South Africa's Youth Unemployment and the Employment Tax Incentive: An Empirical Re-evaluation

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Abstract

Our paper uses a Difference-in-Difference estimator to investigate the impacts of the Employment Tax Incentive after the first year of implementation. We use birth cohort bands to track the difference in employment probabilities of subgroups of targeted workers and untargeted workers. We find evidence of improvements in the employment prospects of young workers in the region of 2 percentage points. Our results were largest for African males. We then perform a placebo and find no evidence that differences in employment probabilities of younger workers and older workers were present before implementation. Possible deadweight loss, displacement and measurement effects limit the extent to which we can attribute changes in employment probabilities to evidence that "new" jobs were "created" by the programme.

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"No one in society remains untouched by the situation of high unemployment. For the unemployed themselves, it is often a tragedy which has lasting effects on their lifetime income."

Mario Draghi

1. Introduction

South Africa's stubborn unemployment continues to be a major socio-economic challenge despite several policy interventions. In the third quarter of 2016, the South African unemployment rate was recorded at 27.1%[2] (Statistics South Africa, 2016). South Africa has one of the highest unemployment rates in the world. The average unemployment rate for OECD member countries was recorded at 7% in 2015 (OECD, 2016) All government economic policies since the dawn of democracy in 1994 have had an objective of reducing unemployment. Government's most recent policy framework, the National Development Plan, aims to reduce unemployment to 6% by the year 2030 (National Planning Commission, 2016).

South African youth[3] are disproportionately affected by the unemployment problem. Youth unemployment is a global phenomenon but South Africa's youth unemployment problem is much more disturbing than in peer countries. According to Statistics South Africa (2016), 54.2% of South Africans aged between 15 and 24 years were unemployed in the first quarter of 2016. For South Africans aged 24 to 35 years, the unemployment rate was recorded at 32.1%.

In January 2014, The National Treasury of South Africa implemented South Africa's most expensive and most direct labour market intervention to date. The intervention came in the form of the Employment Tax Incentive Bill (ETI). The ETI is a tax incentive that firms that are registered for Pay-As-You-Earn (PAYE)[4] tax can claim when they hire a worker aged between 18 and 29 years. The ETI aims to remove some of the labour market imperfections that make it difficult for young South Africans to find employment. National Treasury (2011) projected that the ETI would create 178 000 new jobs over three years. National Treasury (2016) reported that the speed and timing of the ETI lead to more jobs than anticipated and estimations of jobs created by the ETI would be revised upwards. The ETI claims had reached R6.06 billion by the year 2014. With challenges in funding for higher education, a worrying budget deficit and sluggish economic growth that continues to limit South Africa's fiscal space, the National Treasury has tough choices to make concerning expenditure priorities. Whether the ETI is effective or not is not only important for addressing South Africa's high youth unemployment but it also has important implications for the fiscal stance of government.

National Treasury (2016) reported that 645 973 ETI claims had been processed by the year 2014. According to National Treasury (2016), this "supported" 5% of all jobs in the tax base and 17% of jobs for workers aged between 18 and 29 years. Our paper investigates the short-run employment impacts of the ETI. Ranchhod and Finn (2014, 2016) provide the most robust analysis of the short-run impacts of the ETI during its first year of implementation. However, we argue that their paper has three shortfalls. Firstly, Ranchhod and Finn (2014; 2016) use 2014 Quarter 1 as the beginning of the treatment period while firms could claim from the ETI for eligible workers hired from 2013 Quarter 4. Secondly, their paper focuses on the impacts on formal employment even though the ETI was only targeted at the

[2] In this paper, unemployment is based on the narrow definition of unemployment where discouraged workers are excluded from the unemployment count. This definition is however potentially misleading. For more on this issue, the reader can refer to Posel, Casale, and Vermaak (2014). According to Gordhan (2017), the expanded unemployment rate for South Africa was 35% in 2016

[3] In this paper, youth is defined in accordance with National Treasury and Stats SA as persons aged between 18 and 29 years inclusive.

[4] PAYE refers to the tax required to be deducted by an employer from an employee's remuneration paid or payable.

formal *private* sector. Lastly, the authors use age bands to define the treated group in their Difference-in-Difference (DID) estimation.

Our contribution is therefore threefold. Firstly, we evaluate the employment effects of the ETI from 2013 Quarter 4. While the South African labour market is rigid, it is erroneous to cut the treatment period by a full quarter. The introduction of the ETI had been circling the news for several years before it was implemented. If there were early bird firms who hired young workers and claimed from the ETI within the first quarter of implementation, omitting this quarter would lead to underestimating the effects of the ETI. Moreover, omitting the first quarter reduces the treatment period from five quarters to four quarters (20%) and this may lead to biased estimates. Secondly, we focus our analysis on the formal *private* employment since the ETI was targeted at private firms registered to PAYE. Finally, and most importantly, we use birth cohorts instead of age bands to track the treated subgroups. Birth dates are time invariant whereas ages are time variant. Thus, using birth cohorts allows us to track the same treated workers over time.

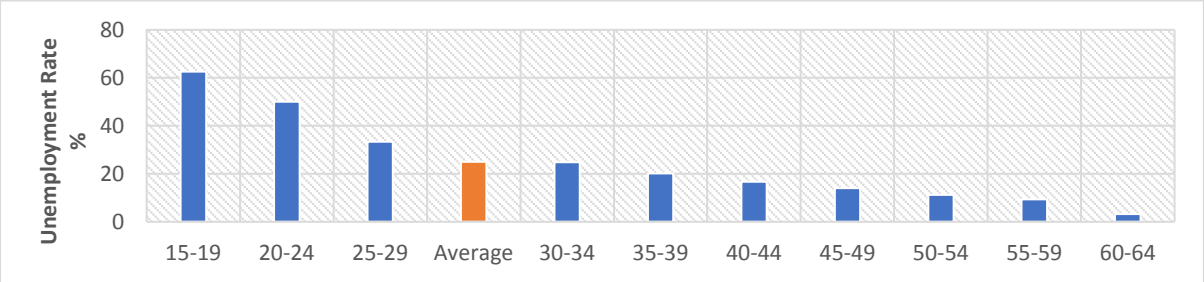
The rest of the paper is organized as follows; Section 2 canvasses stylized facts about South Africa's youth unemployment and explores the major arguments for and against the ETI. Section 3 offers a review of South African and international literature on wage subsidy programmes. Section 4 explains our data and variables while section 6 captures our methodology. We report and discuss our results in Section 6. In section 7, we discuss policy implications of our findings and finally section 8 summarizes and concludes our paper.

2. Background

2.1 Stylized facts about South Africa's ETI

Figure 1 below shows that South Africa's young workers are disproportionally affected by the unemployment problem compared to older workers.

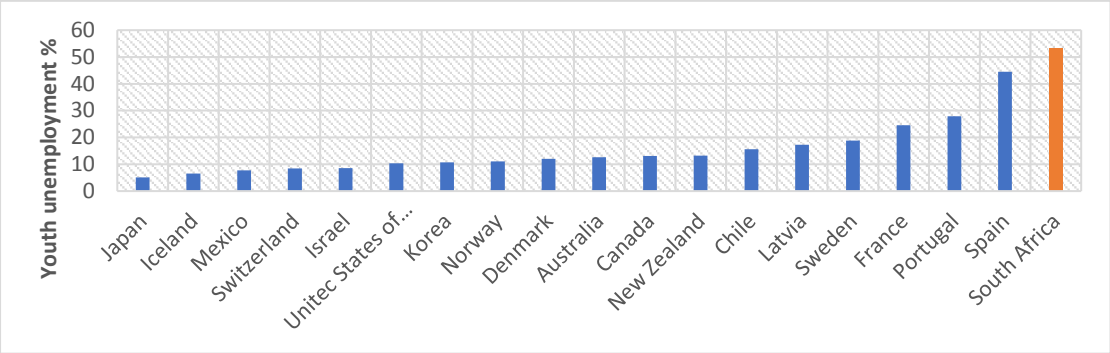
Figure 1: South Africa's unemployment rate by age subgroups 2015



Source: OECD

Figure 2 below offers a comparison of South Africa's youth unemployed rate in 2015 with that of OECD countries. While OECD countries represent mostly the advanced economies of the world, they still offer a useful benchmark as to where South Africa must aim for in the long run. The OCED average unemployment rate for workers aged 15-24 years was recorded at 14% in 2016 (OECD, 2016). Therefore, although youth unemployment problem is a global phenomenon, in South Africa the problem is much more disturbing.

Figure 2: Youth unemployment rates in 2015



Source: OECD

There are similarities in patterns of youth unemployment and overall employment in post-apartheid South Africa. Since 1994, South Africa's labour markets have experienced a large influx of workers from demographic groups that were marginalized from economic participation during the apartheid regime (Hodge, 2009; Kingdom and Knight, 2007). This influx was mainly made up of unskilled African workers. All the while, the labour demand for typically unskilled workers has been declining as sectors such as agriculture and mining have been experiencing sluggish growth (Banerjee *et al*, 2008).

From a macroeconomic perspective, hysteresis could partly explain why South Africa has such a high level and stubborn level of youth unemployment. Hysteresis in physics refers to the extent to which an object is unable to return to its original position after an external force is removed. Ball and Mankiw (2002) and Blanchard and Summers (1986) argue that a form of hysteresis exists in labour markets. Firstly, if workers remain cyclically unemployed for an extended period, their skills may deteriorate i.e. they may become "rusty", compounding to the difficulty the of finding employment. This would in time increase the level of structural unemployment. Secondly hysteresis could manifest in the labour market through the insider-outsider problem. Wages are set by members of the labour force that are employed, the insiders. Assuming self-interest, hysteresis manifests due to the fact that the optimal wage set depends on the number of insiders, and the number of insiders depends on past employment (Blanchard and Summers, 1986; Røed 2002). Hence a shock that reduces the number of insiders in one period may raise the optimal wage in subsequent periods[5] (Blanchard and Summers, 1986; Røed 2002).

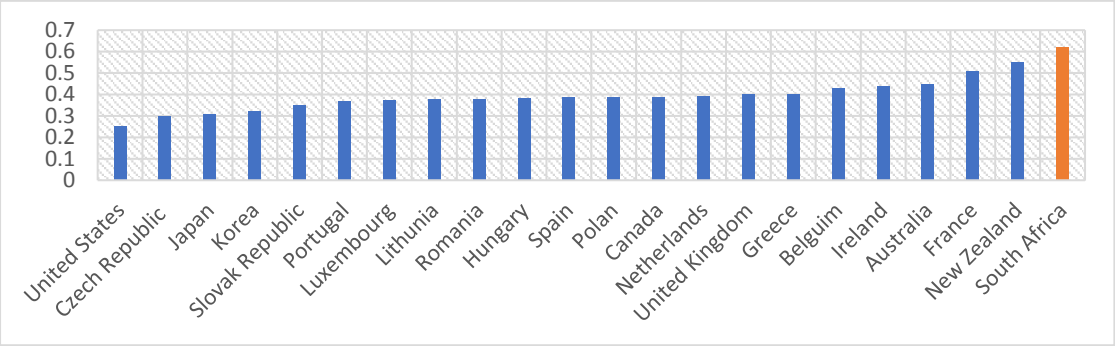
South Africa's stubborn youth unemployment problem is complex and multifaceted. Literature outlines three major challenges that confront South Africa's young workers. Firstly, low skills and high entry level wages create a barrier to labour market entry (National Treasury, 2011). Levinsohn (2007) observes that externalities in the form of unions that extend wage settlements to non-union sectors push up wages[6]. Levinsohn (2007) further argues that wages in South Africa are too high to clear labour markets[7] (See Figure 3 below).

[5] A successful wage subsidy would theoretically lower the equilibrium wage in the labour market to a wage lower than the optimum wage set by insiders, making it easier for outsiders to penetrate the labour market.

[6] The wage effects of bargaining council decisions have received much attention in South Africa's labour market literature. For more on this subject the reader can refer to Butcher and Rouse (2001), Bhorat et. al. (2001) and Magruder (2011)

[7] The Congress of South African Trade Unions argues that South African wages are too low (Coleman, 2014). The South African government has recently implemented a policy intervention that is the direct opposite of a wage subsidy. Deputy President, Cyril Ramaphosa recently signed a minimum wage bill of R3 500 monthly for all formal employment to be implemented from May 2018

Figure 3: Ratio of minimum wage to average wage of full time workers



Source: OECD as quoted in National Treasury (2011)

Secondly, relatively high dismissal costs make South African labour markets rigid. South African firms therefore always opt for the worker with the highest expected quality since experimenting with unskilled workers is too costly (Levinsohn, 2007). These labour market rigidities continue to grow. In 2014 amendments to the Labour Relations Act were passed that extended the rights of temporary and contract workers. Thirdly, Levinsohn (2007) argues that when faced between young workers and older workers, firms are likely to hire older workers at the expense of younger workers since younger workers typically require education and training. Therefore, there are disparities and costs of hiring younger workers and older workers. And in addition to these disparities in hiring costs, there are disparities in the return on hiring younger workers compared to older workers. Older workers are typically more experienced and more productive (Levinsohn, 2007). Finally, young South Africans who only have high school education do not have a productivity signal to offer potential employees due to South Africa's poor education system (Levinsohn, 2007). This compounds to the uncertainty and risk that comes with hiring young South Africans.

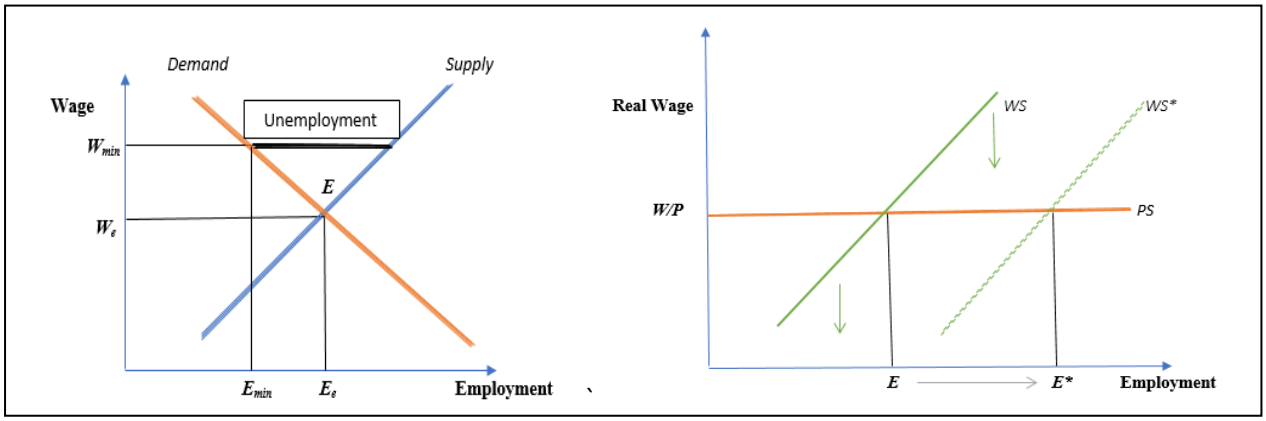
Wage subsidy programmes are the most widely used form of Active Labour Market Programmes (ALMP's) and have been used in advanced and developing economies alike. Until 2014, the South African government had relied on the Expanded Public Works Programme (EPWP) as a labour market intervention. National Treasury (2011) reports that the EPWP provided 1.6 million South Africans with jobs during its first phase. However, the EPWP is not sustainable in that it only provides a temporary lift out of poverty. The jobs are short term and the skills are often not transferrable to formal private sector jobs. In 2014 the South African government implemented its first wage subsidy programme to combat youth unemployment. According to National Treasury (2011), the wage subsidy addresses labour market imperfections through three major channels;

(a) **Risk discount:** The wage subsidy compensates an employer for the costs and risks that are associated with hiring a young worker whose productivity is uncertain. In the words of (Bordos, Csillag and Scharle ,2015), it provides a "risk discount''

(b) **Training compensation:** The wage subsidy compensates an employer for the training and up skilling costs that firms incur when they hire a young unskilled worker

(c) **Labour market participation:** Finally, the wage subsidy improves the hope and confidence among the youth regarding employment prospect. (Bordos, Csillag and Scharle ,2015) argue that subsidy programmes can increase the job search efforts of the targeted workers since it gives them the expectation that their success rate in the labour markets are higher.

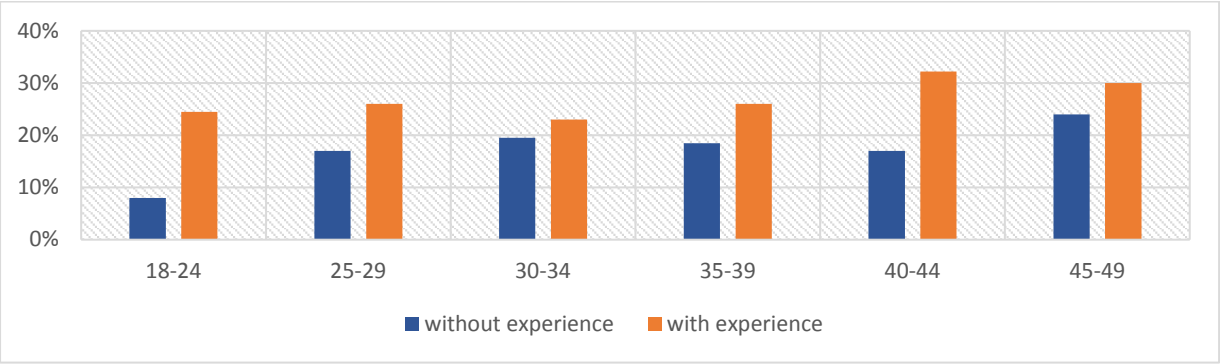
Figure 4: A simple labour market model
Source: Authors' illustration



Consider Figure 4 above. Initially the labour market is settling at a high wage rate, W_{min}. At this going wage rate the labour market is unable to clear. The excess labour supply unemployment is the unemployment. If we assume that other labour market factors remain constant, the wage subsidy lowers wages from W_{min} to W_e. W_e is the labour market clearing wage. The decrease from W_{min} to causes the excess labour supply to be absorbed by firms. Employment increases from E_{min} to E_e, the equilibrium level of employment. The government bears the burden of the difference between wages W_{min} and W_e. The cost of hiring the targeted worker has decreased but the wage rate remains the same. In the simple WS-PS model illustrated above, the labour market initially clears at the level of equilibrium E. Again, if we assume that other market factors remain constant, the wage subsidy shifts the WS curve downwards. The market then clears at a higher level of unemployment E*. This is a demand side remedy

On the demand side, National Treasury (2011) argues that probabilities of exiting unemployment are higher for workers with experience (See Figure 5 below).

Figure 5: Unemployment exit rates with and without experience



Source: National Treasury as quoted in Statistics South Africa, Labour Force Survey panel data

Hence National Treasury (2011) argues that by giving young South Africans higher chances of finding that first job, the subsidy consequently gives youth the opportunity to move to other jobs using skills attained in that first job. This is a remedy to the supply side of the labour markets. So even though the ETI is a short run intervention in that an employer is only required to employ the subsidized worker for 24 months, a successful subsidy programme can impact the supply side in the long run. National Treasury (2011) further argues that by lowering costs associated with hiring young workers while leaving workers' wages unchanged. The targeted wage subsidy programme not only reduces costs of labour relative to capital, but it also reduces labour costs of the targeted group relative too other groups

of workers while leaving wages unchanged. The targeted wage subsidy programme therefore makes targeted workers more attractive to firms by addressing the challenges from the demand side of the labour markets.

Bördös, Csillag and Scharle (2015) also argue that the gain in employment probabilities of the targeted group can persistent in the long run beyond the expiry date of the subsidy. But this hinges on two factors. Firstly, if productivity uncertainties were the only obstacle then the subsidy provides a screening report for the employer where the employer can ascertain the productivity level of the worker. Secondly, through learning by doing, the workers gain experience that helps them with future job searches. In summary, the theoretical expectation is that wage subsidy programmes affect employment both at the extensive margin (the number of different types of subsidized employees hired) and at the intensive margin (the number of hours demanded from currently employed subsidized employees) (Kristiina, Pirttilä and Uusitalo, 2013). The intensive margin depends on the how the wage subsidy affects marginal pay of already employed workers (Kristiina, Pirttilä and Uusitalo, 2013).

2.2 The pitfalls of wage subsidy programmes

Wage subsidy programmes have potential pitfalls that make them susceptible to criticism. Levinsohn (2007) explains that the subsidy favours targeted workers over untargeted workers. The untargeted worker population has two sub populations, those who are employed and those who are not employed. We therefore distinguish between two kinds of displacement effects. The first displacement effect is what authors such as Bördös, Csillag and Scharle (2015) and COSATU (2013) refer to as the "substitution" effect. This displacement effect occurs when a subsidized worker is hired at the expense of an older worker (untargeted) employed worker, i.e., the subsidized worker is "substituted" for an older worker. In this instance, firms do not increase their total employment in response to the subsidy but instead hire a subsidized worker by firing an ineligible worker who is most similar in their characteristics. Hence firms merely reshuffle the pool of the unemployed instead of absorbing parts of it. Levinsohn (2007) argues strict labour market regulations in South Africa make dismissals difficult and hence it is unlikely that substitution of the subsidized worker for employed workers will be significant. National Treasury (2011) argues that differences in productivities of younger workers and world workers would prevent older workers from being substituted for younger workers[8].

The second displacement effect occurs when a new employment opportunity arises and a younger worker is hired instead of an older worker (untargeted unemployed worker). This actually the main of the ETI, to give young workers an opportunity for that first job so that they may find it easier to penetrate into labour markets. Levinsohn (2007) argues that this displacement effect should not be a concern since the fact that the pool of unemployed young people is much larger than the pool of the unemployed older workers so a successful subsidy programme would result in an overall increase in total employment[9]. Levinsohn (2007) further argues that the absolute monetary amount of the subsidy is so small that it is unlikely for substitution to occur at low paying jobs. Furthermore, a careful design of the subsidy programme can minimize displacement effects. Furthermore, a penalty fee of R30 000 is payable to SARS if an employer is found guilty of displacing workers (National Treasury, 2011) [10].

Deadweight loss arises when a firm claims a subsidy for a young worker that the firm still would have hired without the subsidy. Econometric analysis that find correlation between better employment

[8] The inconsistency of National Treasury's argument here reveals an inherent contradiction in targeted wage subsidy programmes. On the one hand, National Treasury (2011) argues that even with the ETI, young workers would not be substitutable with older experienced workers, on the other hand, they argue that the ETI will offer firms compensation that makes younger workers comparable with older experienced workers.

[9] One notes the moral question that this point raises. Is it morally correct to increase the employment prospects of a particular sub-population at the expense of another population given that the overall employment might increase?

[10] In Denmark, measures such as requiring an employment representative to sign off the subsidy and requiring the subsidy to have a net positive impact on employment were put in place to limit substitution and displacement effects respectively

prospects for the targeted group would need to disprove deadweight loss before attributing the increase in the employment prospects of the targeted workers to the wage subsidy programme. Deadweight loss is likely to be high during the periods of good economic growth and firm expansion. As firms expand and in their production, they would also hire more workers and capture the tax incentives as rent. However, it is important to remember that this assumes that firms that expand in output expand their labor capital. Assuming substantia degrees of labour capital intensity firms would hire more labour as they expand, but if labour intensity is low then firms would opt to use more capital than labour as they expand and hence the ETI would incentivize them to substitute machinery for human labour

The ETI requires firms to keep subsidized employees for a minimum probation period of 24 months. Hence firms could hire subsidized employees and fire them on the last day of the probation period, a scenario known as "destructive churning" (Levinsohn, 2007; Levinsohn et al, 2014). Levinsohn (2007) argues that the large pool of labour supply of the targeted workers makes it easy for firms to fire and hire the subsidized workers. However, Levinsohn (2007) puts forward three reasons as to why the destructive churning will be minimum. Firstly, the author argues that it is not good business to fire good workers. Secondly training cost endured by firms make it expensive to hire and fire these young workers. Finally, even if there is significant destructive churning, the wage subsidy programme would have still achieved its objective of getting the subsidized worker that first job that enables them to penetrate further into the labour market.

Wage subsidy programmes that are targeted at the youth in particular may incentivize young people to exit education in favour joining the labour market and thus lead to positive labour market outcomes in the short run whilst being detrimental in the long run (Bordos, Csillag and Scharle, 2015; COSATU 2013). Furthermore, targeted wage subsidy programmes may lead to "stigma effects". Firms may view the targeted group as "damaged goods" since they indicate to the firm that the worker has less productive and hence the firm may avoid hiring the subsidized worker [11](Levinsohn et al 2014; Bordos, Csillag and Scharle ,2015). It is also possible for subsidized workers to take the view that the eligibility is stigmatizing them and hence they may try to hide their eligibility status. (Bordos, Csillag and Scharle ,2015).

Finally, wage subsidy programmes that cover a large proportion of the population may results in high levels of inflation (Levinsohn et al, 2014), and in countries with high levels of corruption and weak administration wage subsidy programmes are susceptible to fraud.

2.3 Defining South Africa's ETI

According to Bördös, Csillag and Scharle (2015), a wage subsidy programme may take one of three forms; (a) direct transfers to firms (hiring subsidy) or workers (wage supplements) for formal employment (b) cut in taxes or social security contributions required from firms when they hire the targeted worker, and/or (c) On the job training that have a subsidized component. South Africa's wage subsidy programme took the form of a tax relief. In January 2014, the Employment Tax Incentive Bill was implemented. National Treasury allocated R5 billion from 2014 to 2016 with the intention of "creating" 178 000 "new" jobs (National Treasury, 2011).

A firm is eligible to claim from the ETI if the firm;

- Is registered for Employees' Tax (PAYE), or must be eligible to register for PAYE
- Is not in the national, provincial or local sphere of government
- Is not a public entity or municipal entity.

A worker qualifies for the ETI if the worker;

[11] The absolute monetary level of the subsidy is important in this regard in that the level of the subsidy must compensate the firms to a point where the gain of hiring the worker exceeds the perceived costs

- Has a valid South African Identity Document
- Is aged between 18 and 29 years inclusive
- Is not a domestic worker
- Is not related or "connected" to the employer
- Is employed for at least 2 years
- Earns a wage not lower than the minimum wage in that sector in terms of wage regulating measures. If no such a minimum wage exists for such employment, then the employee must be earning at least R2000 in monthly wages.
- Earns less than R6000 per month (basic salary plus benefits and bonuses) Is newly employed on or after 1 October 2013

The amount of incentive that a firm can claim depends on the number of eligible workers hired, the duration of employment and the monthly remuneration of the worker. The incentive is computed according to Table 1 below;

Table 1: Computing the ETI

	Year 1	Year 2
Monthly Remuneration	**ETI per month during the first 12 months of employment of the eligible worker**	**ETI per month during the next 12 months of employment of the eligible worker**
R0 - R2 000	50% of Monthly Remuneration	25% of Monthly Remuneration
R 2001 - R4 000	R1 000	R500
R4 001 - R 6000	Formula: R1 000 – ((0.5 x (Monthly Remuneration – R4 000))	Formula: R500 – ((0.25 x (Monthly Remuneration – R4 000))

Source: South African Revenue Services

3 Literature Review

3.1 South Africa

The literature empirically evaluating the ETI is thin. Levinsohn et al. (2014) offered the first empirical evaluation of the youth wage subsidy. The authors used a randomized control trial where a subsidy voucher was given to a young worker and found that the subsidy increased the probability of finding employment for the young worker after two years of the subsidy. However, in their study a wage voucher was given directly to a young worker rather than firms having to claim the subsidy as a tax cut. The authors also found that the subsidy did not affect labour market participation rates, possibly meaning that young people did not drop out of education in order to gain from the subsidy.

Ranchhod and Finn (2014; 2016) offered a more robust evaluation of the impacts of the ETI. They apply DID approach to the Quarterly Labour Force Survey and test the short run effects of the ETI on total employment and formal employment. The authors find no evidence that the ETI impacted the employment prospects of young South Africans for the first six months. The authors provide an update in Ranchhod and Finn (2015) by extending the treatment period to a year and still find no evidence of employment effects associated with the ETI.

Ebrahim, Leibbrandt and Ranchhod (2017) use firm level tax data from 2012 to 2015 to investigate the impacts of the ETI on youth employment. The dataset was built through a collaboration of the South African Reserve Service, National Treasury and United Nations University. The authors employ a conditional DID methodology to study the hiring behavior of firms which claimed the subsidy matched against those who did not claim from it before and after the implementation of the subsidy. The authors

find that the estimated effects of the ETI was small and statistically insignificant for both years 2014 and 2015. Moreover, the firms that claimed form the subsidy and firms that did not claim from the subsidy had the same hiring behavior which means that all new jobs found during the period of study were because of firm expansion. This implies significant deadweight loss (Ebrahim, Leibbrandt and Ranchhod, 2017). Similar to Ranchhod and Finn (2014, 2016), a short fall of Ebrahim, Leibbrandt and Ranchhod (2017) is that they use 2014 as the beginning of the treatment period even though firms could claim from the ETI for eligible workers that were hired from October 2013.

Wage and hiring subsidy programmes have been part of the toolkit of ALMPs for the past 30 years (Bordos, Csillag and Scharle ,2015). Subsidy programmes have been implemented in various forms in both the developed and developing nations of the world. We review some of the international findings below.

3.2 Wage subsidy programmes in advanced economies

Australia: Richardson (1998) investigates the long-term employment effects of a flat rate wage subsidy that was targeted at young Australians aged between 15 and 24 years in 1984 and 1985. The authors use a Bivariate Probit analysis and estimate the effect of participation in the Special Youth Employment Programme on the employment probabilities of the youth 26 months after the subsidy had expired. The programme had a large participation and improved the average probability of having a job at some time between 8 and 13 months after the subsidy expired by 26%, between 14 and 26 months after the subsidy programme expired, the effect was still 20%. Women were the major beneficiaries of the subsidy programme

Finland: In Finland government implemented a wage subsidy that targeted low wage jobs. The subsidy started in 2006 and ended in 2010. The aim of the subsidy was to improve employment prospects of workers over 54 years of age. Huttunen *et al.* (2013) estimated the impacts of the subsidy programme through a DID approach. The authors find no evidence that the wage subsidy programme affected employment rate or wages of targeted workers. However, the subsidy did have small incremental impacts on the number of hours worked by already employed targeted workers which implies possible deadweight loss.

Germany: Jaenichen and Stephan (2011) examine the impact of targeted wage subsidies on employments for hard to place workers in Germany using Propensity Matching. The authors find that the subsidy had positive impacts on employment in Germany. According to their estimates, Three years after the start of the subsidy was introduced, treated workers saw improvements of finding regular employment by between 25 and 45 percentage points. Moreover, between 14 and 28 percentage points of treated workers would have been unemployed in the absence of the subsidy.

Slovakia: In 1992, the Slovakian government introduced a wage subsidy for workers of all ages and gender lasting for two years. Lalive, Zweimüller and van Ours (2002) used an evaluation methodology that took the possible selectivity in the inflow into programs into account and find that the exit rate of unemployment decreased by 10 % for individuals attending a training program. In contrast, the temporary wage subsidy had a significantly positive effect on the outflow out of unemployment of 8 %. The estimated correlation between the transition rate to jobs and the transition rate to training programs was very large and positive (0.63), and it is moderately positive (0.11 and 0.13), respectively for employment programs and wage subsidies (Lalive, Zweimüller and van Ours, 2002).

Sweden: The Swedish government implemented a comprehensive ALMP through a combination of training by the private sector and subsidized placements. Any unemployed worker was eligible for the subsidy programme. Meghir (2008) finds no evidence that the programme lead to changes in the labour markets.

Turkey: Betcherman *et al.* (2009) investigate the effects of two subsidy programme in economically disadvantaged areas of Turkey in 2004 and 2005. Firms could either claim a tax relief or social security subsidy. Using a DID approach, Betcherman *et al*. (2009) find that the first subsidy programme increased registered jobs by 5-11% while the second subsidy programme by 11-15%. The first subsidy programme had substantial deadweight loss of between 47% and 78% while the second subsidy was better designed and had deadweight loss between 27% and 46%. The workers argue that the major impact of the subsidy programmes was to increase the formal registration of jobs rather than to increase actual employment. This is support to the theory that firms and workers in countries with weak labour market enforcement institutions are likely to operate informally.

Another subsidy programme was targeted at young Turkish workers aged between 18 and 29 years and all female workers aged 18 and over. The subsidy programme was implemented for 2 years from June 2008 to June 2010. The Turkish government paid the subsidy by covering the employers portion of the social security payment. Balkan, Baskaya and Tumen (2015) use a DID approach to estimate the casual effects of the programme. At an aggregate level, the subsidy did not have any significant effects on employment probabilities of the targeted group. However, when the treatment group is divided into sub categories to account for heterogeneity, the results point to significantly higher employment probabilities for the subgroups. The results were strongest for older unskilled and uneducated women, increasing the employment probabilities of these women by 4.5%. Balkan, Baskaya and Tumen (2015) attribute the substantial heterogeneity to varying elasticities of labour demand across the sub groups.

United States: Fatz (1996) uses a linear DID approach to examine the employment effects of the Targeted Jobs Tax Credit aimed at young Americans from 1979 to 1994. The authors fund that private sector employment for the youth increased by 3.1 percentage points during the operation of the program. While the employer take-up rate was low, substantial wage subsidies combined with job development efforts by an intermediary did substantially expand private employment for difficult to employ poor youth.

3.3 Wage subsidy programmes in developing countries

Argentina: In Argentina, a group of workers were given a voucher that enabled firms to claim a wage subsidy that covered part of the employees' wage from government. The subsidy programme lasted between 1998 and 2000. A subgroup of the treatment group also received skills training option. Galasso, Ravallin and Salvia (2001) investigated the impacts of this ALMP on employment and income 18 months after implementation using a Difference-in-Difference and Instrument Variables approach. The authors find that recipients of the voucher enjoyed significant higher probabilities of employment but their incomes remained the same. The 2 Stage Least Squares estimated an increase in the proportion of wage employment by 7.5 percentage points. The athours found an extra impact of the training (an increment to the employment rate of 7.5% points versus 6.1% for the voucher only) to be statistically significant. Since some of the control sample were more likely to be employed in temporary employment programmes in the baseline survey 2SLS results might over-state the employment gains from ALMP. To address this concern, Galasso, Ravallin and Salvia (2001) used a DID approach and found that the effect on private employment held in the double difference estimates. There were no other significant effects on other outcomes. The close correspondence between the double- difference and single-difference results is consistent with randomized assignment. The impacts were largest for women and younger workers

Chile: The Chilean government implemented a youth employment subsidy in 2009 for both employees and employers with two separate application processes. Bravo and Rau (2013) investigate the effects of the programme on participation, employment and wages using a Regression Discontinuity Design. The authors find that the subsidy increased employment probabilities of vulnerable youth by 5 percentage points in the first six months which decreases to 1.3 percentage points after a year. According to Bravo and Rau (2013), the participation rate increased by 5 percentage points in the first six month and 2

percentage points by the end of the second year. The authors find no evidence of wage and displacement effects.

Columbia: Attanasio, Kugler, and Meghir (2008) used randomized assignments to investigate the impacts of an ALMP targeted at young workers in Columbia. The targeted young were placed in private training institutions for three months and then placed in a firm for another three months. To be eligible for the subsidy, a worker had to be aged between 18 and 25 years and had to come from a family that belonged to the lowest two deciles of the income distribution. Attanasio, Kugler, and Meghir (2008) estimated that being offered training increased the probability of employment and paid employment by 4.7 and 5.2 percentage points respectively. Women's days and hours worked and salaries also increase after being offered training by close to a day and two and a half hours.

Tunisia: In 1987, the Tunisian government implemented an employment subsidy programme to reduce unemployment among young university graduates who had no prior job experience (Broecke, 2013). Registered firms could claim the subsidy for a year as an exemption from having to pay towards social security contributions. Broecke (2013) estimates the relationship between those who participated and their labour market outcomes through an Ordinary Least Squares estimation. Broecke (2013) found that that the subsidy programme decreased the joblessness rate of university graduates by 8 percentage points but cautioned that this result could in part be a result of selection bias. Moreover, Broecke (2013) found evidence that the subsidy programme had huge deadweight losses.

The above literature therefore speaks to contrasting evidence on the effectiveness of ALMPs. It appears that most subsidy programmes last between two and three years and where they not universal they are targeted at younger workers. The subsidy programmes are always targeted at full time low paying jobs. In many of the reviews, women are found to be the major beneficiaries of wage subsidy programmes. The studies estimate stronger impacts in developed countries than in developing countries. The literature above speaks to estimates that range between 3.1 and 28 percentage points in developed countries, and a range between 1.3 and 8 percentage points in developing countries. McKenzie (2017) comprehensively reviewed empirical studies of ALMPs in developing countries. One average the estimated effects of ALMPs were around 3 percentage points albeit most studies reported effects that were not statistically significant.

4 Data and Variables

Our paper makes use of Labour Market Dynamics Panel data for 2012, 2013 and 2014 from Statistics South Africa. The data set captures labour market outcomes at a worker level on a quarterly basis with a total of 1,027,865 observations during this period.

We arrange the panel data in 12 quarters. Firms could claim from the ETI from 2013 Quarter no.4, hence this is our baseline quarter. Therefore, we define a binary variable, *post,* where post takes on a value of 1 during the treatment period and a value of 0 before treatment. In order words, our time periods are separated into; pre-treatment spanning from 2012Q1 to 2013Q3 (*post* = 0) and post-treatment (*post* = 1) spanning from 2013Q4 to 2014Q4. We also explore the DID estimation where the treatment and control groups are specified on age bands and then evaluate specifications based on birth cohort bands. Our preferred estimation is when specification is based on birth cohorts. We discuss this in more detail in section 5. Table 2 below shows the sample sizes for different widths of our treatment and control groups in the pre-treatment and post-treatment periods.

Table 2: Sample sizes by birth cohorts

Age subgroups	*post- treatment period*	*pre-treatment period*
1985 – 1987	16 055	16 038
1982 – 1984	14 885	15 038
1985 – 1988	21 506	21 667
1981 – 1984	19 389	19 813
1985 – 1989	27 840	27 221
1980 – 1984	23 840	23 840
1985 – 1990	27 960	33 349
1979 - 1984	30 323	28 771

Our model initially follows an age approach where the binary variable, *age_treatment*, refers to a targeted subgroup based on an age band. We then adopt a birth cohort approach by generating a binary variable, *cohort_treatment*, which captures a targeted subgroup based on a birth cohort band. The dependent variables in our analysis are *formal_private_employment*, *informal_employment*, *public_employment*, *total_employment* and *participation*. The *participation* variable captures labour market participation. Our control dummy variables are *province*, *geographical_type* and *education*, *race*, *gender* and *quarter*. These are variables that we would theoretically expect to impact on a worker's employment prospects. We account for heterogeneity by analyzing employment effects of the ETI on subgroups based on two demographic features; *race* and *gender*.

Table 3 below canvasses the sample weights for our covariates. A visual inspection of Table 3 shows that the difference between sample weights of our covariates before and after treatment are marginal. Females account for 53% of our sample and Africans who happen to be most acutely affected by the unemployment problem are the largest racial group. KwaZulu-Natal and Gauteng account for many of our observations with provincial shares of 17% and 14% respectively. Urban formal areas where the ETI would have had the most impact since it was targeted at the formal sector account for 52% of our sample. Close to 80% of our observations are workers who did not have tertiary education and more than 40% did not complete primary school. These are individuals who would have benefited the most from the ETI.

Table 3: Sample weights for covariates

Variable	*pre- treatment*	*post- treatment*	*entire period*
Male	46.94	46.98	46.89
Female	53.06	53.02	53.11
African	79.9	80.30	80.21
Coloured	11.31	11.19	1.94
Indian	1.96	1.95	11.12
White	6.73	6.56	6.74
Gauteng	14.74	14.56	14.63
North West	7.69	7.73	7.75
Western Cape	11.75	11.74	11.52
Free State	8.51	8.36	8.51
Eastern Cape	12.09	12.31	12.16
Northern Cape	5.68	5.50	5.59
KwaZulu-Natal	17.30	17.24	17.29
Mpumalanga	9.98	9.93	9.98
Limpopo	12.28	12.63	12.56

Urban formal	52.46	52.02	52.19
Urban informal	6.82	6.68	6.66
Tribal areas	37.46	37.99	37.9
Rural formal	3.26	3.31	3.25
No schooling	20.11	20.22	20.01
Less than primary	21.07	20.93	20.24
Primary completed	5.73	5.43	5.65
Secondary not completed	30.78	30.56	30.59
Secondary completed	15.35	15.63	15.44
Tertiary	6.39	6.57	6.43
Other	0.58	0.66	0.64

To further validate our results, we consider the trends of the treatment and control groups in the period prior to the implementation of the ETI. We split the pre-implementation period into two time periods by generating a binary variable, *pre*, which takes on a value of 1 during the year 2012 and a value of zero in the first three quarters of 2013. This enables us to perform a placebo analysis. If the ETI is associated with certain labour market developments that we can theoretically attribute to the ETI during the period of treatment, then we would expect to not observe these effects during the pre-treatment.

5 Econometric Approach

Propensity Score Matching, Randomization, Difference-in-Difference, Regression Discontinuity Design and Instrument Variables are the most commonly used methods in impact evaluation studies. Our casual estimations of the ETI is based on a DID approach. We choose the DID over other methods of impact evaluation for several reasons. Firstly, when the baseline data is available the DID relaxes the assumption of conditional selection or selection on observed characteristics required by PSM and randomization. The DID relies on the assumption that unobserved heterogeneity is time invariant and uncorrelated with the treatment over time. This assumption is weaker than conditional exogeneity (Khandker *et al.*, 2010; Kertler *et al.*, 2016). Secondly, we use a DID instead of Regression Discontinuity Design (RDD) for statistical power. Since the RDD method estimates the impact of the program around the cutoff ages, or locally, the estimate cannot necessarily be generalized to workers further away from the cutoff age: that is, where eligible and ineligible workers may not be as similar. While the DID also requires that we compare our treatment group to a control group with an age that does not depart from each other, our treatment group can be larger based on a reasonable width of our age band or birth cohort. Thus, the DID allows us more power than the RDD. Finally, we choose the DID over the IV since in our case it is difficult to find an observable exogenous variable that influences the participating in the ETI but does not influence the outcome of the ETI if participating.

The DID approach compares the changes in outcomes over time between a treatment group and a comparison group. Let us suppose that we simply estimated the before and after regression of probabilities of employment for our younger workers through a liner probability model. If there are external and internal factors that have impacted employment prospects of a young worker, our estimates of the ETI would be biased. We require two differences to resolve this. Firstly, when the same young worker is observed before and after the ETI and we measure a difference in employment probabilities for that worker, we cancel out the effect of all of the characteristics unique to that young worker that are time invariant, both observed and unobserved. We then do the same for older workers and take the difference between the before and after differences for younger workers and older workers (In the Appendix 9.1., we demonstrate how the DID is derived mathematically).

This second difference resolves the problem of the potential bias to environmental factors that might have affected both younger and older workers (Khandker *et al.*, 2010). Our outcome of interest which we denote *y* is thus *the change in employment probabilities* for young workers relative to older workers during the treatment period. Our initial outcome of interest is the difference in probabilities of finding

formal private employment. We then change the *y* variable to evaluate the impact of the ETI on other types of employment.

Our DID estimator is derived from a standard linear parametric model with two time periods: pre-treatment and post-treatment. The *post* binary variable captures our treatment period variable and thus takes on a value of 1 in the post-treatment period and a value of 0 in the pre-treatment period. The binary variable *age_treatment* captures our treatment group. Our model takes the following form,

$$y = \beta_0 + \beta_1 post + \beta_2 age\_treatment + \beta_3 (post * age\_treatment) + \theta \underline{\mathbf{Z}} + \varepsilon \quad \ldots. (1)$$

where and ε is a random unobserved "error" term which contains all determinants of *y* which our model omits and $\underline{\mathbf{Z}}$ represents the vector of our control variables. The coefficients $\beta_0, \beta_1, \beta_2, \beta_3$, are all unknown parameters. The coefficient β_0 captures a constant term while the coefficient β_1 captures the time trend common to control and treatment groups. The coefficient β_2 represents the treatment group specific effect. It accounts for average permanent differences between treatment and control. The coefficient of interest is β_3, the coefficient of the interaction term. This coefficient captures the true effect of treatment. If β_3 is positive and statistically significant then we can attribute improvements in the employment probabilities of targeted workers to treatment.

Now there are three assumptions that must be fulfilled before applying the DID[12];

1. The model in equation (outcome) is correctly specified.
2. The expected value of the error term is zero, $E[\varepsilon] = 0$ [13]
3. The error term is uncorrelated with the other variable in the equation;
   cov (ε, *post*) = 0
   cov (ε, *age_treatment*) = 0
   cov (ε, *post, age_treatment*) = 0

The last of these assumptions is known as the parallel trend assumption. As discussed in the beginning of this section, this assumption is critical to applying the DID. If there are factors that affect the treatment group exclusively without affecting the control group, or vice versa, the DID estimate will be biased. In order to meet the parallel trend assumption, we narrow the width of our control and comparison groups. The narrower the age band or birth cohort, the more reasonable it is to assume that in the absence of the ETI the treatment group and the control group would move in tandem[14]. The ETI targeted workers between 18-29 so we begin by comparing workers aged between 27 and 29 years at the beginning of the year 2014 with workers aged between 30 and 32. Then we experiment with age bands of varying width.

When we replace the *age_treatment* binary variable in Equation 1 with the *cohort_treatment* variable, our model becomes.

$$y = \beta_0 + \beta_1 post + \beta_2 cohort\_treatment + \beta_3 (post * cohort\_treatment) + \theta \underline{\mathbf{Z}} + \varepsilon \quad \ldots\ldots (2)$$

As with the case of the age specification, we begin with a treated subgroup of young workers born between 1985 and 1987 and compare them to older untreated workers born between 1982 and 1984 and then proceed to experiment with the width of our birth cohort bands.

[12] In the Appendix 9.1., we demonstrate that if the above assumptions are met then the DID estimator will be unbiased

[13] This assumption is not difficult to fulfil since our model has the constant term β_0.

[14] To check the robustness of our DID estimator, it would be useful to econometrically test whether the parallel trend assumption holds. But there is no way to prove that the parallel trend assumption holds since we cannot observe what would have happened to the treatment group in the absence of the ETI. Our best approach is to specify our treatment and comparison group in a way that is reasonable to assume that the parallel trend assumption holds. We do this by specifying narrow band

Our placebo analysis follows is based on birth cohorts and follows a similar line of reasoning with two adjustments. In Equation 2, we replace the *post* binary variable with the *pre* and the *cohort_treatement* binary variable with *placebo_cohort_treatment* in equations 1 and 2. We then have the following two equation;

$$y = \alpha_0 + \alpha_1 pre + \alpha_2 cohort\_treatment + \alpha_3 (pre * age\_ cohort\_treatment) + \theta \mathbf{Z} + \varepsilon \quad \ldots\ldots (3)$$

The *age_cohort_treatment* captures a subgroup young workers who would have been eligible for the ETI before it was implemented in 2013 Quarter 4. As discussed in section 4, the *pre* binary variable separates our pre-treatment into two time period. The coefficient of the coefficient of the interaction term, α_3, captures the difference in employment probabilities between younger workers and older workers during the period were there was no ETI. Thus, we expect α_3 to not be statistically significant since there was no treatment during this period.

6 Results

Formal private employment

Table 3 below shows the results for equation 1 and 2 when our outcome of interest is *formal_private_employment*. The coefficient in Table 3 refers to the coefficient of the interaction term β_3. By keeping the width of the treatment group and the control group equal, we keep the size of our comparison and control groups equal.

As discussed in Section 5, in addition to the usual assumptions of OLS estimation, the DID estimation requires a parallel trend assumption. This means that "unobserved characteristics affecting program participation should not vary over time with treatment status" Khandker *et al.*(2010, p.73). In other, words we require reason to assume treatment to be the only source of variation between the treatment group and the control group must be the treatment through-out the treatment period. Now, workers who are aged 29 and 30 are very similar and hence we can attribute any differences in their employment prospects to the treatment. The same argument follows in comparing workers aged between 27 and 29 with workers aged between 30 and 32. However, the standard error increases and the power of our test decreases as our bands become larger and larger. If the band is too wide, then there are more possible sources of variation between the treated group and the control group. It then becomes difficult to attribute differences in employment prospects between the treatment and control group to the ETI.

Now, we also argue that using birth cohorts leads to more robust results than using age subgroups since birth cohorts allow us to track the same subgroup through-out the treatment period. Whereas with ages specifications, the elements of the subgroups change with time. As seen in Table 3 below, coefficients for the age specification are smaller than the coefficients for the birth cohort specification. This is possibly due to the fact that age bands specifications underestimate the effect of the ETI since treated workers fall out our specified treatment Full results for the first two age bands and first two birth cohorts are presented in Appendix 9.2. Therefore, a robust approach is one that uses a birth cohort band of a modest width. There is no rule of thumb as to what defines a birth cohort of modest width. We therefore exercise our judgement in this regard.

Table 3: Difference-in-Difference results for young workers

Age subgroups	*coefficient*	*standard error*	*P>|t|*
27-29 versus 30-32	0.0070195	0.0074145	0.344
26-29 versus 30-33	0.0089558	0.0064382	0.164
25-29 versus 30-34	0.0126877	0.0057662	0.028
24 -29 versus 30-35	*0.0067447*	*0.0052647*	*0.200*

Birth cohorts	*coefficient*	*standard error*	*P>|t|*
1985-1987 versus 1982-1984	0.016733	0.0073296	0.022
1985-1988 versus 1981-1984	0.0207894	0.0063542	0.001
1985-1989 versus 1980-1984	0.0242353	0.0056737	0.000
1985-1990 versus 1979-1984	0.0284496	0.0051735	0.000

The coefficients for our broadest bands for young workers, African males and females are relatively large and have lower p-values in comparison to coefficients from narrower bands. According to National Treasury (2016), during the year 2014 ETI claims were concentrated in jobs for the youngest of youth workers. Figure 3 below shows the distribution of ETI claims across ages.

Figure 6: Age distribution of new jobs for the 2013/14 and 2014/15 tax years

Source: National Treasury (2016)

The ETI was claimed for 15 % of all jobs for young workers aged between 18 and 29. Jobs that went to 18-year-old workers received claims of 32% while 10% of jobs that went to 28 year-old workers received ETI claims. It is therefore possible that we are picking up stronger results in the widest cohort bands because broader bands allow us to capture subgroups that benefited the most from the programme but it is difficult to make accurate inferences for these wide cohort bands because we are likely to violate the parallel trend assumption required for DID estimation. The wider the band, the more likely unobserved characteristics might be influencing our outcomes.

Table 4 below shows that workers born between 1985 and 1988 were 2.08 percentage points more likely to find a job in the formal private sector than their counterparts born between 1981 and 1984. We then control for heterogeneity by focusing on demographic subgroups. Africans are acutely affected by the unemployment problem. Thus, our subgroups are African males and females. Table 3 below shows our results. The impact of the ETI on formal private employment seems to be largest for African males. The prospects of finding employment in the formal private sector for African males born between 1985 and 1988 increased by 3.63 percentage points in comparison to African males born between 1984 and 1981 during the treatment period. This increase is statistically significant at 5%. For African females, we find no evidence of the ETI's impact on prospects of finding formal private employment. The heterogeneity between subgroups could in part be a result of different elasticities of labour demand (Balkan, Baskaya and Tumen, 2015)

Table 4: Difference-in-Difference results for young African males

Males			
Age subgroups	***coefficient***	***standard error***	***P>|t|***
27-29 versus 30-32	0.0307553	0.0127232	0.016
26-29 versus 30-33	0.0282458	0.0110279	0.010
25-29 versus 30-34	0.0377369	0.009868	0.000
24 -29 versus 30-35	0.0280384	0.0090189	0.002
Birth cohorts	***coefficient***	***standard error***	***P>|t|***
1985-1987 versus 1982-1984	0.0268006	0.0125134	0.032
1985-1988 versus 1981-1984	0.036288	0.0108753	0.001
1985-1989 versus 1980-1984	0.0421244	0.009707	0.000
1985-1990 versus 1979-1984	0.0464771	0.0088455	0.000
Females			
Age subgroups	***coefficient***	***standard error***	***P>|t|***
27-29 versus 30-32	0.007811	0.0104207	0.0940
26-29 versus 30-33	- 0.0526232	0.0090543	0.797
25-29 versus 30-34	0.0008514	0.0080965	0.916
24 -29 versus 30-35	0.0007139	0.0073725	0.923
Birth cohorts	***coefficient***	***standard error***	***P>|t|***
1985-1987 versus 1982-1984	0.0120983	0.0102652	0.239
1985-1988 versus 1981-1984	0.0087012	0.0088626	0.326
1985-1989 versus 1980-1984	0.0107136	0.0079058	0.175
1985-1990 versus 1979-1984	0.0454771	0.0088455	0.000

Public employment, informal employment and total employment

If the labour market improvements that we observe for young African males are a result of the ETI, then we would expect them to be concentrated in formal private sector. In other words, we have more evidence that these improvements are *caused* by the ETI if no such improvements exist in sectors that were not targeted. Table 5 shows our results when we estimate the impact of the ETI on *public_employment*, *informal_employment* and *total_employment* for young African males using birth cohorts. *Public_employment* encompasses working for government at a national, provincial or local level and working for a state-owned enterprise such as Eskom. Such employees were not eligible for the ETI. Only firms registered for PAYE were eligible for the ETI so informal employers could not claim. When we compare African males born between 1985 and 1988 to their counterparts born between 1981 and 1984, we find no evidence of changes in prospects of finding a job in the public sector and informal sector. The coefficient of the interaction term, α_3, has a positive sign but it is not statistically different at 5%.

Again, for the broader birth cohort bands, *public_employment* and *informal_employment* coefficients are significant are statistically significant at a 10% significance level. As under formal employment above, we avoid drawing inferences from broader bands since we are more likely to violate the parallel trend assumption. Furthermore, our results for formal targeted employment have consistently larger coefficients than other types of employments across all cohort bands. The ETI is therefore having the "biggest" impact in the formal employment sector where it was targeted. Additionally, it is possible that our results for informal employment and public employment are driven by measurement error.

Table 5: Difference-in-Difference results for African males

public employment	***coefficient***	***standard error***	***P>|t|***
1985-1987 versus 1982-1984	0.0112778	0.0115302	0.328
1985-1988 versus 1981-1984	0.0111624	0.0100249	0.266
1985-1989 versus 1980-1984	0.0161287	0.0089451	0.071
1985-1990 versus 1979-1984	0.0156715	0.00814	0.054
informal employment	***coefficient***	***standard error***	***P>|t|***
1985-1987 versus 1982-1984	0.0132396	0.0115204	0.250
1985-1988 versus 1981-1984	0.0120615	0.0100128	0.228
1985-1989 versus 1980-1984	0.0167431	0.0089354	0.061
1985-1990 versus 1979-1984	0.015431	0.0081304	0.058
total employment	***coefficient***	***standard error***	***P>|t|***
1985-1987 versus 1982-1984	0.0069634	0.0124221	0.575
1985-1988 versus 1981-1984	0.01389	0.0107834	0.198
1985-1989 versus 1980-1984	0.0231167	0.0096342	0.016
1985-1990 versus 1979-1984	0.28132	0.0087744	0.001

The National Treasury (2011) argues the ETI would also improve the labour market participation of young workers. Based on our favourite cohort band of young workers born between 1985 and 1988, we find no evidence that the ETI improved the labour force participation of the African males, the major beneficiaries of the ETI[15]. In other words, while firms were more likely to employ these younger workers, job searching among these younger workers did not improve.

Table 6: DID labour participation results for young African males

participation	***coefficient***	***standard error***	***P>|t|***
1985-1987 versus 1982-1984	- 0.0042323	0.0101188	0.676
1985-1988 versus 1981-1984	0.0092934	0.0088282	0.292
1985-1989 versus 1980-1984	0.0247587	0.0079697	0.002
1985-1990 versus 1979-1984	0.0338856	0.0073573	0.000

Placebo test

Table 7 below reports the results of our placebo test for young African male workers. We expect no differences between the younger workers and older workers since there was no ETI during the pre-treatment period. The coefficient has a positive sign but is not statistically significant at 1% across all the widths our birth cohorts. This means that before the ETI was implemented, employment probabilities of targeted workers were no different from the employment probabilities of older workers. (See Appendix 9.2. for Placebo results for young Africans and young African females)

[15] We still apply our arguments for broad cohort bands from formal employment results.

Table 7: Placebo results for young African males

formal private employment	*coefficient*	*standard error*	*P>|t|*
1984-1986 versus 1981-1983	0.0211471	0.0098734	0.032
1984-1987 versus 1980-1983	0.0173833	0.0085339	0.042
1984-1988 versus 1979-1983	0.0165098	0.0076076	0.03
1984-1989 versus 1978-1983	0.0186838	0.0069361	0.007

Displacement and measurement effect

As discussed in section 3, a major concern with subsidy programmes is displacement effects. In section 3, we distinguished between two kinds of displacement effects and explained that the second type of displacement, where older employed workers are displaced with younger workers, is more problematic. We note that Levinsohn (2007) argues that high dismissal cost in South Africa make such a displacement unlikely and that National Treasury has a penalty fee to curb such a displacement. Notwithstanding this argument, it is possible that the observed ETI impacts on formal private employment probabilities of young African males may be a displacement effect[16]. Firms might be hiring these workers at the expense of older workers. We therefore need to empirically evaluate this possibility before we can conclude that the ETI created "new" jobs.

It also possible that a big part of the ETI effects that we estimate is due to a measurement effect. A measurement effect results when unregistered eligible workers who had been hired before the ETI and would have been hired without the ETI became registered so that the firm can claim from the ETI. In which case, the ETI is not creating new jobs but is merely "formalizing" existing jobs. If this is the case, we would need the informal employment coefficient to be negative and our results fail to meet this required. A weaker required is for coefficients for formal employment to be larger and stronger than informal employment coefficients representing the fact that formal employment gains were higher than gains in informal employment. Our results meet this requirement.

7 Recommendation

It is possible that a different design of the ETI might have larger impacts. Bördös, Csillag and Scharle (2015) argue that subsidies that were targeted at young workers and new hires that were paid out indirectly in the form of modest tax cuts lead to negligible employment gains. This is because subsidies that come in the form of modest tax relief decrease the total wage costs of firms only marginally. A wage subsidy that directly reduces the wages of targeted workers might have larger impacts on labour market outcomes than a wage subsidy programme that is paid out in the form of a tax relief.

Bördös, Csillag and Scharle (2015) further argue that in order to ensure that subsidy programmes have long term effects, firms must be required by law to not dismiss the subsidized young worker post the probation period of the subsidy. In South Africa, already existing labour market rigidities make such a policy move undesirable. Furthermore, the stigmatization effect may also explain why the ETI was not successful. In developed countries, research has found that labour market intervention in the form of subsidies lead to declines in the employment probabilities of the targeted group (Levinsohn, 2007). It

[16] It is difficult to prove that any new job that was available during the treatment period was created by the ETI, for example, to prove that firms decided to expand production and hire more subsidized workers because they were incentivized by the ETI because of the problem of the lack of the counterfactual. To prove that the new job was a result of the ETI would require all other things that affect employment to remain the same during the treatment period and such a scenario is nearly impossible. Rathelot *et al.* (2012) explore measures of testing for displacements effects in labour markets.

is possible that firms are not incentivized to hire these targeted young workers because of the "flawed goods" explanation. In which case firms still see the young workers as less employment worthy than older workers. The question is to whether the absolute monetary amount of the subsidy is enough then becomes valid. If the ETI has a stigmatization effect, then firms would require much more monetary compensation to incentivize them to hire these younger workers.

Furthermore, targeted young workers usually have never worked before and thus lack the experience to look for a job (National Treasury, 2011). Providing young job searchers with adequate information about the labour market has the potential to decrease job search costs and reduce period of unemployment. Without perfect information in the labour markets, employees need more time and resources to find employment while firms require more resources and time to find the appropriate workers. A centralized database where young job searchers register and firms can access their information could be useful in matching job searchers to firms. The Department of Labour in conjunction with Statistics South Africa could facilitate such a database. A similar database exists for skilled and high paying employment in the form of Careers24 and LinkedIn. And job search assistance might be a cheaper form of intervention (McKenzie, 2017)

Smith (2008) recommends a worker-side subsidy over a firm side subsidy for South Africa. Smith (2008) argues that job search subsidies could be given to provide financial assistance to young workers. The subsidy could be used for job search related costs such as transportation. Young workers could receive the subsidy when they demonstrate to government that they have been searching for jobs.

8 Conclusion

The apartheid legacy continues to haunt South Africa's labour market narrative. Demographic groups that were denied mainstream economic opportunities are the demographic groups most acutely affected by the unemployment problem today. The South African labour market has several imperfections and poor economic growth in recent years has aggravated matters. Government interventions have failed to make any sustainable impacts. Young South Africans are naturally most acutely affected by the unemployment problem. The ETI was South Africa's most expensive ALMP to date. International literature of ALMP's in developed and developing countries report results. According to McKenzie (2017), successful ALPM's in developing countries improved employment prospects by 2-3 percentage on average. Moreover, most studies find no evidence of impacts of ALMPs in developing countries (McKenzie, 2017).

Our paper investigated the short-run impacts of the ETI, South Africa's most direct and most expensive ALMP. Contrary to Ranchhod and Finn (2014; 2016) and Ebrahim, Leibbrandt and Ranchhod (2017), we find that the ETI had some positive and significant impacts on the employment prospects of young workers during the first year of treatment. Our approach differs to Ranchhod and Finn (2014; 2016) in three ways. Firstly, we evaluate the ETI from 2013 Quarter 4. This is because even though the ETI was formally introduced in 2014 Quarter 1, firms could claim from the ETI from 2013 Quarter 4. Secondly, we focus on formal *private* employment and not formal employment since only firms registered for the PAYE were eligible for the ETI. Thirdly and most importantly, we use birth cohorts to track the treatment group while Ranchhod and Finn (2014; 2016) use ages. Using birth cohorts instead of age bands ensures that elements of our subgroup remain the same through-out the treatment period. We found a positive and significant formal private employment effect during the period of treatment. The ETI effect is largest for African males. African males born between 1985 and 1988 were 3.6 percentage points more likely to find a job in the formal private sector than their counterparts born between 1981 and 1984.

We then evaluated the impact of the ETI on public employment and informal employment for these young African males and find no impact as expected since the ETI was only targeted at formal private employment. We then performed a placebo test and did not find any evidence of differences in formal

private employment prospects during the period where there was no ETI. While the R6.06 billion cost of the ETI amidst a tight fiscal environment might warrant greater expectations from the programme, our estimates of the impact of the ETI are consistent with the average effect in developing countries as reported by McKenzie (2017). Further research is needed to investigate the extent to which the impacts that we have estimated are a result of displacements, measurement effects and deadweight loss. This will assist in understanding the true cost and benefit of the ETI. And as time passes and more labour market data becomes available, further research should investigate the long-term employment effects of the ETI.

9 Appendix

9.1. Mathematically deriving the DID estimator

Consider the standard linear DID estimation;

$y = \beta_0 + \beta_1 post + \beta_2 age\_treatment + \beta_3 (post * age\_treatment) + \theta \underline{\boldsymbol{Z}} + \varepsilon$

Assuming that the required assumptions for DID hold, we have the following;

$E[y_0^{treatment}] = \beta_0 + \beta_2$

$E[y_1^{treatment}] = \beta_0 + \beta_1 + \beta_2 + \beta_3$

$E[y_0^{control}] = \beta_0$

$E[y_1^{control}] = \beta_0 + \beta_1$

Simple pre versus post estimator

Consider first an estimator based on comparing the average difference in outcome *y* before and after treatment;

$$\widehat{\beta_{3.1}} = y_1^{treatment} - y_0^{treatment} \quad \text{(D1)}$$

Taking expectations gives,

$$\widehat{\beta_{3.1}} = E[y_1^{treatment}] - E[y_0^{treatment}]$$
$$= (\beta_0 + \beta_1 + \beta_2 + \beta_3) - (\beta_0 + \beta_2)$$
$$= \beta_1 + \beta_3$$

Hence this estimator is biased so long as β_1 is different from zero, i.e. if a time-trend exist in the outcome *y* then we will confound the time treand as being part of the treatment.

Simple treatment versus control estimator

Now consider the estimator based on comparing the average difference in outcome y during the after the treatment, between the treatment and control group, ignoring pre-treatment outcomes

$$\widehat{\beta_{3.2}} = y_1^{treatment} - y_1^{control} \quad \text{(D2)}$$

Taking expectations gives,

$$\widehat{\beta_{3.2}} = E[y_1^{treatment}] - E[y_1^{control}]$$
$$= (\beta_0 + \beta_1 + \beta_2 + \beta_3) - (\beta_0 + \beta_1)$$
$$= \beta_2 + \beta_3$$

Hence this estimator is biased so long as β_2 is different from zero, i.e. there exist permanent average differences in outcome *y* between the treatment groups. The true treatment effect will be confounded by permanent differences in treatment and control groups that existed prior to any treatment.

The DID estimator

The difference in difference (or "double difference") estimator is defined as the difference in average outcome in the treatment group before and after treatment minus the difference in average outcome in the control group before and after treatment: it is literally a "difference of differences."

$$\widehat{\beta_{3DD}} = y_1^{treatment} - y_0^{treatment} - (y_1^{control} - y_0^{control}) \quad \text{(DID)}$$

Taking expectations gives,

$$\mathrm{E}[\widehat{\beta_{3DD}}] = \mathrm{E}[y_1^{treatment}] - \mathrm{E}[y_0^{treatment}] - (\mathrm{E}[y_1^{control}] - \mathrm{E}[y_0^{control}])$$

$$= \beta_0 + \beta_1 + \beta_2 + \beta_3 - (\beta_0 + \beta_2) - (\beta_0 + \beta_1 - \beta_0)$$

$$= \beta_3$$

This estimator can be seen as taking the difference between two pre-versus-post estimators seen above in (D1), subtracting the control group's estimator, which captures the time trend β_1, from the treatment group's estimator to get β_3. We can also rearrange terms in equation (DID) to get $\widehat{\beta_{3DD}} = y_1^{treatment} - y_0^{treatment} - (y_1^{control} - y_0^{control})$ in which can be interpreted as taking the difference of two estimators of the simple treatment versus control type seen in equation (D2). The difference estimator for the pre-period is used to estimate the permanent difference β_2, which is then subtracted away from the post-period estimator to get β_3.

9.2. Selected regressions for DID estimator

Table A1: Placebo results for African workers

formal private employment	*coefficient*	*standard error*	*P>|t|*
1984-1986 versus 1981-1983	- 0.0060274	0.0080129	0.425
1984-1987 versus 1980-1983	- 0.0068827	0.0069323	0.321
1984-1988 versus 1979-1983	- 0.0069191	0.0062023	0.265
1984-1989 versus 1978-1983	- 0.0131543	0.0056569	0.020

Table A2: Placebo results for African female workers

formal private employment	*coefficient*	*standard error*	*P>|t|*
1984-1986 versus 1981-1983	- 0.0024418	0.011213	0.828
1984-1987 versus 1980-1983	- 0.0094381	0.0097218	0.332
1984-1988 versus 1979-1983	- 0.0094187	0.0086769	0.278
1984-1989 versus 1978-1983	- 0.0168421	0.0078863	0.033

Table A3: DID results for informal employment for African female workers

informal employment	*coefficient*	*standard error*	*P>|t|*
1985-1987 versus 1982-1984	0.00173998	0.0110335	0.115
1985-1988 versus 1981-1984	0.0092934	0.0088282	0.022
1985-1989 versus 1980-1984	0.0247587	0.0079697	0.002
1985-1990 versus 1979-1984	0.0338856	0.0073573	0.000

Table A4: Full DID results for young workers aged between 27-29 versus older workers aged between 30-32

formal_private_employment	Coef.	Robust Std. Err.	t	P>|t|	[95% Conf.	Interval]
1.post	-.0064013	.005357	-1.19	0.232	-.016901	.0040985
1.age_treatment	-.0247544	.0052471	-4.72	0.000	-.0350388	-.0144701
post#age_treatment						
1 1	.0070195	.0074145	0.95	0.344	-.007513	.0215519
province						
Eastern Cape	-.1137688	.0087048	-13.07	0.000	-.1308302	-.0967075
Northern Cape	-.151026	.0096688	-15.62	0.000	-.1699769	-.1320751
Free State	-.1040184	.0092831	-11.21	0.000	-.1222133	-.0858235
KwaZulu-Natal	-.0543237	.0083694	-6.49	0.000	-.0707278	-.0379197
North West	-.0566597	.009886	-5.73	0.000	-.0760364	-.0372831
Gauteng	-.0157468	.0080336	-1.96	0.050	-.0314927	-9.57e-07
Mpumalanga	-.0068871	.0092015	-0.75	0.454	-.0249221	.011148
Limpopo	-.0211829	.0095755	-2.21	0.027	-.039951	-.0024149
quarter						
Quarter 2	-.0040844	.0052369	-0.78	0.435	-.0143487	.0061799
Quarter 3	-.0016887	.0052273	-0.32	0.747	-.0119342	.0085568
Quarter 4	.0086594	.0052571	1.65	0.100	-.0016445	.0189634
geograhical_type						
Urban informal	.0486224	.0071829	6.77	0.000	.0345439	.0627008
Tribal areas	-.1137609	.0052373	-21.72	0.000	-.1240261	-.1034957
Rural formal	.1620654	.0105205	15.40	0.000	.1414453	.1826855
education						
Less than primary completed	.0101717	.015852	0.64	0.521	-.0208983	.0412417
Primary completed	.0585706	.0167057	3.51	0.000	.0258274	.0913138
Secondary not completed	.0829807	.014221	5.84	0.000	.0551076	.1108538
Secondary completed	.1668138	.0144085	11.58	0.000	.1385731	.1950546
Tertiary	.1752119	.015145	11.57	0.000	.1455278	.2048961
Other	.1526158	.0285849	5.34	0.000	.0965894	.2086423
race						
Coloured	.0614371	.0078634	7.81	0.000	.0460249	.0768494
Indian/Asian	.1651896	.0139244	11.86	0.000	.1378977	.1924814
White	.2663684	.0086854	30.67	0.000	.2493449	.2833919
gender						
Female	-.2034742	.0037553	-54.18	0.000	-.2108347	-.1961137
_cons	.4397567	.0164439	26.74	0.000	.4075265	.4719868

Table A5: Full DID results for young workers aged between 26-29 versus older workers aged between 30-33

formal_private_employment	Coef.	Robust Std. Err.	t	P>|t|	[95% Conf.	Interval]
1.post	-.007848	.0046882	-1.67	0.094	-.0170367	.0013408
1.age_treatment1	-.0393789	.0045592	-8.64	0.000	-.0483149	-.0304429
post#age_treatment1						
1 1	.0089558	.0064382	1.39	0.164	-.003663	.0215746
province						
Eastern Cape	-.1086934	.0075758	-14.35	0.000	-.1235419	-.0938449
Northern Cape	-.1564021	.0083986	-18.62	0.000	-.1728632	-.139941
Free State	-.1004023	.0080697	-12.44	0.000	-.1162189	-.0845857
KwaZulu-Natal	-.0495343	.0072861	-6.80	0.000	-.063815	-.0352537
North West	-.0562847	.0085906	-6.55	0.000	-.0731222	-.0394472
Gauteng	-.0164144	.0069976	-2.35	0.019	-.0301296	-.0026992
Mpumalanga	-.0128427	.0079891	-1.61	0.108	-.0285013	.002816
Limpopo	-.0163679	.0083084	-1.97	0.049	-.0326524	-.0000834
quarter						
Quarter 2	-.003702	.0045334	-0.82	0.414	-.0125873	.0051834
Quarter 3	.0014009	.0045361	0.31	0.757	-.0074898	.0102917
Quarter 4	.0085774	.0045528	1.88	0.060	-.0003461	.017501
geograhical_type						
Urban informal	.03928	.0062224	6.31	0.000	.0270842	.0514758
Tribal areas	-.1214279	.0045203	-26.86	0.000	-.1302877	-.1125681
Rural formal	.1552227	.0092394	16.80	0.000	.1371136	.1733317
education						
Less than primary completed	.0182755	.0136011	1.34	0.179	-.0083826	.0449337
Primary completed	.068756	.0144019	4.77	0.000	.0405285	.0969835
Secondary not completed	.0893876	.0121423	7.36	0.000	.0655888	.1131864
Secondary completed	.1658109	.0123063	13.47	0.000	.1416906	.1899312
Tertiary	.181786	.0129454	14.04	0.000	.156413	.2071589
Other	.1392915	.0245875	5.67	0.000	.0911001	.1874828
race						
Coloured	.061256	.0068488	8.94	0.000	.0478324	.0746796
Indian/Asian	.1737583	.0121467	14.30	0.000	.1499509	.1975658
White	.2605873	.0075802	34.38	0.000	.2457301	.2754444
gender						
Female	-.1979066	.0032552	-60.80	0.000	-.2042868	-.1915264
_cons	.4390957	.0141438	31.05	0.000	.411374	.4668174

Table A6: Full DID results for young workers born between 1984-1986 versus older workers born between 1981-1983

formal_private_employment	Coef.	Robust Std. Err.	t	P>|t|	[95% Conf.	Interval]
1.post	.003081	.005297	0.58	0.561	-.0073012	.0134631
1.cohort_treatment	-.0396479	.0051532	-7.69	0.000	-.0497482	-.0295476
post#cohort_treatment						
1 1	.016733	.0073296	2.28	0.022	.002367	.0310991
province						
Eastern Cape	-.1158427	.0086577	-13.38	0.000	-.1328118	-.0988736
Northern Cape	-.1501518	.0096501	-15.56	0.000	-.1690661	-.1312376
Free State	-.1100171	.0092442	-11.90	0.000	-.1281357	-.0918986
KwaZulu-Natal	-.0597437	.0083396	-7.16	0.000	-.0760893	-.0433981
North West	-.05673	.0098072	-5.78	0.000	-.0759521	-.0375079
Gauteng	-.0221409	.008041	-2.75	0.006	-.0379014	-.0063805
Mpumalanga	-.0175571	.0091561	-1.92	0.055	-.0355031	.0003889
Limpopo	-.0287924	.009494	-3.03	0.002	-.0474007	-.0101841
quarter						
Quarter 2	-.0070123	.0052023	-1.35	0.178	-.0172089	.0031843
Quarter 3	-.0038254	.0051935	-0.74	0.461	-.0140047	.0063538
Quarter 4	-.0083664	.0051734	-1.62	0.106	-.0185063	.0017736
geograhical_type						
Urban informal	.046988	.007125	6.59	0.000	.0330229	.0609531
Tribal areas	-.1162492	.0051415	-22.61	0.000	-.1263266	-.1061719
Rural formal	.1634943	.0106516	15.35	0.000	.1426172	.1843713
education						
Less than primary completed	.0360379	.0157619	2.29	0.022	.0051445	.0669313
Primary completed	.0818503	.0165777	4.94	0.000	.0493579	.1143427
Secondary not completed	.1005477	.0140943	7.13	0.000	.072923	.1281725
Secondary completed	.1817304	.0142681	12.74	0.000	.1537648	.2096959
Tertiary	.1940988	.0150249	12.92	0.000	.1646499	.2235477
Other	.1470376	.0280651	5.24	0.000	.09203	.2020453
race						
Coloured	.0579957	.0078637	7.38	0.000	.0425828	.0734086
Indian/Asian	.1670716	.0138503	12.06	0.000	.1399249	.1942183
White	.2670709	.0088525	30.17	0.000	.24972	.2844219
gender						
Female	-.1893647	.0037068	-51.09	0.000	-.1966301	-.1820994
_cons	.4171365	.0163338	25.54	0.000	.3851221	.4491509

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Table A7: Full DID results for young workers born between 1984-1987 versus older workers born between 1981-1982

formal_private_employment	Coef.	Robust Std. Err.	t	P>|t|	[95% Conf.	Interval]
1.post	.0011501	.0046396	0.25	0.804	-.0079435	.0102438
1.cohort_treatment1	-.0548411	.0044593	-12.30	0.000	-.0635813	-.046101
post#cohort_treatment1						
1 1	.0207894	.0063542	3.27	0.001	.0083353	.0332435
province						
Eastern Cape	-.1096363	.0075068	-14.60	0.000	-.1243495	-.094923
Northern Cape	-.1491862	.0083522	-17.86	0.000	-.1655565	-.1328158
Free State	-.1039388	.0079725	-13.04	0.000	-.1195649	-.0883128
KwaZulu-Natal	-.0528413	.0072303	-7.31	0.000	-.0670126	-.03867
North West	-.0554149	.0084881	-6.53	0.000	-.0720515	-.0387783
Gauteng	-.0217919	.0069611	-3.13	0.002	-.0354355	-.0081483
Mpumalanga	-.0111197	.0079216	-1.40	0.160	-.026646	.0044065
Limpopo	-.02223	.0082117	-2.71	0.007	-.0383249	-.006135
quarter						
Quarter 2	-.0025776	.0044988	-0.57	0.567	-.0113953	.00624
Quarter 3	.0003748	.004492	0.08	0.934	-.0084295	.0091791
Quarter 4	-.0057115	.0044738	-1.28	0.202	-.0144802	.0030572
geograhical_type						
Urban informal	.0376861	.0061529	6.12	0.000	.0256264	.0497457
Tribal areas	-.1189575	.0044468	-26.75	0.000	-.1276732	-.1102418
Rural formal	.1564182	.0092397	16.93	0.000	.1383084	.1745279
education						
Less than primary completed	.020755	.0138856	1.49	0.135	-.0064606	.0479706
Primary completed	.0656825	.014678	4.47	0.000	.0369138	.0944512
Secondary not completed	.0860175	.012472	6.90	0.000	.0615725	.1104625
Secondary completed	.1602881	.0126228	12.70	0.000	.1355475	.1850287
Tertiary	.1748343	.0132651	13.18	0.000	.1488348	.2008339
Other	.1258249	.0247605	5.08	0.000	.0772946	.1743553
race						
Coloured	.0681926	.0068214	10.00	0.000	.0548226	.0815626
Indian/Asian	.1873311	.0120822	15.50	0.000	.1636502	.211012
White	.2708232	.007717	35.09	0.000	.2556979	.2859484
gender						
Female	-.1907698	.003211	-59.41	0.000	-.1970633	-.1844762
_cons	.4329943	.0143792	30.11	0.000	.4048111	.4611775

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