ESTIMATING THE SHORT RUN EFFECTS OF SOUTH AFRICA’S EMPLOYMENT TAX INCENTIVE ON YOUTH EMPLOYMENT PROBABILITIES USING A DIFFERENCE-IN-DIFFERENCES APPROACH

VIMAL RANCHHOD* AND ARDEN FINN†

Abstract
South Africa’s Employment Tax Incentive (ETI) came into effect on the 1st of January 2014, with the objective of reducing the substantial national youth unemployment rate. Under the ETI, firms are eligible to claim a deduction from their taxes due, for the portion of their wage bill that is paid to certain groups of youth employees. We utilise several waves of nationally representative data and implement a difference-in-differences methodology at the individual level, in order to identify the effects of the ETI on youth employment probabilities in the short run. Our primary finding is that the ETI did not have any statistically significant and positive effects on youth employment probabilities. The point estimate from our preferred regression is $0.005$ and the 95% confidence interval is from $0.017$ to $0.006$. We also find no evidence that the ETI has resulted in an increase in the level of churning in the labour market for youth. Thus, any decrease in tax revenues that arise from the ETI are effectively accruing to firms which, collectively, would have employed as many youth even in the absence of the ETI.

JEL Classification: H25, H32, J38

Keywords: Youth, unemployment, South Africa, wage subsidy, employment tax incentive

1. INTRODUCTION

Stubbornly high levels of unemployment in general, and extremely high levels of youth unemployment in particular, have been constant features of South African society since the transition to democracy in 1994. The youth (defined in this paper as adults aged between 18 and 29) experience an unemployment rate that is more than double that of the overall population unemployment rate, and about two-thirds of unemployed youth have never had a job (National Treasury, 2011). Policy discussions aimed at boosting youth employment have been taking place since the mid-2000s, with the objective of

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raising employment levels and mitigating the “wasting” effect of being unemployed for an extended period of time. These discussions led to the Employment Tax Incentive (ETI), which was initiated on the 1st of January 2014. The ETI takes the form of a direct intervention in the labour market to reduce the cost to firms of employing young workers. This is one of the most ambitious youth employment initiatives in the country’s history, with a proposed cost to government of R5 billion over 3 years intended to create 178,000 new jobs for youth over that period.

In this paper, we ask the question “Did the youth employment tax incentive have an impact on youth employment rates in South Africa in the short run?” We utilise data from the nationally representative Quarterly Labour Force Survey (QLFS) from 12 quarters before and 2 quarters after the implementation of the ETI in order to identify the short run effects of the intervention. We employ a number of difference-in-differences estimators with different control groups to identify what effect the programme had on the probability of youth employment. We find no evidence of a positive effect on youth employment probabilities in the short run, and no evidence that the implementation of the ETI has led to increased levels of churning amongst youth in the labour market.

The remainder of this paper is structured as follows. Section 2 presents a background to the ETI in South Africa and summarises both local and international evidence on youth employment incentives. In Section 3, we discuss the rules of the ETI. Section 4 describes the datasets and variables that we use, while Section 5 outlines the methods that we use. Section 6 presents the main results of our analysis, Section 7 discusses the outcomes of some robustness checks and Section 8 provides some concluding discussion.

2. BACKGROUND TO THE ETI IN SOUTH AFRICA AND INTERNATIONAL EVIDENCE

One of the first papers to discuss a youth wage subsidy for South Africa is by Levinsohn (2007). He argues that when firms hire workers, they predict a productivity level based on training and experience. For youth in South Africa, education is a weak signal and in addition, young people have a shortfall of work experience. Moreover, firms face substantial costs in having to train inexperienced workers. Firms may thus prefer to hire older workers who can signal their productivity through experience.

Thus, most interventions have taken the form of one of two strategies. The first branch aims to address the lack of relevant skills or reliable education by raising the quality of workers through supply-side interventions. The second branch offsets the cost (thereby lowering the risk) of employing inexperienced/young workers through demand-side initiatives.

The ETI is a demand-side intervention. In South Africa, there have been two major demand-side interventions; the Expanded Public Works Programme (EPWP) and Learnership Agreements.1 The EPWP provided 1.6 million jobs in its first phase, but is widely recognised as a short-term and unsustainable solution (Meth, 2011 and Betcherman et al., 2004). A Learnership is a policy whereby firms are subsidised for providing approved training to employees, integrated with a job. While the effects of Learnerships have not been established, Schoer and Rankin (2011) found that just 19% of firms offer

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1 As defined in Section 17 of the Skills Development Act.
Learnerships. National Treasury (2011) indicates that 73% of Learnership uptake is through large firms, and that the programme mainly benefits medium-skilled workers who earn relatively higher salaries. Burns et al. (2010) further suggest that the subsidy is too low to cover the related training costs.

Aggregate trends indicate that these initiatives have not solved the youth unemployment problem. The ETI was proposed as an incentive for firms to hire youth, compensating firms for the risks that firms take in hiring youth with uncertain productivity levels, as well as for the training that firms may have to provide to these workers.

A number of criticisms have been levelled against the ETI. The first is that a “deadweight loss” is incurred through companies who would have hired young workers in the absence of the programme, as has been documented in other countries with similar schemes (Betcherman et al., 2004). Moreover, companies with market power might capture these subsidies as economic rents, rather than pass them on to consumers. Destructive churning is also a potential problem if companies release workers after the subsidy expires, and employ new workers who qualify for the subsidy as replacements. The fear of young hires replacing established employees entails a similar response (COSATU, 2013).

Levinsohn et al. (2014) investigate the effectiveness of a youth wage subsidy in South Africa by conducting a controlled experiment whereby a voucher was given to unemployed individuals in a treatment group. Compared to the control group who did not receive the voucher, employment was higher by 25% (7.4 percentage points higher than the 31% probability of employment in the comparison group), and this persisted for 18 months after the expiry of the voucher.

Burns et al. (2010) create a micro-simulation for the effects of the subsidy, and find a 4.7% increase in employment, for what they term a “medium-high” assumed wage elasticity of demand of 0.7. These findings are qualified with the warning that these gains are not long-term and do not address deeper structural issues in the economy.

2.1 Evidence from Youth Employment Initiatives in Other Countries
Betcherman et al. (2007) analyse the Youth Employment Inventory, a database of worldwide youth employment interventions released by the World Bank. 39% of interventions were skills-based, with subsidies constituting approximately 12% overall. Of these, fewer than half of all programmes were cost-effective and successful. Three general conclusions emerge: finance is important in determining success, interventions work better in countries with flexible labour markets, and context – not type of programme – determines the success of an initiative. Wage subsidies have been successful in Europe especially, with increases in employment of 12 and 13% in Poland and the Czech Republic, respectively.

In contrast, Smith (2006) uses data from several OECD countries, and argues that any benefits of a demand-side subsidy are manifest through the supply-side. Finally, Puerto (2007) studies similar interventions in developing economies from Latin American and the Caribbean countries. Including costs, an average return of 4% in sustainable employment was created compared to control groups. Generally, programmes that are

2 In technical economic terms, this would not be a “deadweight loss,” as there is no obvious decrease in efficiency that would arise from such a flat transfer. There is, nonetheless, an opportunity cost to the fiscus, due to the reduction in resources available for other state activities.
demand-driven and involve the private sector have been more successful. At the same
time, Burns et al. (2010) observe that in Argentina’s Proempleo Experiment, the take-up
of firm vouchers was low while the increase in employment was high, which lends sup-
port to the findings of Smith (2006) and Levinsohn et al. (2014) that the gains are sup-
ply driven.

The overall evidence is thus mixed, but raises two salient points: the potential for
deadweight loss from jobs that would have been created anyway, and whether the inter-
vention is aimed at the demand-side or the supply-side of the labour market. In summa-
tion, some interventions do generate positive returns, but others do not. With this
evidence in mind we now turn to a description of the ETI, and how it has been imple-
mented in South Africa.

3. OUTLINE OF THE PROGRAMME

The ETI was signed into law in 2013, with effect from the 1st of January 2014. The
stated aim was to spend R5 billion over 3 years to create 178,000 new jobs for the youth
at a cost of approximately R28,000 per job. The budget for the first year of implementa-
tion was R1 billion (National Treasury, 2014). Employees qualify for the ETI if they are
aged between 18 and 29 years and hold a South African ID. The employee cannot have
been hired for the current job before the 1st of October 2013.

The main eligibility criterion for an employer is registration for employees’ income
tax (PAYE). There is no limit to the number of qualifying workers that an employee can
hire. The employer claims the incentive by reducing the amount of PAYE tax that is pay-
able to SARS on a monthly basis, based on the number of employees supported by the
incentive and the relevant salaries of those employees. The benefits of the programme
extend to a maximum of 24 months, and the programme in its current form is effective
until the 31st of December 2016.

The ETI Bill (Republic of South Africa, 2013) sets out the amounts that employers
can claim from the incentive each month, and this information is presented in the Table
1 below.

Figure 1 presents a graphical contrast of the cost to company versus the wage of a sub-
sidised worker over the R0 to R6,000 range. As in the table above, the wage is subsidised
by 50% up to R2,000, beyond which the subsidy is a flat rate of R1,000, up to a wage
of R4,000. In the final segment the cost to company and the wage received by the subsi-
dised employee converge until they meet at R6,000.

Administrative data on the take-up of the ETI during the first six months of the pro-
gramme is not yet publicly available. Nonetheless, we have two pieces of relevant infor-
mation about take-up rates obtained from official speeches of government
representatives. First, the erstwhile Minister of Finance noted in his February 2014

Table 1. Details of the ETI per qualifying employee

<table>
<thead>
<tr>
<th>Monthly remuneration</th>
<th>Year 1</th>
<th>Year 2</th>
</tr>
</thead>
<tbody>
<tr>
<td>R0–R2 000</td>
<td>50% of monthly pay</td>
<td>25% of monthly pay</td>
</tr>
<tr>
<td>R2 001–R4 000</td>
<td>R1 000</td>
<td>R500</td>
</tr>
<tr>
<td>R4 001–R6 000</td>
<td>R1 000 − 0.5*(monthly pay − R4 000)</td>
<td>R500 − 0.25*(monthly pay − R4 000)</td>
</tr>
</tbody>
</table>

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budget speech that 56,000 beneficiaries were recorded in the first month of the year (Gordhan, 2014). Second, in his June 2014 State of the Nation Address, President Zuma stated that in the first five months after its introduction, there were 133,000 beneficiaries of the ETI, most of whom were employed in the wholesale and retail, manufacturing, and finance sectors (Zuma, 2014).

By reducing the costs, both in absolute and relative terms, of young workers, the ETI aims to stimulate demand for youth labour and thus reduce the youth unemployment rate. The crucial parameter is the “wage elasticity of labour demand for youth labour,” which could be very high or very low. This could, in turn, result in the ETI having a very large or a very small impact on the youth unemployment problem. This theoretical uncertainty provides the motivation for our empirical analyses.

4. DATA AND VARIABLES

In this paper, we make use of 14 waves of the QLFSs conducted by Statistics South Africa (StatsSA). We used all the waves from QLFS 2011:1 up to and including QLFS 2014:2, which spans the period from January 2011 until June 2014, thus capturing trends in the data from three years prior to the introduction of the ETI as well as data from the first six months in which the intervention was implemented.

The QLFS surveys are treated as repeated cross-sections in our study. In each wave, enumerators attempt to contact approximately 30,000 dwellings and interview all adult resident members of these dwellings. The sampling frame is obtained from the Census Master sample and sampling follows a stratified clustered design, as per various StatsSA documents that are released with each cross-section. StatsSA releases a weight variable that we use in all computations that are intended to reflect population-level statistics.

Each wave comes with a set of related documentation including a user guide, metadata document and questionnaire. The datasets, together with the documentation, are available for download from www.datafirst.uct.ac.za.

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Table 2 indicates the aggregate sample size of our dataset, as well as the cell sizes of various groups of respondents within each wave. We restrict our sample to working aged adults aged 18–64. Our overall sample across all waves is 667,610 observations, including 246,941 youth aged 18–29, 79,289 “almost youth” aged 30–34, 199,801 “prime aged” respondents aged 35–49, and 141,579 “older adult” respondents aged 50–64. The advantage of such a large dataset is that we have a lot of statistical power, which implies that our results are likely to be relatively precisely estimated.

4.1 Variables Used
We have two main variables of interest; employed is a binary variable that indicates whether a respondent is classified as employed in the StatsSA derived status variable, and employed_formal is a binary variable that indicates whether a respondent is employed in the formal sector.5

To conduct our analyses, we constructed several temporal variables. First, we generate a trend variable which is equal to the wave number. Second, we generate a “pre-” variable and a “post-” variable, which identifies whether a row of data is obtained from waves 1–12, or waves 13–14, respectively. Third, in our main regressions, we separate the post-variable into post1 and post2, which denotes wave 13 and wave 14, respectively. Fourth, to account for seasonality in the labour market, we generate a set of quarter_i dummy variables, which represents the quarter within a calendar year that the relevant survey was conducted.

Of the demographic variables, we use the Age variable to construct the relevant age groups which capture whether a respondent was a potential beneficiary of the incentive or not. In addition, we use race, gender and the highest educational attainment. The education variable is converted into a dummy variable called tertiary that takes on a value

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4 This is slightly different to the StatsSA age range, in that they include respondents aged 15–17 as well. We chose not to include this subset of the sample as they are not eligible for any tax incentives under the ETI.

5 The sector of employment is derived by StatsSA.
of one if the respondent has attained some form of a post-secondary school level education. For some of our analysis, we refer to “skilled” and “unskilled” youth, and the skilled variable is identical to the tertiary variable. Finally, we make use of a number of other variables. At the household level, we use the province variable and generate an urban variable. To investigate churning, we construct four dummy variables. Amongst the employed, we generate recentjob_3 and recentjob_6, which identify whether a respondent began his/her job within the last three or last six months, respectively. Amongst the people who are not employed, we construct recentloss_3 and recentloss_6, which identifies whether a respondent who was not employed at the time of the survey lost their most recent job within the past three or past six months, respectively.6

In Table 3, we summarise some of the variables that we used. The mean age amongst youth is 23.12 years, while amongst the non-youth it is 44.8 years. About 49% of youth are male, while amongst the non-youth this is approximately 44%. About 84% of the youth are African, while about 75% of non-youth are African. The youth are less likely to have some tertiary education, at about 7%. The proportion of youth residing in urban areas is slightly lower than that of non-youth, at 61% and 66%, respectively. The major provinces that our sample is drawn from are Gauteng and KwaZulu-Natal.

Note that recentjob_3 and recentloss_3 are nested within recentjob_6 and recentloss_6, respectively. Thus, if a respondent found their current employment within the previous three months then it implies that they also found their current employment within the previous six months, and similarly for having recently lost a job within the previous three or six months (conditional on not being employed).

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Table 3. Means of covariates used in regressions

<table>
<thead>
<tr>
<th>Variable</th>
<th>Youth Pre-</th>
<th>Youth Post-</th>
<th>Youth Diff.</th>
<th>Non-Youth Pre-</th>
<th>Non-Youth Post-</th>
<th>Non-Youth Diff.</th>
<th>Full Sample (All waves)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age (years)</td>
<td>23.12</td>
<td>23.20</td>
<td>0.08</td>
<td>44.81</td>
<td>44.86</td>
<td>0.05</td>
<td>36.79</td>
</tr>
<tr>
<td>Male</td>
<td>0.49</td>
<td>0.49</td>
<td>0.00</td>
<td>0.44</td>
<td>0.44</td>
<td>0.00</td>
<td>0.46</td>
</tr>
<tr>
<td>African (%)</td>
<td>83.68</td>
<td>84.17</td>
<td>0.49</td>
<td>74.40</td>
<td>74.73</td>
<td>0.33</td>
<td>77.89</td>
</tr>
<tr>
<td>Coloured (%)</td>
<td>10.34</td>
<td>10.29</td>
<td>–0.05</td>
<td>13.05</td>
<td>13.25</td>
<td>0.20</td>
<td>12.07</td>
</tr>
<tr>
<td>Indian (%)</td>
<td>1.65</td>
<td>1.81</td>
<td>0.16</td>
<td>2.66</td>
<td>2.62</td>
<td>–0.04</td>
<td>2.29</td>
</tr>
<tr>
<td>White (%)</td>
<td>4.33</td>
<td>3.73</td>
<td>–0.60</td>
<td>9.89</td>
<td>9.41</td>
<td>–0.48</td>
<td>7.76</td>
</tr>
<tr>
<td>Tertiary</td>
<td>0.07</td>
<td>0.07</td>
<td>0.01</td>
<td>0.13</td>
<td>0.13</td>
<td>0.00</td>
<td>0.11</td>
</tr>
<tr>
<td>Urban</td>
<td>0.61</td>
<td>0.61</td>
<td>0.00</td>
<td>0.66</td>
<td>0.67</td>
<td>0.00</td>
<td>0.64</td>
</tr>
<tr>
<td>Province (%)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Western Cape</td>
<td>10.80</td>
<td>10.92</td>
<td>0.12</td>
<td>13.36</td>
<td>14.37</td>
<td>1.01</td>
<td>12.51</td>
</tr>
<tr>
<td>Eastern Cape</td>
<td>10.52</td>
<td>10.96</td>
<td>0.44</td>
<td>11.30</td>
<td>11.38</td>
<td>0.08</td>
<td>11.04</td>
</tr>
<tr>
<td>Northern Cape</td>
<td>4.77</td>
<td>4.82</td>
<td>0.05</td>
<td>5.79</td>
<td>5.62</td>
<td>–0.17</td>
<td>5.40</td>
</tr>
<tr>
<td>Free State</td>
<td>8.77</td>
<td>8.07</td>
<td>–0.70</td>
<td>8.91</td>
<td>8.84</td>
<td>–0.07</td>
<td>8.82</td>
</tr>
<tr>
<td>KwaZulu-Natal</td>
<td>18.47</td>
<td>19.03</td>
<td>0.56</td>
<td>16.00</td>
<td>15.67</td>
<td>–0.33</td>
<td>16.91</td>
</tr>
<tr>
<td>North West</td>
<td>7.50</td>
<td>7.30</td>
<td>–0.20</td>
<td>8.12</td>
<td>7.76</td>
<td>–0.36</td>
<td>7.85</td>
</tr>
<tr>
<td>Gauteng</td>
<td>16.06</td>
<td>15.40</td>
<td>–0.66</td>
<td>17.59</td>
<td>17.21</td>
<td>–0.38</td>
<td>16.95</td>
</tr>
<tr>
<td>Mpumalanga</td>
<td>10.70</td>
<td>10.83</td>
<td>0.13</td>
<td>8.86</td>
<td>8.94</td>
<td>0.08</td>
<td>9.55</td>
</tr>
<tr>
<td>Limpopo</td>
<td>12.42</td>
<td>12.69</td>
<td>0.27</td>
<td>10.08</td>
<td>10.22</td>
<td>0.14</td>
<td>10.97</td>
</tr>
</tbody>
</table>

Notes:
- These are sample means and are unweighted.
- The “Diff.” column is the difference between the mean of the relevant variable in the Post-period relative to the Pre-period.
- The Pre- and Post- periods are represented by waves 1–12, and waves 13–14, respectively.
To summarise, the youth in our sample do look somewhat different to the non-youth, but in ways that are not surprising. The youth are mostly unskilled, most likely to be African, almost balanced in terms of gender, and are just over 23 years old on average. Approximately three out of five of these youth live in urban areas, and more than a third of them live in either KwaZulu-Natal or Gauteng.

Ideally, we want the underlying samples to be stable within each group, across the Pre- and Post- periods. From the relevant Diff column in Table 3, our overall observation is that the samples within each group and across the time periods are extremely stable. This implies that the sampling framework was consistent across time, and that there was no substantial selective migration over time. This makes us more confident in the validity of our subsequent empirical findings.

5. METHODS

We employ standard econometric methods in this paper. First, we present a number of summary statistics from our sample. These take the form of the means and conditional means of the employment to population ratio,$^7$ for various age groups within each wave. We then estimate two sets of regression results. The first is a set of before-after regressions that focuses only on youth. We fit a model of the form:

\[ y = \beta_0 + \beta_1 \text{trend} + \beta_2 \text{post1} + \beta_3 \text{post2} + \beta X + \epsilon \]

Our regressions are OLS regressions run at the individual level and are linear probability models. We have two dependent variables of interest, namely employed and employed_formal. We chose to investigate employment in the formal sector in isolation, as the design of the ETI is such that it does not have any direct bearing on firms in the informal sector. For each of these dependent variables, we fit the regression model to the data for two different estimation samples; all of the youth in our data, and youth that reside in urban areas only. Our rationale for focussing only on urban areas is again informed by the design of the ETI, as we expect that a disproportionate number of eligible firms will be based in urban areas.$^8$ By focussing on the formal sector and/or on urban youth we thus hope to have a better targeted regression model, in terms of our ability to identify the effects of the ETI.

We further include age, age-squared, male, urban (where relevant), tertiary, as well as indicator variables for province categories and race categories as control variables. The regressions are weighted and the standard errors are clustered at the PSU level.

Our coefficients of interest in these regressions are $\beta_2$ and $\beta_3$, which correspond to the variables post1 and post2. We chose to separate the “treatment period” by including two dummy variables separately as the ETI might not have had much of an impact in wave 13 as it had just been introduced at that time. $\beta_2$ and $\beta_3$ measure the magnitude of any trend break in the dependent variable (in the relevant estimation sample), in each of the two waves after the introduction of the ETI. In the absence of any contemporaneous

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$^7$ This is also referred to as the “labour absorption rate.”

$^8$ In waves 1 to 12 of our dataset, 63% of rural youth who are employed are employed in the formal sector, while the corresponding statistic for urban youth is just below 81%.

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confounding factors, these parameters measure the effects of the ETI in wave 13 and wave 14, respectively.

Our second set of results is obtained from a regression model that uses the differences-in-differences (DD) approach popularised by Card and Kruger (1994). The primary limitation of the before-after estimator is that any changes in the general economic environment that affect youth employment probabilities, which are also coincident with the introduction of the ETI, will also be captured by the relevant coefficients. The DD estimator is more robust to such threats to the identification of the effects of the ETI.

To implement the DD estimator, we fit a model of the form:

\[ y = \beta_0 + \beta_1 \text{trend} + \beta_2 \text{youth} + \beta_3 \text{post1} + \beta_4 \text{post2} + \alpha_1(\text{post1} \times \text{youth}) \\
+ \alpha_2(\text{post2} \times \text{youth}) + \lambda \mathbf{X} + \varepsilon \]

We use the same dependent variables and estimation samples as with the before-after regression models described above. Our estimation samples now also include all of the non-youth in our data. In addition to all of the covariates from the before-after regression models, we also include youth, youth_post1 and youth_post2. In conforming to the terminology used in the DD literature; our “treated group” variable is youth, the “treatment period” is captured by post1 and post2, and the “treated group in the treatment period” variables are the interaction dummies youth_post1 and youth_post2.

Our primary coefficients of interest are \( \alpha_1 \) and \( \alpha_2 \), which correspond to the youth_post1 and youth_post2 variables. The trend variable captures general trends in employment rates over the fourteen waves of data, while the youth variable captures mean differences in the employment rate of youth relative to the non-youth in the estimation sample. The variables post1 and post2 allow for a break in the general trend in employment in the estimation sample in wave 13 and wave 14, respectively. What the coefficient on the youth_post1 variable thus represents is mean changes in the probability of employment amongst youth relative to the non-youth, in the first of the post-ETI periods, in addition to any pre-existing differences in employment probabilities that were observed between youth and non-youth prior to the introduction of the ETI, and also in addition to any general changes in the economic climate that occurred in wave 13 that affected the employment probabilities of both youth and non-youth equally. In the absence of any additional confounding factors, this represents the average treatment effect of the ETI in wave 13.

6. RESULTS

6.1 Labour Absorption Rates

In Table 4 below, we summarise the labour absorption rates in each wave. The overall mean, across all the waves and all the age groups is 0.465, which means that slightly below half of all the adults aged 18–64 were employed during this time period. One interesting observation is how the labour absorption rate varies across the life cycle. Amongst the youth aged 18–29, the labour absorption rate is only 0.292. The absorption rate increases further with age until it peaks at just below two-thirds amongst the prime

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9 The interpretation of the coefficient corresponding to the youth_post2 variable is analogous.
10 We discuss additional possible confounding factors further in the section on robustness tests.
aged adults, and then drops sharply amongst the older adults to 0.47. The absorption rate is thus about 18 percentage points higher for the older adults than the youth.

Of particular interest in this study is whether there is a change in the trend line of youth employment relative to non-youth employment in either wave 13 or wave 14. The trend lines are depicted graphically in Fig. 2 below. When we consider the aggregate absorption rate, there does seem to be a small upward trend between wave 1 and wave 12. Between January 2011 and December 2013, the proportion employed increased from 46% to a peak of 47.4%. This may be small in terms of percentage points, but it is substantial in terms of the number of new jobs that were created. The trends amongst the youth as well as amongst the non-youth are both similar to that of the aggregate, albeit at different levels of absorption. In 2014, however, the absorption rate amongst the non-youth seems to have stabilised or decreased slightly, from a peak of 57.6% in wave

Table 4. Mean labour absorption rates by wave and age group

<table>
<thead>
<tr>
<th>Wave</th>
<th>Youth</th>
<th>Non-Youth</th>
<th>Total: All Youth</th>
<th>Almost Youth (30–34)</th>
<th>Prime Aged (35–49)</th>
<th>Older (50–64)</th>
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</tbody>
</table>

Notes:
- All means are weighted using the sampling weights.
- The sample includes any respondent of a particular group, including those who are not economically active.

Figure 2. Mean labour absorption rates by age group

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12 to 57.4% and then 57.2% in wave 13 and wave 14, respectively. This pattern is also observed amongst the youth, although the decline is more pronounced. The absorption rate amongst the youth decreases from a peak of 30.4% in wave 12, prior to the introduction of the ETI, to 29.2% and 29.3% in wave 13 and wave 14, respectively.

### 6.2 Before-After Regression Results

In this section, we present and discuss the results from our before-after regression models. In column 1 of Table 5, the estimation sample includes the entire set of youth in our sample from ages 18–29 inclusive, from both urban and non-urban areas. The dependent variable is “employed,” and we have suppressed the coefficients on the control variables for brevity.11 As discussed in our methods section, our coefficients of interest correspond to the post1 and post2 variables. They measure the difference in the employment probability of youth in the first and second quarters of 2014 relative to what one would expect to observe based on all of the covariates and the pre-existing trend.

The first thing to note is that the coefficients in column 1 are negative in sign, when we would have anticipated that they would be positive if the ETI was having a positive impact on youth employment probabilities. The second pertinent observation is that they are extremely small in magnitude. For example, the coefficient on post1 of 0.00162 indicates that that out of every 10 000 youth, we estimate that there are 16.2 fewer employed youth than we would have expected, in the first quarter of 2014. Similarly, the coefficient on post2 of 0.000142 indicates that that out of every 10 000 youth, we estimate that there are 1.42 fewer employed youth than we would have expected, in the second quarter of 2014. The third relevant piece of information is that the corresponding

11 The full regression results are available from the authors upon request.
standard errors are very small. This derives from the large sample sizes that we have, and indicates that our coefficients are quite precisely estimated. Thus, the 95% confidence intervals for the coefficients on post1 and post2 are (−0.0127, 0.0095) and (−0.0123, 0.0120), respectively. For all intents and purposes, this is consistent with a “zero effect” of the ETI on the employment probability of youth.

In the remaining columns in Table 5, the substantive findings are the same as those obtained from column 1. Our overall findings remain unchanged when we consider only formal sector employment, or focus only on youth in urban areas, or unskilled youth only, or combinations of these groups and dependent variables; the coefficients are small in magnitude, are not statistically significantly different from zero, and have fairly small standard errors.

6.3 Difference-in-Differences Regression Results

In this section, we present and discuss the results from our main regression models. In column 1 of Table 6, we present our most preferred regression results. The dependent variable is employed, the estimation sample includes the entire working aged population in our sample from the ages of 18–64 inclusive, and we have not restricted the sample by geography or by skill level. As with our presentation and discussion of our “before-after” regression models, we have suppressed the coefficients on the control variables.

We notice that the trend variable has a very small coefficient and is not statistically significantly different from zero. This implies that the trend in employment probabilities was flat over the period of observation. The fact that the coefficient estimates on post1 and post2 are also small and not statistically significant indicates that there was no break in the trend for the non-youth in either the first or second quarters of 2014. The youth coefficient of 0.0517 is surprising in that it is positive and significant, but it needs to be interpreted in conjunction with the age and age squared coefficients, as there is a mechanical mapping from age to the youth variable. Once one accounts for the effects of the age and age squared variables, the results accord with our expectations based on the summary statistics discussed above.

As discussed in the methods section, the primary coefficients of interest to us are those that correspond to youth_post1 and youth_post2. The first of these, at −0.0056, indicates that, conditional on all the other covariates, the youth in the first quarter of 2014 were just over one half of a percentage point less likely to be employed than the appropriately adjusted comparison group of non-youth in that quarter. Similarly, the coefficient of −0.0053 indicates that, conditional on all of the other covariates, the youth in the second quarter of 2014 were 0.53 of a percentage point less likely to be employed than the appropriately adjusted comparison group of non-youth in that quarter.

Note that neither of the two point estimates are statistically significantly different from zero, and the corresponding confidence intervals are (−0.016, 0.005) and (−0.012, 0.005).

12 These are our preferred results for two reasons. First, this specification allows for the largest sample size, which in turn gives us the most statistical power with the available data. Second, the ETI applies to youth in both urban and rural areas, and to both skilled and unskilled youth. Moreover, it is not clear how good a proxy the Stats SA definition of the formal sector is in terms of its ability to identify the eligibility of a firm.

13 As before, these are available from the authors on request.

14 This point is true for all of the regression results presented in this table.
Table 6. Difference-in-Differences regressions on youth employment probability

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Notes:
- Standard errors in parentheses
- Asterisks denote statistical significance as follows: *** p<0.01, ** p<0.05, * p<0.1
- Suppressed coefficients on race dummies, male, tertiary (Col 1–4), urban (Col 1,3,5,7), province dummies and quarter dummies.
- Regressions incorporate survey weights.
- Standard errors are clustered at the PSU level.
(-0.017, 0.006) for the first and second quarters of 2014, respectively. These are fairly precisely estimated coefficients, which just barely include the value of zero. What this implies is that, even if the true population parameter is positive, it is likely to be extremely small in magnitude.

When we consider the other regression models that we have fit to the data, we notice a remarkably consistent set of results. First, all of the relevant coefficients are negative in sign. Second, they are all small in magnitude, at less than one percentage point in absolute value. Third, with one exception, they are not statistically significantly different from zero.¹⁵ Fourth, all of the relevant standard errors are small in magnitude, which implies correspondingly narrow confidence intervals. Taken together, we can be reasonably confident that the ETI has not had any substantial positive effect on aggregate youth employment probabilities in the short run.

7. ROBUSTNESS TESTS

In this section, we discuss the major concerns relating to our analyses and address them to the extent possible.

7.1 Robustness Checks on the Validity of Using a Difference-in-Differences Estimator

There are three main assumptions required for a DD estimator to provide an unbiased estimate of the average-treatment effect of the ETI. First, we require that the trend of the dependent variable was the same for both the youth and the non-youth in the pre-period. To test this, we estimated our DD regressions from Table 6 above, but included an interaction term youth*trend. This variable allows for the trends to differ between the treatment and control groups in the pre-period.¹⁶ None of the coefficients on the youth*trend interaction term are statistically significant, and they are all extremely small in magnitude, with values below 0.001 in absolute value. We are thus empirically satisfied that our first requirement is met.

The second requirement is that there are no contemporary shocks in the economic environment that affect the treatment and control group differently. This requirement is impossible to test, but the time trend for both youth and non-youth employment rates are quite stable across the Pre- and Post- periods. This is shown graphically in Fig. 2, and is observable for the control groups by investigating the coefficients on Post1 and Post2 in the regressions in Table 6 above, all of which are small and statistically insignificant. Similarly, the results from the before-after estimates indicated that there was no trend break in the employment probabilities amongst youth either.

The third requirement is that the ETI did not affect the employment probabilities of the control group. This condition could be violated if there are significant substitution or complementary effects towards the groups of non-youth. We are not too concerned about this for two reasons. Firstly, as discussed in the preceding paragraph, neither the youth nor the non-youth show any evidence of a trend break in their employment

¹⁵ The one exception is the coefficient corresponding to youth_post1 in column 3. In this case, it is negative and statistically significantly different from zero.

¹⁶ The results of these regressions are available in the appendix of the working paper version of this paper, available at: http://opensaldru.uct.ac.za/handle/11090/766.
probabilities in the post-period. Secondly, this critique does not apply to the before-after estimator, and the before-after estimator and the difference-in-differences estimator both suggest the same conclusions.

7.2 Sensitivity of the Results to our Choice of Dependent Variable
Our results up to this point indicate that the ETI has not had any significant positive effect on aggregate youth employment probabilities. It may, however, still be the case that the ETI is having an effect on the youth labour market. One possibility is that the “net rate” at which youth are finding jobs has indeed increased, but that the time period has been too short for this to have an observable impact on the aggregate youth employment probabilities.17 Alternatively, there may be an increase in the rate at which youth find employment but this is offset by a corresponding increase in the rate at which youth lose employment. This would be rational from a firm’s perspective as only newly hired youth are eligible for the ETI. A different version of this behaviour would be that firms could hire youth who were previously employed in a different firm. This could result in a stable aggregate labour absorption rate, but a higher level of churning in the youth labour market.

To explore these possible scenarios, we regress the variables that capture whether an employed youth was recently employed on the trend variable and the quarter dummies, as well as the post1 and post2 indicator variables. In essence, we are testing whether there is evidence of a positive trend break in the job finding rate amongst youth.18 We perform the analogous analysis for a recent job loss on the subset of youth who are not employed. The results of our regressions are presented in Table 7. In column 1, with the dependent variable being a recent job within the previous three months, we are interested in whether the coefficients on post1 or post2 are positive and statistically significant. The coefficient on post1 is 0.00766, which is positive, but small and not statistically significant. The coefficient on post2 is −0.00337, which is negative, but not statistically significant. In the second column, we are only interested in the coefficient on post2, as it is only in the second quarter of 2014 that a job found within the previous six months could be attributed to the introduction of the ETI. This point estimate is positive but small at 0.00364, and it is also not statistically significant. We thus find no evidence that the job finding rate had increased as a response to the introduction of the ETI.

With regard to job loss within the previous three or six months, we also find no evidence of a trend break in either the first or second quarters of 2014. The coefficients on the post1 and post2 variables are all small in magnitude and are not statistically significant. In summation, we find no evidence that the ETI has led to any increases in the rate at which youth find jobs. We also find no evidence to suggest that the ETI had any impact on the rate at which youth lose jobs. Thus, the churning hypothesis has no empirical support, to the extent that this is captured by the proxies that we used from the QLFS.

17 By “net rate” we mean the rate at which youth who are not employed find employment, less the rate at which youth who are employed lose jobs, adjusted for the relevant sub-population sizes.
18 The use of repeated cross sections is not ideal for this purpose. Unfortunately, at present there is no longitudinal data that covers the period in which the ETI came into effect.
7.3 Sample Sizes and Statistical Power

An additional concern with our results relates to the sample size and statistical power that we have, in terms of our ability to identify any effects of the ETI. A priori, this is a valid concern as, despite our large sample sizes, the plausible parameter values that the ETI could realistically generate are probably quite small.

To address this, we performed multiple one-tailed hypothesis tests on the coefficients on youth_post1 and youth_post2 from our DD regressions, over a range of values. To be precise, we tested whether the true population parameter corresponding to the relevant coefficient was greater than or equal to 0.01, 0.005, 0.0025 and 0.001. For the coefficient on post2 from our preferred regression, we reject the null hypothesis that the true population parameter is greater than or equal to 0.005 at the 5% level of significance. Thus, the effects of the ETI on youth employment probabilities in the short run are, at best, small in magnitude.

7.4 Issues Relating to Model Specification and Estimation Samples

In addition to the choice of the dependent variable, one needs to consider whether our results are sensitive to the choice of specification or estimation sample. For example, while the law came into effect on the 1st of January 2014, any new youth employee hired from the 1st of October 2013 is potentially eligible for the ETI. Moreover, since firms might reasonably have anticipated the law coming into effect, they may have started to employ more youth in anticipation of the law. This can be tested by assuming that QLFS2013:4 is effectively also a “treated” period. Another eligibility criterion is that government offices, state parastatals and private households are not eligible for the ETI.

Table 7. Regressions to test for changes in outflows or inflows into employment amongst youth

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<td>(0.00434)</td>
<td>(0.00546)</td>
<td>(0.00170)</td>
<td>(0.00210)</td>
</tr>
<tr>
<td>3.qtr</td>
<td>−0.0215***</td>
<td>−0.00786</td>
<td>−0.00661***</td>
<td>−0.00732***</td>
</tr>
<tr>
<td></td>
<td>(0.00446)</td>
<td>(0.00633)</td>
<td>(0.00160)</td>
<td>(0.00218)</td>
</tr>
<tr>
<td>4.qtr</td>
<td>−0.0286***</td>
<td>−0.0176***</td>
<td>−0.00792***</td>
<td>−0.0100***</td>
</tr>
<tr>
<td></td>
<td>(0.00452)</td>
<td>(0.00579)</td>
<td>(0.00178)</td>
<td>(0.00214)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.0815***</td>
<td>0.165***</td>
<td>0.0390***</td>
<td>0.0631***</td>
</tr>
<tr>
<td></td>
<td>(0.00407)</td>
<td>(0.00634)</td>
<td>(0.00200)</td>
<td>(0.00269)</td>
</tr>
<tr>
<td>Observations</td>
<td>66,764</td>
<td>66,764</td>
<td>180,177</td>
<td>180,177</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.003</td>
<td>0.002</td>
<td>0.000</td>
<td>0.000</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

19 The p-value from each of these hypothesis tests are presented in Appendix Table A3 of our working paper, which is available online as a SALDRU working paper.

20 The hypothesis tests from the other regressions indicate qualitatively the same result.
By considering only the aggregate rate of youth employment, we might be missing important nuances in the effects of the law that occur only amongst employees in private firms. Alternatively, it may be that all of the non-youth adults combined is not the appropriate comparison group, and we may thus want to consider comparing youth to groups of adults defined by more narrow age ranges.

To investigate the robustness of our findings to these concerns, we estimated several different regression models. We varied the empirical specification, or the dependent variable, or the estimation sample; and our overall findings are extremely robust. In all cases, the coefficients and corresponding standard errors are small in magnitude, the coefficients are seldom statistically significant, and in the few cases where they are significant, they are negative in sign.21

8. CONCLUSIONS

South Africa’s ETI was introduced in January 2014 to address a large and persistent unemployment problem amongst youth. In this paper, we make use of fourteen waves of nationally representative QLFS data to investigate the short run effects of the ETI on the aggregate employment probabilities amongst youth.

In the first six months since the introduction of the ETI, we find no evidence that the program had any substantial, positive and statistically significant effect on aggregate youth employment probabilities. Our preferred estimate of the effects of the ETI has a 95% confidence interval of (−0.017, 0.006). We can thus be fairly confident that, at best, the effects of the ETI are small in magnitude. We also find no evidence that the rate at which youth find or lose employment has changed since the ETI was introduced.

There are several reasons why the ETI may not be having an impact on youth employment probabilities in the short run, and these explanations are not mutually exclusive. First, it may be that the ETI has not had any effects yet, but that with time it may become more effective. Second, the ETI is targeted at medium sized and larger formal sector firms, which would further limit its potential impact.22 Third, the value of the incentive may be too low to substantially affect firms’ hiring decisions. Fourth, it may be the case that the decision makers who decide on whether to employ a person or not are personally unaffected by the ETI.

From a policy perspective, our study raises several issues. First, the youth unemployment problem remains as large as it did prior to the introduction of the ETI. In addition, the scale of the program is quite modest relative to the number of unemployed youth. Second, a profit maximising firm will consider the net marginal cost of an employee, including any incentive attached to the marginal employee, relative to the marginal benefit of employing this marginal employee. The aggregate level of tax relief received due to the ETI is not relevant for a profit maximising firm’s decision on whether or not to hire an additional employee. This raises questions about the potential of the

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21 We cannot present all of these results due to space constraints. Most of these are presented in the working paper version of this article. If they are not available in the working paper, then they can be obtained from the authors upon request.

22 This expectation about which firms are likely to benefit from the ETI is a conjecture based on the eligibility criteria for firms to claim tax relief under the ETI.
program to address the youth unemployment problem. Third, the lack of effectiveness of the ETI has implications for the efficacy of policy from a public finance perspective. Fourth, for a modest increase in the number of employment positions available for youth, the effective aggregate subsidy per new post from the government’s perspective could still be exceptionally large. This is because all new appointments can benefit from the ETI, but only new appointments in new positions will have an impact on the labour absorption rate. This possibility is made even more likely because the youth labour market is characterised by relatively high levels of turnover.23

Our overall conclusions are somewhat disappointing. The ETI does not seem to be increasing youth employment levels substantially in the short run, and there is some chance that it might never have the impact that was desired. At the same time, the labour market issue of youth unemployment, and the public finance implications of the ETI, are both extremely important policy topics in contemporary South Africa. Further research is required to understand all of the many layers of complexity of both the ETI as well as the youth unemployment problem.

REFERENCES


CONGRESS OF SOUTH AFRICAN TRADE UNIONS (COSATU). (2012). The Youth Wage Subsidy in South Africa: Response of the Congress of South African Trade Unions (To the National Treasury and Democratic Alliance). COSATU.


23 In the four waves of QLFS data from 2013, the average proportion of recent appointments in formal sector appointments was 0.064 amongst youth aged 18–29, in each wave. Amongst almost youth (30–34), prime aged (35–49) and older adult (50–64) respondents, the corresponding proportions were 0.038, 0.025 and 0.014, respectively.