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The effects of the Employment Tax Incentive on South African employment

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Abstract: South Africa's Employment Tax Incentive, launched in 2014, aimed to address low youth employment by reducing the cost of hiring young workers. We make use of anonymized tax administrative data from the 2012–2015 tax years to examine the effect of the Incentive on youth employment. We match firms claiming the subsidy with similar firms not claiming the subsidy and observe their hiring behaviour before and after the implementation of the policy. We find no statistically significant impact on youth employment on average. We see a positive and statistically significant effect on youth and non-youth employment in firms with fewer than 200 employees. However, we cannot distinguish whether the increase is due to the policy or to employment growth within the firm.

Keywords: wage subsidy, South Africa, difference-in-differences, youth, unemployment, Employment Tax Incentive
JEL classification: H25, H32, J23, J38

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1 Introduction

The post-apartheid period in South Africa is marked by low labour force participation, high unemployment, and slow growth in labour demand (Leibbrandt et al. 2010). We also see an increase in youth participation in the labour force since 1994. There are two possible explanations for this: previously discouraged youth may be more actively seeking employment; and there may have been an increase in the number of new youth entrants to the labour market (Leibbrandt et al. 2010). Normally, when labour supply increases faster than labour demand, the result is a decline in wages. However, in South Africa, there has been an increase in unemployment (Abel et al. 2014; Levinsohn 2014). Many have tried to explain the country's high unemployment rates (Banerjee et al. 2008; Rodrik 2008) and the solutions proposed are often viewed as controversial (Levinsohn 2014).

In an effort to reduce youth unemployment specifically, the South African government launched the Employment Tax Incentive (ETI) in January 2014. The ETI is a direct intervention in the labour market aimed at stimulating demand for youth labour. The policy gives firms a tax credit for hiring individuals between the ages of 18 and 29 years.

One of the imperfections in the South African labour market is the effect of collective bargaining or negotiated union wages on wages for both union and non-union workers, which results in wages that are too high to clear the market (Levinsohn 2014). The higher wages allow firms to choose more experienced workers from the surplus of willing workers, creating a bias against younger, less experienced workers. The wage subsidy implicit in the ETI lowers the cost of the young, inexperienced worker to employers without lowering the wages of the workers themselves. This mechanism reduces the risk to firms when hiring.

Almost three years after the ETI began, the impact on the labour market, for both younger and older workers, is unclear. Levinsohn et al. (2014) conducted a randomized control trial before the launch of the ETI in which participants were given a wage subsidy voucher to present to employers when seeking employment. They found that those with a wage subsidy voucher were more likely to be employed, and that the employment effect persisted even after the subsidy had ended. Ranchhod and Finn (2014, 2015), on the other hand, found no change in the employment probabilities for youth when they analysed the ETI in the first year it was implemented.

The main contribution of the present work is our analysis of the ETI at the firm level. We do this by examining the growth in the employment of youth. In particular, we quantify the jobs created for young and old workers. We also examine the cost of the policy in relation to the number of new jobs created.

The paper is organized in the following manner: In the next section, we look at the details of the policy and at some of the policy evaluation that has been conducted in South Africa. Section 3 reviews the international literature on wage subsidies with a focus on studies that use administrative data. Section 4 deals with the data used in our analysis. Section 5 presents descriptive statistics, our empirical analysis, and the results of our evaluation of the ETI. Finally, in Section 6, we draw some conclusions.

2 The Employment Tax Incentive policy

Several studies conducted after the ETI was proposed in 2010 were of the opinion that it had the potential to change the employment prospects for youth (Burns et al. 2010; Levinsohn and Pugatch 2014; Levinsohn et al. 2014; Mtembu and Govender 2015; National Treasury 2011).

Burns et al. (2010) argued that the wage subsidy might be successful in creating jobs in South Africa if it was associated with skills training, especially in industries that are sensitive to labour costs, and should have a focus on youth. Levinsohn and Pugatch (2014) suggested that the wage subsidy could decrease the share of youth experiencing long-term unemployment.

Levinsohn et al. (2014) conducted a randomized control trial before the policy was enacted to examine how a wage subsidy might affect youth unemployment in the South African context. Participants in the trial were given a voucher for a wage subsidy that the firm could claim monthly for up to six months. The authors found that participants who were given a wage subsidy voucher were more likely to be in waged employment both one year and two years after they were given the voucher.

Mtembu and Govender (2015) examined the perception of the wage subsidy among unemployed youth and employers in Kwazulu-Natal and found that both parties were in support of the policy, hoping that it would decrease youth unemployment and ease the wage burden.

The ETI policy, which was enacted in December 2013, was implemented on 1 January 2014 and retroactively applied to new hires from 1 October 2013. The ETI was to run for a period of three years, ending on 31 December 2016. It aimed to subsidize 423,000 youth jobs and create 178,000 new youth jobs over the policy period at a cost to the government of ZAR5 billion (National Treasury 2011). We outline the details of the policy below.

2.1 Policy details

The ETI is subject to a set of criteria for the firms wishing to claim the subsidy as well as for the individuals for whom firms can claim the subsidy. The firm-level criteria are:

- Firms in the public sector are ineligible.
- Firms need to be registered for Pay-As-You-Earn (PAYE), income tax being deducted by the firm from an employee's income.
- Claims can be made only if firms do not owe the South African Revenue Service (SARS) any money.
- Claims can be made for individuals between the ages of 18 and 29 years hired after 1 October 2013 and earning less than ZAR6,000 per month but more than the minimum wage.
- No older worker must be displaced to make way for an eligible worker.

The policy does not require any training for the employed youth and is available to all industries. No requirements are placed on length of unemployment for eligible youth.

To reduce the displacement of older workers that might result from the policy, there is a ZAR30,000 penalty per employee displaced. Penalties are also imposed on firms that claim the ETI for workers who are paid less than the minimum wage.

Firms can claim the subsidy for a 24-month period for an eligible employee. However, the amount of the subsidy is greater for the first 12 months of the employment contract than the second 12 months. The amount claimed per employee is based on the employee's salary, as shown in Table 1.

Table 1: ETI monthly subsidy per employee

	Monthly subsidy	
Monthly pay (ZAR)	First 12 months	Next 12 months
0–2,000[1]	50% of monthly pay	25% of monthly pay
2,001–4,000	ZAR1,000	ZAR500
4,001–6,000	ZAR1,000 - 0.5 x (monthly pay - 4,000)	ZAR1,000 - 0.25 x (monthly pay - 4,000)

Source: Authors' computation based on Employment Tax Incentive Act (2013).

2.2 Policy evaluation

As the policy period is still under way, only a handful of studies have been published since its implementation. De Jongh et al. (2016) studied perceptions of the ETI among 13 local businesses in the Vaal triangle in Gauteng. The authors found that firms were in support of the policy, but that 8 of the 10 businesses claiming subsidies had not created any new jobs. They found that firms were more concerned with the skill level of young employees than they were interested in the tax incentive.

Odendaal (2016) conducted a comparative analysis of the ETI and similar policies enacted in other countries. The author suggested that the policy, as it was enacted, was unlikely to reach its goal of combatting youth unemployment, citing lack of firm awareness of the policy, the short duration of the ETI, and the absence of compulsory skills training among other reasons for his conclusion.

Ranchhod and Finn (2014) examined the policy six months after its inception. They measured the effects of the ETI using nationally representative survey data. Using an individual-level difference-in-differences (DID) approach, the authors found no change in youth employment probabilities in the first six months after the policy was implemented. Extending their analysis to the first year of the policy (Ranchhod and Finn 2015), they did not find a statistically significant change in the probability of youth employment.

The limited and conflicting results from the quantitative and qualitative literature leaves fertile ground for further evaluation of the policy. Access to administrative tax data for individuals and firms provides us with an opportunity to examine the policy at the firm level. In the next section, we look to the international literature for the analysis of similar policies elsewhere.

3 Previous literature

We examine the international literature on wage subsidies with a focus on studies that use administrative data. Previous studies on the ETI have not made use of administrative data and thus we look to the international literature for guidance on the methodology.

Betcherman et al. (2010) implement a DID method to examine the effects of two successive employment subsidy policies in Turkey. The authors use monthly administrative panel data for the period 2002–2005. The employment subsidy policies were expanded in a progressive manner across neighbouring provinces, a fact that the authors use to identify appropriate treatment and control groups for estimation. The two policies varied in their incentives, which included a subsidy on social security contributions, an income tax subsidy, an energy consumption subsidy, and a five-year land subsidy.

[1] Minimum wages occur only in some sectors. The subsidy applies to part-time employment covering those earning between ZAR0 and ZAR2,000.

The authors find that the employment subsidy schemes led to significant net increases in registered jobs in provinces where the policy was implemented, despite deadweight loss (considerable in the case of the first policy). Furthermore, they find that the employment subsidy policies led to an increase in the number of registered firms; in other words, informal or unregistered firms were incentivized to register to benefit from the policy.

Crichton and Maré (2013) use a propensity score matching approach to analyse a wage subsidy policy in New Zealand. The authors use tax administrative data covering a 10-year period from 2000 to 2010. The wage subsidy was targeted at disadvantaged jobseekers, lasted for up to one year, and represented approximately 50 per cent of the weekly wage. Firms employing a subsidized worker were matched to a subset of firms that had a similar likelihood of hiring a subsidized worker but that had not yet hired one. They restricted their sample to firms continuously hiring in the three months prior to hiring a subsidized worker to ensure that firms with similar employment trends were matched. The probability of hiring a subsidized worker is modelled as a function of past employment trends, workforce composition, industry, and region. The authors run separate logistic regressions in firm size categories. Each treated firm is matched to a minimum of five control firms. The authors find that firms increase the hiring of subsidized workers and see an increase in their total employment relative to the matched comparison firm. The authors cannot, however, establish whether the growth in total employment is due to the subsidy, as firms are increasing their employment at the same time.

Rotger and Arendt (2010) use a DID matching estimator to estimate the employment effects of a wage subsidy on small private firms in Denmark. The wage subsidy amounted to approximately 50 per cent of the monthly wage and was available for up to one year. The authors use monthly administrative data including individual, firm, and firm-level data for 10 months in 2006. The authors use a logit model to estimate the propensity score for treatment before conducting the DID estimation. They find little evidence of deadweight loss and substitution effects. Their results show that the wage subsidy has a net employment effect of 0.26 employees who would not have been hired without the subsidy.

Kaiser and Kuhn (2016) measure the effects of another Danish wage subsidy programme, which aimed to increase the employment of highly skilled workers. The subsidy lasted between 6 and 12 months and subsidized up to half of eligible employees' wages. The authors examine the performance of the firms that hired subsidized workers, using a sample of 316 firms. They match treatment and potential control groups on observed characteristics in the year before the wage subsidy programme was introduced. They estimate a dynamic fixed-effects regression and find a positive significant effect on the number of employees per firm in the year of the programme.

Bruhn (2016) examines the effect of a wage subsidy on firms in the manufacturing industry in Mexico. Subsidies were granted to firms that retained workers instead of letting them go during an economic crisis. The policy lasted eight months. Monthly administrative data for the period 2004–2013 is used in a propensity score matching and DID regression. A positive but not statistically significant effect is found. The effect ranges from a 5 per cent to a 13 per cent increase in employment. After the policy, the author finds that employment at firms in eligible industries recovered from crises more quickly than in ineligible industries.

Hujer et al. (2002) estimate the effects of wage subsidies on labour demand in West Germany. Subsidies were targeted at individuals with poor labour market prospects, including the long-term unemployed and those over 50 years old. The subsidy ranged between 30 and 80 per cent of the monthly wage, depending on the programme, and lasted around 24 months. The authors make use of annual firm survey data to calculate the effect of the subsidy using a conditional DID approach. They measure the change in labour demand by examining the change in actual employment at the firm. No significant effect of the subsidy on employment is found. They suggest that this is due to displacement or substitution effects.

Kangasharju (2007) examines the effect of a wage subsidy on employment in subsidized firms in Finland. The subsidy was available to the long-term unemployed and unemployed youth under 25 years old and was equivalent to approximately one-third of their average monthly wages. The subsidy had a maximum length of 10 months. The author uses a DID approach preceded by regression and matching methods using annual tax administrative data for the period 1995 to 2002. The author measures the change in employment by the change in payroll, and concludes that there is roughly a 9 per cent increase in employment at the subsidized firm based on the change in payroll.

In summary, wage subsidy programmes are mostly targeted at the unemployed with low labour market prospects. This often includes youth, the long-term unemployed, and in some cases those over 50. Some of the literature reports no statistically significant effects of the wage subsidy on employment. In many cases, the observed effects are modest, ranging from 0.17 to 1.09 additional jobs (Crichton and Maré 2013; Kaiser and Kuhn 2016; Rotger and Arendt 2010). Kangasharju (2007) and Betcherman et al. (2010), on the other hand, see a 5 and 12 per cent increase in employment in their respective studies. Many of these studies suggest that the long-term effects of the policies are either modest or short-lived. Only a few estimate deadweight loss and substitution effects, as these are often harder to measure. Betcherman et al. (2010), additionally, look at expenditure on the subsidy programmes to estimate the cost of job creation. They warn that the cost of subsidized employment is especially high in cases where there is large deadweight loss.

In terms of methodology used, all studies have access to data before and after the policy, which is well suited to a DID estimation. The studies face the same challenge of creating a convincing counterfactual to firms that take up the policy. Most match firms on a set of observables to deal with the selection issue. Betcherman et al. (2010) and Bruhn (2016) exploit implementation and eligibility criteria to create suitable control groups.

In the next section, we describe our dataset, which includes data from before and after the policy implementation.

4 Data

The South African Revenue Service (SARS) made anonymized tax data available to measure the effects of the ETI through a joint SARS/National Treasury/UNU-WIDER initiative. These data include, but are not limited to, Company Income Tax (CIT), Employee Tax Certificate (or IRP5), and EMP501 data. The data are reported by tax year. The tax year for individuals in South Africa runs from 1 March to 28/29 February. This means that for the 2014 tax year we see the ETI being claimed only for two months, whereas in the 2015 tax year, we see the ETI being claimed for the full year, starting 1 March 2014 and ending 28 February 2015. At the time of writing, IRP5 data is available only until 2015 and CIT data only until 2014. Companies have 12 months from their financial year end in which to complete their tax returns; thus, some company data may be incomplete.

4.1 Description of IRP5 data

The IRP5 data is job-level administrative tax data. For each job with an annual income greater than ZAR2,000 there is an entry if the firm is registered for PAYE. Employees who earn less than ZAR2,000 in a tax year are not issued with an IRP5 form.

If an individual has multiple jobs in a year (within the same firm or in different firms), that individual will be seen multiple times in the data. Government or public entities also issue IRP5

forms to their employees; consequently, the data includes employees in both the formal private sector and the public sector. An individual working in a firm that is not registered for PAYE will not be included in the IRP5 data. We use IRP5 data from 2012 to 2015.

The individual identifier is an anonymized South African national identity (ID) number. Where no South African ID number existed but an anonymized passport number was present, we assume that the individual is a non-resident or non-citizen of South Africa.

The data also includes some basic individual-level information—dates of the start and end of employment, an annual amount for the ETI claimed—and firm-level information, such as income source and amount earned.

The PAYE reference number (or payroll reference) serves as the firm identifier in the IRP5 data. Larger firms may have several payrolls and therefore several PAYE reference numbers but only one company income tax reference number. Within the CIT data, public firms have no company income tax reference number.

For the analysis, we organize the data by the PAYE reference number within the tax year. This allows us to create a panel of firms. The analysis is thus restricted to data on formal private sector and public sector firms.

4.2 Description of the CIT-IRP5 panel

The Company Income Tax-IRP5 (CIT-IRP5) panel was created through a joint initiative between SARS, the National Treasury, and UNU-WIDER. The dataset matches employer–employee variables from the IRP5 and CIT datasets as well as value added tax data from firms and customs data from firms that trade. The panel includes tax information from 2008 to 2014 and makes use of the company income tax reference number as the unique identifier for the firm. Pieterse et al. (2016) provide a more detailed description of the CIT-IRP5 panel. They discuss how the panel was constructed and any biases it might contain, and compare the panel with other data sources.

To complement our analysis, we merge firm-level variables from the CIT-IRP5 panel with the IRP5 panel we create. Firm-level variables from the CIT-IRP5 panel include firm sales, age of firm, firm assets, firm debt, and firm labour broker status. The CIT and IRP5 data are extractions of data from the revenue collection by SARS.

4.3 Data challenges

The IRP5 data is unaudited, presenting some challenges when conducting any analysis. We discuss below some of the issues that affect our analysis.

Missing South African ID numbers

We use the anonymized South African ID number to differentiate individuals in the data. However, there are approximately 330,000 observations per year (1 per cent of all observations) without a South African ID number. We assume that those without a South African ID number are foreigners. We are comfortable with this assumption, as foreigners are not eligible for the ETI.

Fraudulent ETI claims

Each IRP5 certificate after 2013 contains information on whether the ETI was claimed and the amount of the claim. There are many instances where an ETI claim is indicated but no ETI claim amount is shown or where an ETI claim amount is indicated but no ETI claim is registered.

The ETI is targeted at a specific group of youth—individuals between the ages of 18 and 29, starting work after 1 October 2013, earning less than ZAR6,000 per month but more than the minimum wage, and working in the private sector—and the data includes many claims for the ETI from people who do not meet these eligibility criteria. We identify five types of fraudulent claim in the data:

1. Over age: those more than 30 years old at the start of their work period
2. Under age: those younger than 18 years at the start of their work period
3. Before the policy: Many claims in the 2014 tax year originated from individuals hired before 1 October 2013.
4. Public sector: claims from those employed in the public sector
5. Over-claimed: The maximum possible claim amount for an individual per month is ZAR1,000. The ETI came into effect on 1 January 2014; thus for the 2014 tax year the maximum claim per individual is ZAR2,000. The ETI was in effect for the full 2015 tax year; thus the maximum claim for 2015 is ZAR12,000 per eligible employee. We identified claims in excess of these amounts.

In cases where claims are indicated but the individual is ineligible for the claim, we set the ETI claim value to zero. In cases where individuals are eligible for the ETI but their claims exceed the maximum, we set their claim to the maximum per tax year.

The table below presents the percentages of fraudulent ETI claims in the 2014 and 2015 tax years.

Table 2: Types of fraudulent claim by tax year (%)

Type of fraudulent claim	2014	2015
Over age	1.62	2.65
Under age	0.25	0.36
Before policy	37.9	-
Public sector	0.35	0.17
Over-claimed	0.15	0.12
Total number of fraudulent claims	112,623	31,167
Percentage of fraudulent claims	40%	3%
Total number of claims (including fraudulent claims)	279,752	942,645

Source: Authors' estimates based on IRP5 data.

The figures in Table 2 may be understated in two categories: over age and over-claimed. First, we cannot establish that claims ceased when an employee turned 30, as we make use of annual data. Second, employees earning between ZAR2,000 and ZAR4,000 have a maximum claim of ZAR1,000 per month. For employees earning ZAR1,500 per month the maximum claim in the first 12 months is ZAR750 per month. By setting the maximum claims to ZAR2,000 and ZAR12,000 for 2014 and 2015, respectively, we may be missing employees over-claiming in the ZAR0–2,000 and ZAR4,000–6,000 categories of monthly income.

In Table 2 we see that in the first two months of the ETI, 40 per cent of the claims made are fraudulent. This is mainly due to the large number of claims made for individuals who started work before 1 October 2013. The number of fraudulent claims is approximately 3 per cent in the 2015 tax year.

4.4 Data cleaning

To evaluate the ETI claims we clean and construct the data set as follows:

- We merge the IRP5 data for 2012–2015 tax years to form a panel dataset.

- We drop any employee in the dataset who is 15 years old or younger.
- We drop any employee with a missing ID number.
- We drop any employee with duplicate ID numbers in any tax year and no income. We assume that the duplicate entry is due to a revision.
- We retain only one entry where we find duplicate ID numbers in a tax year if they report multiple entries with income values. We take an average of the different income amounts reported.
- We set the ETI claim to the maximum per tax year for those eligible for the ETI but who have over-claimed.
- We amend the ETI claim indicator where the individual is not eligible.
- We match company tax reference numbers with PAYE reference numbers to merge firm variables from the CIT-IRP5 panel.

Table 3 reports the number of observations after our data-cleaning process. It also displays the numbers of employees dropped on account of missing ID numbers.

Table 3: Data description by tax year

	2012	2013	2014	2015
Number of observations	12,789,050	13,538,687	13,922,668	14,343,813
Number of missing ID numbers	328,358	379,961	395,310	399,765

Source: Authors' estimates based on IRP5 data.

Focusing on the ETI, Table 4 reports the number of individual claims for the ETI and the number of firms claiming the ETI. The figures in Table 4 reflect the ETI claims after the fraudulent claims have been accounted for.

Table 4: Summary statistics for ETI claims by tax year

	2014	2015	Both
No. of ETI claims	165,700	898,797	105,670
No. of firms claiming ETI	14,551	34,654	11,883
No. of firms not claiming ETI	236,534	208,472	180,269
Percentage of ETI-claiming firms	6%	14%	6%
Total number of firms	251,085	243,126	192,512

Source: Authors' estimates based on IRP5 data.

The take-up of the ETI appears to be low in 2014. However, as explained above, the 2014 tax year covers only two months of the policy period, during which firms may not have hired any eligible youth.

5 Empirical analysis and results

We use a conditional difference-in-differences (DID) methodology in this paper. This is similar to the approaches of Bruhn (2016), Hujer et al. (2002), and Rotger and Arendt (2010) (see Section 3). We have a panel of firms created from aggregated individual and firm-level data, detailing the take-up of the ETI. The data includes information pre- and post-implementation of the policy, which lends itself well to the methods proposed in the literature—that is, a DID methodology.

First, we present some descriptive statistics for the population of firms. This comprises the means of the variables included in our model, as well as the take-up rate. The descriptive statistics also provide us with an indication of the types of firm that are benefitting from the ETI. We analyse, descriptively, the changes in the hiring and separation rates for youth and non-youth to describe some of the employment patterns in the firms. Then we describe our methodology and the changes we make to our panel to suit our approach. We match treated and control firms and describe their differences to assess the quality of the matching. Finally, we estimate our DID model for youth employment. We end this section with a description of the cost of the policy vis-à-vis the number of jobs created, as estimated from our results.

5.1 Descriptive statistics

Table 5 summarizes the industries benefitting from the ETI. Manufacturing, wholesale and retail, and financial and insurance services account for more than 55 per cent of ETI claims in both 2014 and 2015. Claims for the subsidy are highest in the manufacturing industry and lowest in the information and communication industry. Manufacturing has a greater need for unskilled and semi-skilled labour, while the information and communication sector tends to require higher-skilled labour.

Table 5: ETI-claiming versus all firms by tax year and industry sector (%)

	2014		2015	
Industry sector	ETI	All	ETI	All
Agriculture, forestry & fishing	2.8	3.4	3.8	3.4
Mining and quarrying	1.4	0.9	1.3	1.0
Manufacturing	26.3	21.6	25.7	21.4
Electricity, gas, steam, & air-conditioning supply	0.4	0.5	0.4	0.5
Water supply, sewerage, & waste management	0.5	0.8	0.6	0.8
Construction	5.7	6.4	5.9	6.3
Wholesale & retail	18.3	14.2	17.8	14.1
Transportation & storage	3.4	3.2	3.3	3.2
Accommodation & food service activities	7.6	3.8	7.2	3.9
Information & communication	0.3	0.3	0.3	0.4
Financial & insurance services	12.7	14.9	11.9	14.9
Real estate activities	0.6	2.9	1.0	2.9
Professional, scientific, & technical	9.2	10.7	9.4	10.8
Administrative & support service activities	1.5	1.1	1.5	1.1
Public administration & compulsory social security	0.0	1.2	0.0	1.3
Education	2.9	2.3	2.8	2.3
Human health & social work activities	2.3	5.9	2.8	6.1
Arts, entertainment, & recreation	1.2	1.0	1.2	1.0
Other service activities	3.0	4.8	3.2	4.8
Total number of employers	13,601	238,238	32,414	229,208

Note: Industry codes are based on the International Standard Industrial Classification of All Economic Activities Revision 4 (ISIC4), available at: http://unstats.un.org/unsd/cr/registry/regcst.asp?Cl=27.

Source: Authors' estimates based on IRP5 data.

In Table 6, we examine the take-up rate by firm size in terms of weighted number of employees per firm in the 2014 and 2015 tax years. We define the take-up rate as the percentage of firms claiming the ETI divided by the total number of firms in the category. We see very high take-up

rates from larger firms in comparison with smaller firms. We return to this important point later in analysing the impact of the policy.

Table 6: ETI-claiming and all firms by tax year and firm size category

		2014			2015	
Number of employees	ETI	All	Take-up	ETI	All	Take-up
0–10	2,877	159,607	2%	7,967	152,821	5%
11–50	5,418	69,603	8%	14,861	68,356	22%
51–200	4,020	16,619	24%	8,315	16,682	50%
201+	2,193	5,256	42%	3,353	5,267	64%
Total no. of firms	14,508	251,085	6%	34,496	243,126	14%

Note: Number of employees per firm are weighted by the period worked.

Source: Authors' own estimates based on IRP5 data.

Firm size is likely to be an important determinant of the change in the labour composition at the firm. The effect of hiring an additional youth employee at a firm with 10 employees is very different from that at a firm with 500 employees. The subsidy may mean more to a small firm, allowing them to hire additional youth, while larger firms, already hiring unskilled or semi-skilled youth, might be claiming the subsidy for youth they were already planning to hire, creating a deadweight loss. The evaluation method has to recognize the differences in behaviour of firms of different sizes within its focus on determining general changes in youth-hiring trends with the advent of the ETI.

As can be seen from Table 7, almost 60 per cent of the firms claiming the ETI have a turnover of more than ZAR10 million per annum. The table shows firms with a turnover between ZAR10 million and ZAR50 million per annum as having the highest take-up rate of the ETI. This is consistent with the table above, which shows that larger firms have a higher take-up.

Table 7: ETI-claiming versus all firms by turnover (%)

		2014	
Turnover (rands per annum)	ETI	All	Take-up
0–500,000	2.4	7.8	1.8
500,001–1,000,000	10.7	17.4	3.6
1,000,001–5,000,000	7.8	7.4	6.2
5,000,001–10,000,000	19.2	10.7	10.4
10,000,001–50,000,000	20.8	4.7	25.9
50,000,001 +	39.0	52.1	4.3
Total percentage	100	100	-
Total no. of firms	14,183	243,962	5.8%

Source: Authors' own estimates using the CIT-IRP5 panel.

In Table 8, we describe the age of the firms in our panel. The take-up of the ETI among firms in all age groups is above 6 per cent, but older firms have a higher take-up rate: above 14 per cent.

Table 8: ETI-claiming versus all firms by firm age (%)

		2014	
Age (years)	ETI	All	Take-up
0–1	4.2	3.6	7.6
1–5	13.9	14.5	6.4
6–10	27.3	29.6	6.1
11–20	37.0	37.8	6.5
21–40	12.3	11.9	6.9
40 +	5.3	2.7	13.3
Total percentage	100	100	-
Total number of firms	13,278	200,238	6.6%

Source: Authors' own estimates using the CIT-IRP5 panel.

From Tables 6, 7, and 8 we can see that larger and older firms have a higher take-up rate of the ETI. This is as expected; firms with a greater capacity to hire youth and submit the claim for the subsidy do so within the first two months of the policy. The Manufacturing and Wholesale and Retail sectors are the industries with the largest ETI, take-up as they typically require low- or semi-skilled labour.

Changes in the labour force composition within firms

The ETI imposes some restrictions designed to hinder the substitution of young, subsidized workers for older employees. We look at the hiring and separations of employees to establish whether hiring a subsidized worker leads to substitution for older workers.

We define youth as those aged between 18 and 29 years, as this is the target population for the subsidy. We define non-youth as individuals older than 30 years but younger than the retirement age of 65. We calculate the rate of hiring as the difference in the number of hires between two years divided by the number of hires in the previous year.

Table 9: Rates of hires and separations by tax year and by youth/non-youth category (%)

Hiring	Youth	Non-youth	Total
2013	-1.6	-2.8	-2.2
2014	-7.3	-11.0	-9.2
2015	5.4	4.6	4.9

Separation	Youth	Non-youth	Total
2013	-1.4	5.0	2.1
2014	3.5	10.5	7.9
2015	9.0	15.3	12.6

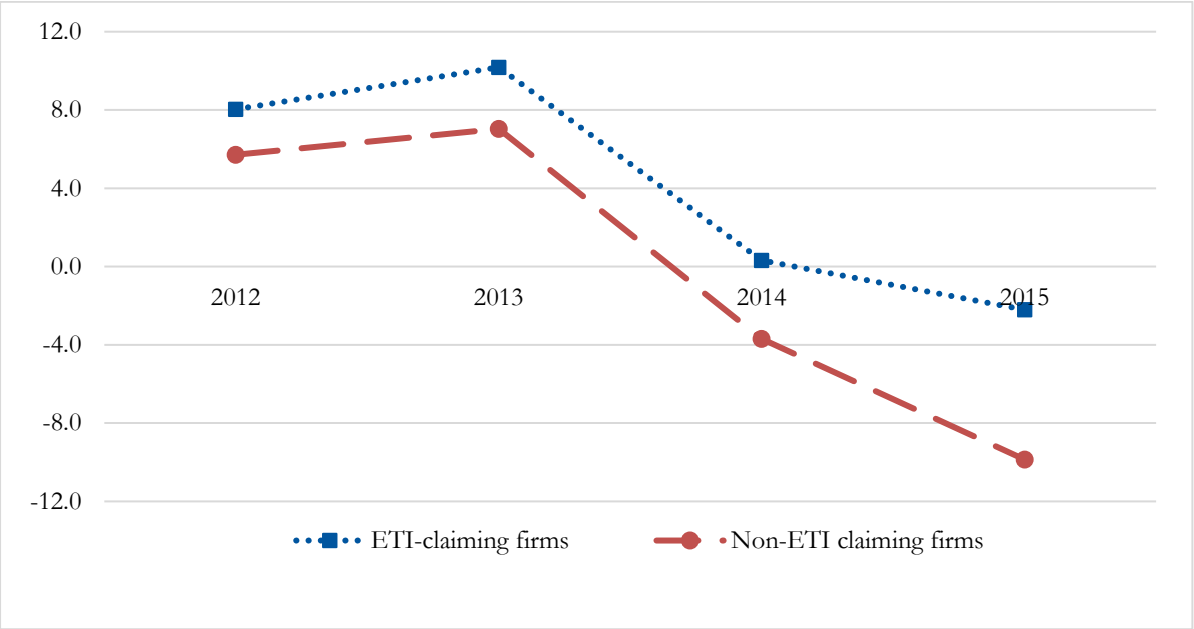
Source: Authors' own estimates based on IRP5 data.

In Table 9 we see a decline in hiring rates between 2013 and 2014 for both youth and non-youth, but an increase in hiring for both youth and non-youth between 2014 and 2015, when the rate of hiring for youth is only slightly higher than the rate for non-youth. We see an increase in separations across all groups from 2013 to 2015 and that youth are less likely to leave a firm than non-youth. Anecdotally, there has been some concern that, once the subsidy ends, youth employees are being dismissed. Although we see an increase in the rate of separations for youth,

we cannot infer that this assertion is true, as the data covers only a little over half of the subsidy period.

In Figure 1, we compare the employment rate of youth for firms claiming the ETI and those not claiming the ETI for the period 2012–2015. We calculate the employment rate as the difference in the number of employed between two years divided by the number of employed in the previous year.

Figure 1: Employment rates for youth, ETI-claiming vs non-ETI claiming firms, 2012–2015



Source: Authors' own estimates based on IRP5 data.

Across the years, firms claiming the ETI have a higher employment rate for youth. Even as the employment rate declines after 2013, the youth employment rate in ETI-claiming firms remains above that of the non-claiming firms and the rate of the decrease is not as steep.

In summary, the rate of separation for non-youth is above 15 per cent while for youth it is 9 per cent in 2015. Firms with a higher youth employment rate can derive greater benefit from the ETI than firms that are not hiring youth or are downsizing. The employment rate may thus be an important determinant for ETI take-up, and we include it in the model we discuss in the next section.

Panel preparation

We conduct further data preparation for our approach. We disregard firms that do not exist in 2013, or are found only in 2013. In order to apply the conditional DID approach, we need firms in both the pre- and post-treatment period.

The table below shows the firm panel used to evaluate the ETI. Of the 276,000 firms observed, more than 78 per cent are observed in 2013, 2014, and 2015. Approximately 33,000 are observed only in 2013 and 2014. Around 25,000 firms are observed in 2013, with no observation in 2014, but observed again in 2015.

Table 10: Firm panel description, 2013–2015

Number of firms	Percentage	Pattern
217,493	78.6	111
33,592	12.1	11.
25,633	9.3	1.1
276,718	100	

Note: The '1' in the pattern column means the firm is observed. The '.' means the firm was not observed in the particular year.

Source: Authors' own estimates based on IRP5 data.

5.2 Empirical approach

We want to test whether there is an increase in youth employment at the firm due to the implementation of the ETI. Wage subsidies decrease the relative cost of employing youth, thereby, in theory, increasing the demand for these workers. At the same time, a decrease in the wage bill lowers production costs, resulting in lower prices and an increase in demand for produced goods—termed the output effect. The effect on non-youth therefore depends on whether the substitution effect or the output effect is larger. We therefore also want to test whether there is any change in non-youth employment because of the ETI.

To analyse the ETI, we use the population of firms that claimed the ETI in the tax years 2014 or 2015. To mitigate potential selection bias, we use a 'conditional difference-in-differences' approach. We calculate propensity scores using a probit regression accounting for a range of firm characteristics. The propensity scores are used to find matches for firms claiming the ETI with firms not claiming the ETI. At this point we check that the treated firms are observationally similar to the control firms. Subsequently, we run a DID regression with the matched sample to consider both observable and unobservable time-invariant factors that may affect the ETI claims. The resulting estimation is the average treatment effect on the treated.

As a scenario, Firm A and Firm B are both expanding their firms and hiring more workers. Both firms are in industries that have a need for semi-skilled youth. In the absence of the subsidy these firms will continue to hire and expand. The subsidy is introduced in 2014 but only Firm A claims the subsidy. Firm B does not claim the subsidy, as it does not know how or has not heard of the ETI. Firms A and B are matched on the basis of their pre-policy characteristics, including sector, firm size, and employment rate. During the 2014 and 2015 tax years, it is assumed that a recession takes place that affects both firms' hiring patterns. Firm A sees an increase in youth employment beyond the recession compared with the period before. A before–after comparison will be limited, as it does not account for any change in the economic environment. A DID approach, on the other hand, takes the economic environment into account and reflects the true effect of claiming the ETI. This means that a DID approach will distinguish any increase in youth employment at Firm A and allow us to attribute it to the policy.

Prior to our estimation of the average treatment effect, we outline how we intend to achieve our results. First, we remove any ineligible firms (public sector firms) from the panel. Second, we consider the observable differences between ETI-claiming and non-claiming firms. We describe in the descriptive section many of these differences. For example, small firms are perhaps over-represented and larger firms under-represented in the non-ETI claiming firms' distribution. We include the observable differences in the calculation of propensity scores.

The propensity score is calculated from a binary probit regression that controls for the firms' choice whether to claim the ETI. We control for continuous variables: firm sales turnover, firm assets, firm age, firm debt, firm mean employee age, firm hiring rate, and firm employment growth

rate. We also control for categorical variables such as firm province, firm size by employment, and firm industry, as well as firm labour broker status. Treatment and control firms are thus weighted composites of all possible alternatives. We assign the propensity scores to the firms in 2014 and 2015.

We match with replacement, allowing each firm to be matched more than once. Matching with replacement increases the quality of the match, as replacement increases the set of possible matches (Abadie and Imbens 2006). We conduct exact matching; in other words, propensity scores for the treatment and control firms are the same value. Firms with no matches are dropped.

Following the matching procedure, we test if the treatment and control groups are balanced to confirm that our treatment group is observationally highly similar to our control group. However, there may still be some unobservable differences between firms after matching: for example, average level of employee education. Other events, such as economic growth, will affect the firms' hiring pattern at the same time as the implementation of the policy.

After establishing a suitable control group, we run a DID regression accounting for any possible changes in the economic environment. Using the matched data, we estimate the effect on the number of all employees, number of youth employees, and number of older employees.

The estimation equation is

$$Y_{it} = \alpha + \beta ETI_i + \delta t_i + \gamma(ETI_i * t_i) + \varepsilon_i$$

where:

Y_{it} = the three respective outcome variables:
- t = time index
- i = firm index
- α = constant term

ETI = dummy variable for ETI claim at the firm
δ = time trend common to control and treatment groups
γ = true effect of treatment, the year-specific treatment effects
β = treatment group-specific effect.

The parameter of interest in the above regression is γ, which estimates the change in employment beyond the number that would have been employed in the absence of the subsidy. More technically, this is termed the average treatment effect on the treated.

5.3 Results of the conditional difference-in-differences estimation

Table 11 displays the differences in means between firms claiming the ETI and firms not claiming. Firms that are more likely to be in the treatment group have greater sales turnover and assets, are slightly older, have a lower mean employee age, and have higher employment and hiring rates. We run a probit model to estimate the propensity score that is used to match the treatment and control firms. The standardized mean difference, in columns 4 and 5 of Table 11, is defined as the difference in means divided by the standard error. A standardized mean difference above 10 per cent indicates an imbalance between the treatment and control groups for the respective covariate. In Table 11, all the standardized mean differences are below 5 per cent, giving us confidence that balance has been achieved between the treatment and control groups.

Table 11: Comparison between firms claiming ETI and matched control firms

	Baseline means		Standardized mean differences	
	Non-ETI	ETI	Raw	Matched
Number of firms	105,670	11,883	72,866	7,126
Firm sales turnover (in millions of rands)	82.7	344	0.133	-0.015
Firm debts (in millions of rands)	120	182	0.013	-0.007
Firm assets (in millions of rands)	163	240	0.014	-0.007
Firm age (in years)	13.88	15.75	0.156	-0.025
Mean employee age (in years)	39.55	35.00	-0.730	-0.023
Firm hiring rate (%)	0.21	2.69	0.033	0.016
Firm employment growth rate (%)	0.31	1.68	0.034	0.021
Firm labour broker status	0.00	0.02	0.141	0.006
Firm province (reference Western Cape)				
- Eastern Cape	0.05	0.06	0.023	0.003
- Northern Cape	0.01	0.02	0.030	-0.012
- KwaZulu-Natal	0.03	0.03	0.011	0.009
- Gauteng	0.13	0.14	0.034	0.001
- North West	0.03	0.03	0.012	0.005
- Mpumalanga	0.46	0.42	-0.081	0.011
- Free State	0.04	0.04	-0.004	0.001
- Limpopo	0.02	0.02	-0.024	0.006
Firm size (reference 0–10 employees)				
11–50 employees	0.40	0.37	-0.049	0.036
51–200 employees	0.07	0.35	0.741	-0.017
201 + employees	0.01	0.19	0.614	-0.010
Firm industry (reference Agriculture, forestry, & fishing)				
Mining & quarrying	0.01	0.01	0.017	0.004
Manufacturing	0.28	0.30	0.053	0.001
Electricity, gas, steam, & air-conditioning supply	0.00	0.00	-0.001	-0.004
Water supply, sewerage, & waste management	0.01	0.01	-0.051	0.012
Construction	0.07	0.06	-0.059	-0.018
Wholesale & retail	0.18	0.21	0.082	0.025
Transportation & storage	0.04	0.04	0.009	0.014
Accommodation & food service activities	0.03	0.08	0.197	0.012
Information & communication	0.00	0.00	-0.011	-0.005
Financial & insurance services	0.14	0.12	-0.070	0.001
Real Estate Activities	0.03	0.01	-0.177	-0.011
Professional, scientific, & technical	0.10	0.07	-0.077	-0.027
Administrative & support service activities	0.01	0.01	0.027	0.000
Education	0.01	0.01	0.020	0.005
Human health & social work activities	0.03	0.01	-0.113	-0.033
Arts, entertainment, & recreation	0.01	0.01	0.027	-0.010
Other service activities	0.01	0.01	-0.012	0.048

Source: Authors' own estimates based on IRP5 data.

Propensity scores were calculated and 7,126 matches were made for firms claiming the ETI in both the 2014 and 2015 tax years. As described in Table 3, there are almost 12,000 firms that claimed the ETI in both years; in other words, we find matches for 60 per cent of the firms claiming the subsidy.

Table 12 displays the results from the conditional DID (cDID) estimation. The coefficients represent the effect of claiming the ETI for each outcome compared with matched firms that did not claim. For firms that claimed in the 14-month period, there is no statistically significant difference in youth employment at the firm in the two years.

Table 12: Results of the cDID estimators from matched firms, by tax year

Outcomes	2014	2015
Youth employees	1.069	2.582
	(2.418)	(2.785)
Non-youth employees	1.996	12.57**
	(5.398)	(6.274)
All employees	2.649	14.34*
	(7.455)	(8.642)

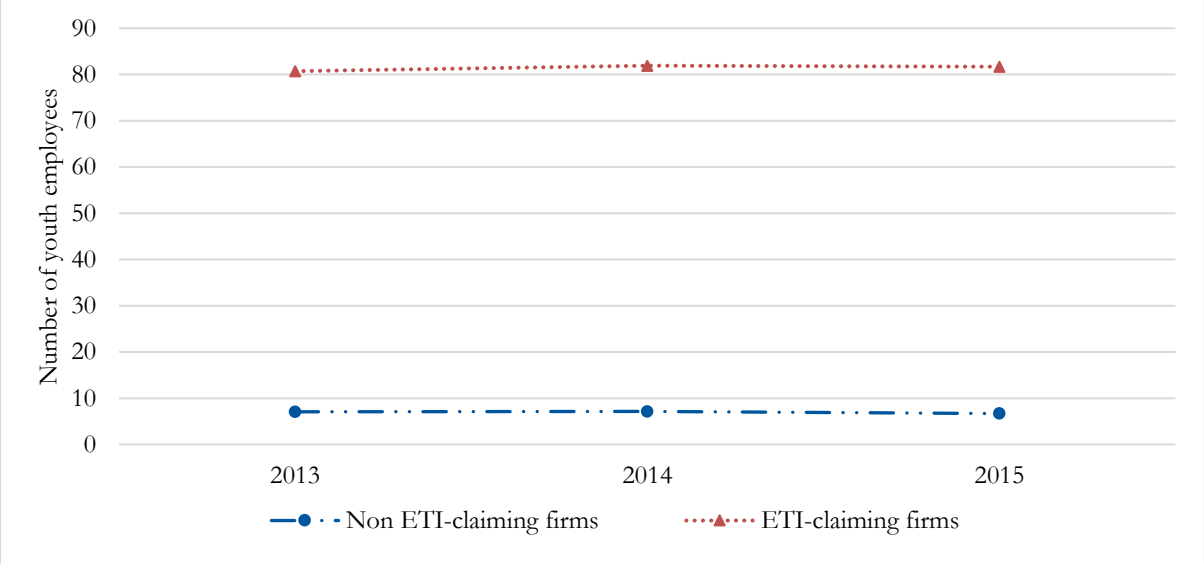
Note: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Source: Authors' own estimates based on IRP5 data.

The coefficient for youth employment is positive in both years. This means that ETI-claiming firms have 1.069 and 2.582 more youth employed in 2014 and 2015, respectively, when compared with non-ETI claiming firms. We appreciate that the effects in 2014 are smaller than in 2015, as a longer claim period will see a greater accumulated effect. However, this result is not statistically significant, as the standard errors on these estimates are very large relative to the coefficient.

The result of the conditional DID estimation is displayed in Figure 2. We see no trend break for youth employment in 2014 or 2015, indicating no significant change in youth employment that can be attributed to the ETI.

Figure 2: cDID estimation for youth employees, ETI-claiming versus non-ETI claiming firms



Source: Authors' own estimates based on IRP5 data.

We estimate the effect of the ETI in heterogeneous sub-groups, as the results could vary. Our sample is large enough to match firms within firm size categories before conducting the DID estimation. Table 13 contains the results.

Table 13: Results of the cDID estimators, matching within firm size categories

Firm size category	Youth employees		Non-youth employees	
	2014	2015	2014	2015
0–10 employees	2.227***	2.857***	2.746***	3.816***
	(0.248)	(0.254)	(0.353)	(0.416)
11–50 employees	1.587***	3.170***	2.035***	4.153***
	(0.15)	(0.169)	(0.26)	(0.303)
51–200 employees	4.621***	7.516***	6.520***	11.45***
	(0.916)	(1.044)	(1.638)	(2.128)
201 + employees	13.07	25.16	25.7	43.88
	(53.96)	(53.32)	(116.4)	(119.6)

Note: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Source: Authors' own estimates based on IRP5 data.

As can be seen from Table 13, there is a positive significant effect on youth employment at firms with fewer than 200 employees. Very large firms, however, do not see a significant increase in the hiring of youth employees. The third and fourth columns in Table 13 show the additional employment expansion of non-youth workers. In all cases the coefficients are larger for non-youth than they are for youth employees. All the coefficients in Table 13 are statistically significant except for firms with more than 200 employees. It is unlikely that the large increase in non-youth employment is due to the hiring of subsidized youth employees. In saying this, we imply that firms with 50–200 employees hiring 7.5 additional youth workers would on average hire 11.4 additional non-youth workers. Such an outcome could reasonably be expected from a firm that is expanding its employment. Thus, we cannot attribute our results to the policy alone and must consider that firms may also be expanding their employment.

Firms claiming in both 2014 and 2015 tend to be larger firms with a greater turnover and a larger number of employees. These firms also have greater hiring and employment rates. The differences on key variables are reported in Table A1 in the appendix. To take into account the differences between firms claiming in both years and firms claiming in one year, we amend the matching process. Firms claiming the subsidy in 2014 are matched using 2013 as the baseline. Firms claiming in 2015 are matched with 2014 as the baseline. It may be the case that a control firm in 2014 is in the treatment group in 2015 if the firm claimed the subsidy in 2015 and not in 2014. This increases the number of firms we are able to match. In 2014 we find matches for 8,172 out of 14,551 firms and in 2015 we find 16,222 matches out of 34,654 firms. The results are shown in Table 14.

Table 14: Results of the amended cDID estimators, matching within firm size categories

Firms size category	Youth employees		Non-youth employees	
	2014	2015	2014	2015
0–10 employees	1.792***	1.233***	2.217***	1.378***
	(0.196)	(0.0557)	(0.28)	(0.0916)
11–50 employees	1.494***	1.517***	1.967***	1.949***
	(0.146)	(0.121)	(0.242)	(0.231)
51–200 employees	4.247***	2.814***	6.585***	4.667***
	(0.791)	(0.664)	(1.365)	(1.077)
201 + employees	23.64	19.12	32.61	13.84
	(46.42)	(59.89)	(101.4)	(117.6)

Note: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Source: Authors' own estimates based on IRP5 data.

In Table 14 we see positive significant effects on youth and non-youth employment at the firms in establishments with fewer than 200 employees. The results are similar to Table 13 but are more modest.

5.4 Cost of the policy

We briefly examine the cost of the policy vis-à-vis the number of jobs created for youth. The cost per job is displayed in Table 15. These estimates are based on the results in Table 13. We make the assumption here that jobs are created as a result of the policy.

We calculated the number of jobs created by multiplying the treatment effect by the number of firms. The estimated costs are calculated by summing up all the ETI claims per year. The cost per job created is the estimated cost divided by the number of jobs created.

Table 15: Estimated costs and number of jobs created in ETI-claiming firms, by tax year

	2014	2015	Total
Estimated number of jobs created	34,822	63,028	97,850
Estimated cost (in rands)	97,668,416	1,229,142,572	1,326,810,988
Cost per job created (in rands)	2,805	19,502	13,560

Source: Authors' own estimates based on IRP5 data.

In Table 15, we calculate a total of 97,850 jobs created in the first 14 months of the policy at a cost of ZAR1.33 billion. This indicates a cost of ZAR13,560 per job created.

The number of jobs created is small when compared with the 898,797 job claims made for the ETI in 2015, which we report in Table 4. This points to a problem of deadweight loss. Deadweight loss is created when a subsidy is claimed for a youth employee a firm already intended to hire in the absence of the subsidy. To calculate this, we make a comparison of the total number of jobs subsidized with the total number of jobs created. The National Treasury, in its public discussion paper, predicted that 423,000 jobs would be subsidized and 178,000 jobs would be created over three years (National Treasury 2011). This means that 245,000 jobs would be subsidized unnecessarily, an expected deadweight loss of approximately 57 per cent. With only 63,028 jobs created from 898,797 job claims, this gives us an estimated deadweight loss of approximately 92 per cent. This is well beyond the estimates of deadweight loss in a similar subsidy study by Betcherman et al. (2010), with an upper limit of 77 per cent. Betcherman et al. (2010) conclude that the inclusion in the policy of stringent eligibility criteria reduced the deadweight loss.

Our conclusion is that the positive and statistically significant effects on youth employment found in small and medium-size firms come with a very high deadweight loss. The high percentage of deadweight loss is presumably driven by the larger firms that claim the subsidy but are not creating any new youth jobs.

6 Conclusion

There is a small body of literature evaluating the effect of wage subsidy policies on labour demand using administrative data. We add to this literature with our examination of the ETI as a policy intervention on labour demand. We use administrative tax data with the population of firms that were eligible for the subsidy to estimate the effects of the ETI on employment at firms in South Africa. Using a conditional difference-in-differences approach we examine the change on the youth and non-youth labour market across firms matched in terms of firm characteristics and over a period that spans the start of the policy.

At the aggregate level of youth employment, we see no significant change in the number of youth employed in the first 14 months of the ETI. The estimated effect is 1.07 newly created jobs in 2014 and 2.58 newly created jobs in 2015. These effects are small in magnitude and are not statistically significant. The period under examination is short and it is possible that the effects of the ETI may only be seen further into the implementation period.

When we break down firms into different size categories, we see that firms employing up to 200 workers have a positive significant effect on both youth and non-youth employment. This increase in total employment can be interpreted as a result of a general expansion in employment in firms. Therefore, at this stage, we cannot tell if the increase in youth employment would have occurred in the absence of the policy. Despite this, using these estimates to calculate the number of jobs created, the deadweight loss appears to be very high due to very few new jobs being created.

Until the data for the 2016 tax year are made available for research, we cannot see whether subsidized employees have remained employed after the subsidy period, have gained employment elsewhere, or are again unemployed. As more data become available, further research needs to be conducted to examine in greater detail the hiring behaviour of firms in general and the impact of the policy on that behaviour. Examination of the employment trends both before and after the implementation of the ETI would give us greater confidence in our results. An in-depth analysis of the substitution effects for those earning just above ZAR6,000 and between the ages of 30 and 35 years needs to be conducted and at the same time a more nuanced discussion of deadweight loss is needed to measure the cost of the policy.

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Appendix A

Table A1: Comparison between firms claiming ETI in 2014, 2015, and in both years

	2014	2015	Both
Firm sales turnover (in millions of rands)	244	180	306
Firm debts (in millions of rands)	637	153	172
Firm assets (in millions of rands)	640	198	223
Firm age (in years)	12.21	13.57	15.04
Mean employee age (in years)	35.83	36.20	35.17
Firm hiring rate (%)	1.07	0.70	2.65
Firm employment growth rate (%)	0.85	0.55	3.09
Firm labour broker status	0.01	0.01	0.01
Firm province (reference Western Cape)			
- Eastern Cape	0.06	0.07	0.06
- Northern Cape	0.02	0.02	0.02
- KwaZulu-Natal	0.05	0.03	0.03
- Gauteng	0.15	0.17	0.14
- North West	0.04	0.03	0.03
- Mpumalanga	0.40	0.38	0.41
- Free State	0.05	0.04	0.04
- Limpopo	0.02	0.03	0.02
Number of employees	79.1	82.3	210.4
Number of youth employees	23.6	25.6	74.8
Number of non-youth employees	53.1	54.8	132.3
Firm industry (reference agriculture, forestry, & fishing)			
Mining & quarrying	0.02	0.01	0.01
Manufacturing	0.25	0.26	0.27
Electricity, gas, steam, & air-conditioning supply	0.00	0.00	0.00
Water supply, sewerage, & waste management	0.01	0.01	0.00
Construction	0.07	0.06	0.05
Wholesale & retail	0.16	0.18	0.19
Transportation & storage	0.02	0.03	0.04
Accommodation & food service activities	0.05	0.06	0.08
Information & communication	0.00	0.00	0.00
Financial & insurance services	0.15	0.11	0.12
Real estate activities	0.01	0.01	0.01
Professional, scientific, & technical	0.10	0.09	0.09
Administrative & support service activities	0.02	0.01	0.01
Education	0.03	0.03	0.03
Human health & social work activities	0.03	0.03	0.02
Arts, entertainment, & recreation	0.01	0.01	0.01
Other service activities	0.04	0.04	0.03

Source: Authors' own estimates based on IRP5 data.