squared	0.067	0.083	0.066	0.084	0.066	0.080	0.053	0.0722	0.075	0.100
Number of pairs		1,598		1,576		1,598		1,576		1,576

All regressions control for gender, number of earners in the household in 2009 and education level

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 5. The impact of the voucher on employment, through firm take-up or enquiry (2011)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Wage employment					
Estimator:	OLS	FE	OLS	FE	OLS	FE
Independent variables:						
Voucher	0.0480** (0.0188)	0.0632*** (0.0236)	0.0502*** (0.0187)	0.0669*** (0.0235)	0.0479** (0.0188)	0.0629*** (0.0236)
Firm enquired about voucher	0.200** (0.0860)	0.348*** (0.133)			0.169 (0.124)	0.280 (0.185)
Worker subsidized			0.206* (0.108)	0.359** (0.164)	0.0562 (0.159)	0.120 (0.228)
Observations	2,358	2,358	2,358	2,358	2,358	2,358
R-squared	0.070	0.095	0.069	0.092	0.070	0.095
Number of pairs		1,598		1,598		1,598

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 6. The impact of the voucher on employment two years after allocation, through firm take-up or enquiry (2012)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Wage employment					
Estimator:	OLS	FE	OLS	FE	OLS	FE
Independent variables:						
Voucher	0.0422* (0.0217)	0.0816*** (0.0308)	0.0477** (0.0217)	0.0875*** (0.0307)	0.0423* (0.0218)	0.0817*** (0.0308)
Firm enquired about voucher	0.266*** (0.0845)	0.350** (0.157)			0.360*** (0.104)	0.441* (0.263)
Worker subsidized			0.174 (0.114)	0.275 (0.180)	-0.160 (0.144)	-0.130 (0.301)
Observations	1,865	1,865	1,865	1,865	1,865	1,865
R-squared	0.077	0.125	0.073	0.120	0.078	0.125
Number of pairs		1,387		1,387		1,387

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 7. Impact of the voucher on search (2011)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	Searched more after interview				Approached more firms after interview			
Estimator:	OLS	IV	FE	FE-IV	OLS	IV	FE	FE-IV
Independent variables:								
Voucher	-0.0356* (0.0207)		-0.0407 (0.0296)		-0.00769 (0.0233)		-0.0355 (0.0343)	
Understood voucher		-0.0520* (0.0304)		-0.0613 (0.0448)		-0.0112 (0.0340)		-0.0531 (0.0516)
Observations	1,852	1,852	1,852	1,852	1,826	1,826	1,826	1,826
R-squared	0.036	0.032	0.098		0.053	0.052	0.110	
Number of pairs			1,383	1,383			1,372	1,372

Regressions control for gender, education, wage employment in 2010, the number of earners in the household in 2009 and sampling cluster fixed effects. In the IV regressions understanding the voucher is instrumented with allocation to the treatment group.

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 8. Impact of the voucher on search (2011)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	Changed search approach				Searched most intensively one month after the interview			
Estimator:	OLS	IV	FE	FE-IV	OLS	IV	FE	FE-IV
Independent variables:								
Voucher	0.0233 (0.0177)		0.0214 (0.0261)		-0.000902 (0.0168)		-0.00503 (0.0214)	
Understood voucher		0.0341 (0.0259)		0.0325 (0.0399)		-0.00142 (0.0264)		-0.00814 (0.0346)
Observations	1,871	1,871	1,871	1,871	2,355	2,355	2,355	2,355
R-squared	0.041	0.037	0.114		0.030	0.030	0.049	
Number of pairs			1,392	1,392			1,598	1,598

Regressions control for gender, education, wage employment in 2010, the number of earners in the household in 2009 and sampling cluster fixed effects. In the IV regressions understanding the voucher is instrumented with allocation to the treatment group.

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 9. Impact of the voucher on movement and declining a job offer (2011)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	Moved				Turned down a job offer			
Estimator:	OLS	IV	FE	FE-IV	OLS	IV	FE	FE-IV
Independent variables:								
Voucher	0.00429 (0.0120)		-0.00602 (0.0134)		-0.00844 (0.00857)		-0.0272** (0.0114)	
Understood voucher		0.00674 (0.0188)		-0.00974 (0.0217)		-0.0131 (0.0133)		-0.0438** (0.0183)
Observations	2,355	2,355	2,355	2,355	2,324	2,324	2,324	2,324
R-squared	0.196	0.196	0.160		0.050	0.051	0.083	
Number of pairs			1,598	1,598			1,585	1,585

Regressions control for gender, education, wage employment in 2010, the number of earners in the household in 2009 and sampling cluster fixed effects. In the IV regressions understanding the voucher is instrumented with allocation to the treatment group.

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 10. Heterogeneous treatment effects (2011)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable:	Turned down a job offer	Wage employment	Ln(monthly earnings)	Searched more	Approached more firms	Changed search method	Moved
Estimator:	FE	FE	OLS	FE	FE	FE	FE
Independent variables:							
Households containing employees – treatment	0.00834 (0.0250)	0.0341 (0.0516)	0.0731 (0.0741)	-0.0192 (0.0643)	0.0413 (0.0742)	0.0660 (0.0565)	-0.0253 (0.0298)
Households with no employees – treatment	0.0335 (0.0317)	0.0692 (0.0654)	-0.00332 (0.0947)	-0.0231 (0.0807)	0.113 (0.0922)	0.0312 (0.0712)	-0.0184 (0.0378)
Households containing employees – control	0.0516* (0.0273)	-0.0424 (0.0563)	0.0804 (0.0764)	0.0179 (0.0698)	0.123 (0.0803)	0.0466 (0.0614)	-0.0217 (0.0325)
Observations	2,327	2,358	1,054	1,855	1,829	1,874	2,358
R-squared	0.089	0.087	0.155	0.093	0.116	0.114	0.156
Number of pairs	1,585	1,598		1,383	1,372	1,392	1,598

Regressions control for gender, education, the number of earners in the household in 2009 and sampling cluster fixed effects. The base category is individuals in the control group in households with no earners in 2010.

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 11. Attrition

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Attrition in 2011		Attrition in 2012		
Estimator:	OLS	FE	OLS	FE	FE
Independent variables:					
Treatment	0.00239 (0.0148)	0.00600 (0.0165)	-0.0235* (0.0130)	-0.0337** (0.0159)	-0.0570** (0.0223)
Wage employment _{t-1}	0.0242 (0.0188)	0.0192 (0.0298)	0.0434*** (0.0161)	0.0749*** (0.0266)	0.0406 (0.0352)
Wage employment _{t-1} × treatment					0.0665 (0.0448)
Wage employment _{t-2}			0.0147 (0.0187)	0.0130 (0.0295)	0.0126 (0.0294)
Male	-0.0171 (0.0153)		0.0247* (0.0131)		0.0665 (0.0448)
Earners in household - 2009	0.000720 (0.00124)	0.00192 (0.00241)	-2.21e-06 (0.000492)	-0.00281 (0.00224)	-0.00277 (0.00224)
Grade 11	-0.0663** (0.0296)	-0.00516 (0.0458)	-0.00801 (0.0272)	0.00501 (0.0570)	0.00657 (0.0569)
Matric without endorsement	-0.0834*** (0.0278)	0.0119 (0.0547)	-0.0312 (0.0246)	-0.0803 (0.0647)	-0.0777 (0.0647)
Matric with endorsement	-0.0473 (0.0335)	0.0479 (0.0622)	0.0166 (0.0299)	-0.0382 (0.0693)	-0.0371 (0.0693)
Observations	3,057	3,057	2,355	2,355	2,355
R-squared	0.079	0.050	0.037	0.100	0.103
Number of pairs		1,852		1,598	1,598

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Appendix A: Sample design and descriptive statistics

Table A1. Sample size and composition

	N	Attrition (% of previous round remaining)	Proportion in:	
			Control	Treatment
Total sample				
2009	4,009		50.29	49.71
2010	3,064	76%	49.25	50.75
2011	2,358	77%	49.28	50.72
2012	1,866	79%	48.15	51.85
EA sample				
2009	2,567		50.25	49.75
2010	1,860	72%	48.71	51.29
2011	1,367	73%	48.87	51.13
2012	1,058	77%	47.16	52.84
LC sample				
2009	1,442		50.35	49.65
2010	1,204	83%	50.08	49.92
2011	991	82%	49.85	50.15
2012	807	81%	49.44	50.56

Descriptive statistics

For a number of key covariates the sample is balanced between the treatment and control groups across all rounds. The only variables and years where there is a significant difference between the two groups is the proportion of the sample in Gauteng in 2010, where there are relatively fewer people in the treatment group, and the consequent increase in the proportion who are in the Limpopo province in the same year.

The increasing proportion of individuals with matric, the South African school leaving qualification, is also evident in the later rounds as individuals finish school over the course of the study.

Table A2. Descriptive statistics of covariates and sample balance

	Round	n	Proportion Mean	Proportion in:		T-test p-value
				Control	Treatment	
Male	2009	4009	0.45	0.45	0.45	0.88
	2010	3064	0.43	0.43	0.43	0.99
	2011	2358	0.44	0.43	0.44	0.56
	2012	2107	0.43	0.42	0.43	0.67
Matric	2009	4009	0.58	0.58	0.58	0.67
	2010	3064	0.65	0.65	0.66	0.57
	2011	2358	0.71	0.70	0.72	0.32
	2012	2107	0.72	0.72	0.72	0.94
Proportion in Gauteng	2009	4009	0.48	0.48	0.48	0.98
	2010	3064	0.49	0.51	0.47	0.02
	2011	2358	0.50	0.51	0.49	0.43
	2012	1762	0.50	0.49	0.50	0.81

Table A2. Descriptive statistics of covariates and sample balance, continued

	Round	n	Proportion		Proportion in:		T-test p-value
			Mean	Std Dev	Control	Treatment	
Proportion in KwaZulu-Natal	2009	4009	0.25		0.25	0.25	0.95
	2010	3064	0.21		0.21	0.22	0.52
	2011	2358	0.17		0.17	0.16	0.43
	2012	1762	0.16		0.16	0.15	0.62
Proportion in Limpopo	2009	4009	0.27		0.27	0.27	0.98
	2010	3064	0.30		0.28	0.31	0.06
	2011	2358	0.31		0.29	0.32	0.10
	2012	1762	0.31		0.31	0.32	0.54
Proportion in another province	2009	4009	0.00				
	2010	3064	0.00		0.00	0.00	0.63
	2011	2358	0.03		0.03	0.03	0.62
	2012	1762	0.03		0.03	0.02	0.27
			Mean	Std Dev			
Number of earners (current)	2009	4009	1.42	4.73	1.50	1.34	0.29
	2010	3064	1.77	6.47	1.60	1.94	0.15
	2011	2358	2.03	8.09	1.88	2.17	0.39
	2012	1763	1.25	1.14	1.23	1.26	0.60

Table A3. Descriptive statistics of potential outcome variables and sample balance

	Round	n	Mean	Std Dev	Proportion in:		T-test p-value
					Control	Treatment	
Labour force participation	2009	4009	0.74		0.75	0.74	0.408
	2010	3057	0.81		0.81	0.80	0.572
	2011	2358	0.76		0.76	0.76	0.816
	2012	1866	0.76		0.76	0.77	0.537
Education (school and other studying)	2009	4009	0.13		0.13	0.13	0.398
	2010	3057	0.13		0.13	0.13	0.586
	2011	2358	0.09		0.10	0.08	0.248
	2012	1866	0.11		0.11	0.10	0.530
Wage employment	2009	4009	0.09		0.09	0.09	0.907
	2010	3057	0.20		0.19	0.22	0.060
	2011	2358	0.34		0.31	0.37	0.003
	2012	1866	0.33		0.30	0.35	0.015
Monthly earnings (Rand)	2009	4009	291	1,546	283	299	0.755
	2010	3064	776	2,162	734	817	0.286
	2011	2358	1,501	3,166	1,450	1,550	0.445