The Employment Tax Incentive Scheme in South Africa: An Impact Assessment

By Haroon Bhorat, Robert Hill, Safia Khan, Kezia Lilenstein and Ben Stanwix

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Abstract

The Employment Tax Incentive (ETI) is a South African wage subsidy programme introduced in 2014 as a form of tax relief for firms hiring workers between the ages of 16 and 29, who earn less than R6 000 per month. Designed as a tool to combat high levels of youth unemployment, the results of studies estimating the effect of the ETI using survey data have shown little to no effect of the subsidy. This paper makes use of the administrative tax data made available through National Treasury in collaboration with the South African Revenue Service (SARS) to accurately identify and estimate the impact of the ETI using individual and firm-level tax returns for the period 2013 to 2016 using a Difference-in-Differences methodology combined with propensity score matching. The impact of the ETI is found to be statistically significant but small in magnitude: During a time when employment levels were decreasing, it is estimated that for every 1 job lost in a non-ETI claiming firm, ETI firms only lost between 0.51 and 0.66 jobs on average. This translates to a total of 35 333 jobs saved between 2014 and 2016 as a result of the ETI. Small firms of fewer than 10 employees have experienced the most benefit from the ETI, with growth of between 0.888 and 0.928 percentage points greater than comparable non-ETI firms, again illustrating a small, but statistically significant, effect of the policy. Furthermore, the effect of the ETI seems to be declining over time, with growth of ETI firms relative to non-ETI firms slowing down in 2015/16 relative to 2014/15, although this is not generalisable to all firm sizes. The ETI does not appear to have negatively impacted employment for workers who are thought to have been most at-risk of displacement due to the subsidy, and has not had any measurable impact on the non-wage benefits of those employed as a result of the subsidy.

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1. Introduction

It is widely understood that South Africa faces an unemployment crisis. The narrow unemployment rate reached 29 percent in the second quarter of 2019, with the broad unemployment rate almost 10 percentage points higher (Stats SA, 2019). These figures are more than double unemployment rates for comparator countries such as Brazil, Turkey, or Russia, and among the highest of any middle-income economy in the world (World Bank, 2018). For young people in South Africa, the prospects of finding employment are even worse than the aggregate statistics suggest – broad unemployment reached 56 percent in 2019 for those between the ages of 15 and 24 (Stats SA, 2019). The reasons for such high and persistent levels of unemployment are multi-faceted and the subject of ongoing debate. As a result, different assessments of the problem suggest different solutions. From a policy perspective, one of the most broadly appealing proposals to tackle unemployment is through the adoption of a wage subsidy, where the overall intention is to lower the cost of employment for firms in order to encourage increased hiring.

The possibility of an employment subsidy in some form has been under discussion as a solution to unemployment in South Africa since the mid-1990s (Standing et al., 1996; Heintz & Bowles, 1996). Policy discussions on the topic became more focused in the late 2000s,[1] and the National Treasury put together a basic outline for a youth employment incentive in 2011 (National Treasury, 2011). Experimental work by Levinsohn et al. (2014), aiming to test the impacts of a wage subsidy for young work seekers, suggested that the intervention would have substantial positive employment impacts on the targeted group. In 2014, the South African government officially introduced a wage subsidy for the first time.[2] The chosen policy intervention specifically targets young work seekers, and was initially set up to run for a two-year period after which it would be reviewed – it has subsequently been extended to February 2029. Called the Employment Tax Incentive (ETI), the intervention aims to stimulate employment of young people in the formal sector by reducing both the perceived risks and the direct financial costs associated with hiring younger workers. Additionally, it is well known that the more time unemployed youth spend out of the workforce the less likely they are to find a job, and the subsidy attempts to interrupt this cycle (Mlatsheni & Leibbrandt, 2015).

More specifically, the ETI acts as a form of tax relief to firms who hire workers between the ages of 18 and 29 years where the monthly wage is below R6 000 (SARS, 2018). Burns et al. (2010) suggest that paying a subsidy to the firm and not the worker is the preferred subsidy design for sustainable labour force growth. The wage subsidy can be claimed by a firm for two-years, after which time the ideal outcome is that the employee has gained the necessary skills and experience to find unsubsidised employment. The subsidy amount paid out to each firm is

[1] See, for example, Levinsohn (2008).
[2] See Muller (*forthcoming*) for a detailed, critical discussion of the political economy origins of the wage subsidy.

calculated on a sliding scale conditional on the individual's wage, and is decreased by half in the second year. In Rand terms the maximum subsidy that can be claimed in the first year is R1 000 per month, and in the second year this falls to R500 per employee – proportionally this equates to a wage subsidy of 17 percent in year one and 8 percent in year two, at the highest wage level.

The intended outcome of the ETI in terms of firm response is that the offer of a subsidised wage for young work seekers encourages firms that would otherwise not have employed anyone to create an additional job, thereby reducing youth unemployment and increasing the experience and skill levels of youth in the labour force. However, the reality is certainly more complex, and it is possible that the ETI could engender a range of distorting effects. Some firms will claim the subsidy for new hires that they would have made regardless of the intervention. Firms may reduce the wage rates of new hires in order to claim the subsidy. The subsidy could incentivise firms to fire non-eligible workers and replace them with subsidised young workers. The subsidy could also incentivise firms to engage in a cycle of replacing existing young workers in order to keep receiving the subsidy. In each of these cases, there are outcomes that include the deadweight loss of public funds as well as negative financial and employment outcomes for eligible and non-eligible workers.

Judging by the increasing levels of youth unemployment in South Africa and the fact that the youth unemployment rate has remained relatively constant since 2010, it is not clear that the ETI has been successful in achieving its overall goal of reducing youth unemployment. However, policy impacts are not so easily determined, and a careful assessment of both the intended and unintended effects of the ETI is required. In this paper, we extend previous evaluations of the ETI to provide a rigorous empirical analysis of the effects of the policy in its first two years, focused on the 2015 and 2016 tax years. To do this we make use of administrative tax data that cover the 2012 to 2016 tax period, which allows us to identify all firms that claim the subsidy and the individual workers they hire. This group of ETI-claiming firms is then matched to a comparable set of firms who do not claim the subsidy, and we run a standard Difference-in-Differences (DiD) analysis. We match these two groups using propensity score matching (PSM), and track employment outcomes over the relevant period. We will provide more detail on this methodology in due course, but the main goal of the paper is to determine the impact of the ETI on three key labour market outcomes:

- Firstly, we investigate whether the ETI has had an effect on the employment rate of eligible youths. This is the primary question of interest, and we run a series of robustness checks on our results to ensure that they are reliable.
- Secondly, we test to see whether workers in the age bracket above those eligible for the subsidy, but who otherwise have similar characteristics to their eligible counterparts, have been displaced because of the ETI.

- Thirdly, we examine the quality of jobs among young workers by looking at changes in two non-wage benefits – pension and medical aid – where we test whether the ETI has led to a reduction of job quality on these measures.

The remainder of this paper is structured as follows. Section two briefly discusses the relevant literature that has analysed the impacts of the ETI thus far. Section three introduces and describes the data that are used in this study, before section four presents our methodology for examining the ETI impacts detailed above. Section five provides a descriptive overview of our data sample. Section six then presents and discusses our econometric results. Section seven concludes.

2. Literature

The ETI was officially introduced on October 1, 2013 and thus remains a relatively new programme from an impact assessment perspective. The most suitable data for measuring the impacts of the ETI are the administrative tax data collected by the South African Revenue Service (SARS), which allows for an analysis of the full sample of firms that take-up the subsidy and the individuals they employ. Unfortunately, this dataset is not publicly available and requires significant pre-planning and collaboration to access. Together these two issues limit the amount of detailed work that can be done on the topic at present. Consequently, very few papers have used the tax data to examine the impacts of the ETI, and several others try to exploit labour force survey data for this purpose. All work thus far necessarily focuses on the short-term effects of the policy, and this is exacerbated by lags in the release of data. We briefly summarise and comment on the main findings of what constitutes the South African literature on the ETI below.[3]

The first paper to examine the immediate effects of the ETI was Ranchhod and Finn (2014)[4]. The authors use data from labour force surveys to test whether the subsidy led to an increase in overall rates of youth employment, and whether there was an increase in job churning in the six months after the implementation of the policy. Using an Ordinary Least Squares (OLS) regression technique and a standard DiD strategy, the authors find that the subsidy had no statistically significant impact on youth employment, and also no discernible effect on the rate of job churning among young workers. Given that the paper focuses on the first six months after the implementation of the ETI, the lack of any statistically significant impact may have been influenced by a slow take-up of the subsidy – but without having access to the tax data it was not possible for the authors to provide information of direct subsidy take-up rates among firms. In an attempt to address this possibility that firms were slow to respond to the policy, and that there may be a lag in observable impacts, the authors extended the time period of analysis in Ranchhod and Finn (2015) where they include four quarters of data (one year) after

[3] See Levinsohn (2008), Ranchhod and Finn (2016) and Ebrahim et al. (2017) for a discussion of the relevant international literature and an assessment of aggregate outcomes from wage subsidy programmes globally.
[4] Subsequently published as Ranchhod and Finn (2016).

the introduction of the ETI. Again, their results show that there is no statistical evidence to suggest that the ETI had any impact on the rate of youth employment.

Together these two papers suggest that any additional jobs created for young people over the course of 2014 would have existed regardless of the subsidy, and thus the ETI represented a financial transfer to participating firms of approximately R2bn in its first year (Ranchhod & Finn, 2015). One concern about the approach of these two papers, aside from the relatively short post-intervention timeframe, is that the data come from labour force surveys and this constrains the precision of an analysis into the impacts of the ETI. Put simply, given the number of jobs the ETI aims to create, it is unlikely that impacts will be identified by testing for changes in the overall rate of youth unemployment. The potential problem here is that the impact of the ETI on overall youth employment probability in any given year is likely to be small relative to the total number of unemployed youth. As the authors note, there are concerns about the, "sample size and statistical power that we have, in terms of our ability to identify any effects of the ETI" (Ranchhod & Finn, 2015:32). Thus, approaching the question of ETI impact from the point of view of overall youth employment, as measured by the household survey data, may not be the most suitable avenue for gauging the policy's impact.[5]

A number of subsequent studies have used the administrative tax data, as well as a small-sample qualitative methods approach, to examine take-up of the ETI among firms and assess its various effects. The papers include: Makgetla (2016); Rankin and Chatterjee (2016); De Jongh et al. (2016); Odendaal (2016); Ebrahim et al. (2017) and Ebrahim and Pirttilä (2019). It is worth noting that at the time of writing not all of these papers have been released in full, and none has been published in a peer-reviewed journal. As a result, one can argue that at present a lack of published evidence precludes clear consensus around the short-term effects of the ETI. Regardless, we focus here on the research that has been done, and particularly on the papers that employ quantitative methods to test the impact of the ETI. We do not review each paper in detail, but briefly discuss the main findings, with a focus on the work of Rankin and Chatterjee (2016) and Ebrahim et al. (2017), which are the two papers that directly inform our work.

Ebrahim et al. (2017) use the administrative tax data to create a sample of matched individuals and firms over a 12-month period following the introduction of the ETI. This sample includes all individuals classified as 'youth', and they apply a DiD approach to assess the employment effects of the subsidy, with a particular focus on how these effects differ across firm size. At the aggregate level the authors' findings support those of Ranchhod and Finn (2015, 2016) – when comparing firms that take-up the subsidy against those that do not, they find no statistically significant impact on youth employment in 2014. Beneath this aggregate result, however, the authors observe a positive and significant employment effect for both youth and non-youth among firms with fewer than 200 employees that take-up the subsidy. This suggests

[5] See Ranchhod and Finn (2016) for a more detailed discussion of this issue.

that the ETI has had positive employment impacts for a subset of firms. The authors do caution, however, that they "cannot attribute [their] results to the policy alone" (Ebrahim et al., 2017:11). According to Ebrahim et al. (2017) it may be that larger firms were already planning on increasing their youth hires, and so the provision of a subsidy does not have any impact on large firm hiring patterns – but would substantially impact smaller businesses. This sentiment is echoed in Ranchhod and Finn (2016), who observe that the ETI seems to be targeted mainly at medium-to-large firms in the formal sector, which could severely limit the potential employment effects that result from the policy.

Ebrahim and Pirttilä (2019) extend the work of Ebrahim et al. (2017) by including an additional year of tax data (2015/16), which gives them two years of post-ETI data for their analysis. In addition, they replicate the work of Ranchhod and Finn (2016) but use three years of post-ETI labour force survey data. The authors find that while the ETI appears to have increased wages of those in the targeted group, there is again no strong evidence from either tax or labour force data that the policy has led to an aggregate improvement in employment for those in the target group. The paper ends by noting that data for subsequent years will be added to their analysis as it is released, given that one possible reason for the lack of overall impact is that many firms are simply unaware of the subsidy: particularly smaller firms where the effects appear to be larger. Odendaal (2016) supports this assumption regarding low take-up among smaller firms based on firm interviews undertaken by the author.

Work by Magketla (2016), and Rankin and Chatterjee (2016) together present a more positive account of the ETI, using diverse methodologies. We focus here on the approach and findings of Rankin and Chatterjee (2016) who conduct a comprehensive empirical analysis of the impact of the ETI in the 2014/15 tax year. They analyse a sample of firms and eligible youth[6] and use several econometric techniques to identify policy effects. The authors report a positive and statistically significant overall impact of the ETI on youth employment. As in previous work, the results also reveal differential impacts by firm size, where the impact of the ETI is strongest among firms with 800 employees or fewer.[7] In percentage terms their preferred specification shows that firms taking up the subsidy expanded their employment at a rate of 10 percentage points higher than equivalent, unsubsidised firms. They also attempt to summarise their main finding in numerical terms. They indicate that in the 2015 tax year a total of 686 402 jobs were supported by the ETI, and their econometric analysis suggests that 55 123 of these were "either created because of the ETI or would have been lost without the ETI" (Rankin & Chatterjee, 2016:1).[8] This means that 8 percent of the supported jobs exist because of the ETI, or alternately, 92 percent of the subsidised jobs would have been created regardless of the subsidy. The resultant 'cost per created job' of the ETI is R44 555.

[6] They define eligible youth as those in the 18-29 year old age bracket, earning below R6 500.

[7] Importantly, the sample size decreases rapidly for larger firms, which affects the precision of the results.

[8] The authors calculate that if job duration is taken into account this equates to 22 831 full year equivalent (FYE) jobs, at a total cost of R107 576 per FYE job.

Rankin and Chatterjee (2016) also extend their analysis to examine the employment effects across industries, and examine the impact of the ETI on displacement of non-eligible workers, and the wages of subsidised youth. They find that the positive employment impacts are robust across industry categories, with percentage growth rates in employment being highest (above 10 percent) in Construction, Transport & Communications, Tourism, Manufacturing, and Financial services. There appear to be no substantial displacement effects for non-eligible workers between the age of 30 and 35. Finally, the authors observe higher wages for subsidised youth, where it appears that both ETI-subsidised jobs and being in a firm that claims the ETI (regardless of whether your job is subsidised) impacts positively on earnings.

There are several preliminary conclusions to be drawn from the work that has been done to date. Descriptively, we know that take-up of the ETI is higher among large firms and lower for smaller firms. In addition, the ETI supports a large number of jobs in total, however, this is not equivalent to creating new jobs. As to the extent of job creation, and where these jobs have been created, the evidence is mixed. Those papers that use administrative tax data (which we would argue is the most appropriate) in general find either no impacts or positive, but small, overall employment impacts at the aggregate level. Beneath these aggregate results, the papers appear to agree that positive employment impacts are observable among smaller firms. There are some methodological disagreements across these papers, as well as differences in sample selection that may account for the variable results, and of course the ETI remains a relatively new policy. As Arndt (2018) notes after reviewing some of the available evidence, there are three possible explanations for the findings that indicate no aggregate impact: "(I) the ETI has not been in place long enough to generate detectable overall impacts; (II) the ETI is particularly attractive to firms that are growing and who would be hiring with or without the ETI; and (III) the wage of lower earning youth workers is only one of many considerations related to hiring of younger workers." (Arndt, 2018: 175). Beyond employment the ETI appears to have had a positive influence on the wages of workers in firms that take-up the ETI, and importantly there is no convincing evidence to suggest that the policy has resulted in any displacement of non-eligible workers.

In addition to these aggregate conclusions, there are a range of heterogeneous results which require more careful scrutiny. According to Rankin and Chatterjee (2016), the greatest proportion of ETI-supported jobs is amongst 18-year-olds (32 percent), while only 7 percent of jobs held by 29-year-olds are supported. This provides strong evidence for ensuring that age is carefully controlled for in an econometric model, as the ETI may have strong effects for some quite narrow cohorts. Similar results have been found internationally, where age-specific employment rates differ significantly after the introduction of a wage subsidy (Huttunen et al., 2013). Ebrahim et al. (2017) find positive employment effects for smaller firms, but this appears to hold for all age categories and not just youth. Rankin and Chatterjee (2016), using a different methodology, also identify positive employment effects that extend outside of their eligible youth sample. This is a concern because it points to the possibility that there are unidentified firm characteristics driving the employment results, which are confounding attempts to isolate the direct impact of the ETI. Our paper aims to clarify these issues by using an additional year of tax data, applying what we contend is the most appropriate combination of sample selection and econometric analysis, and running a series of robustness checks to support our identification strategy. The data we use, and how we have cleaned it, is the focus of the following section.

3. Data

The data for this paper come from firm and individual level tax returns collected by SARS, and made accessible in collaboration with the National Treasury. The data are in the form of an anonymized employer-employee panel, which is constructed specifically for this analysis. Individual level data come from employer-issued Employee Tax Certificates (IRP5 forms) which disclose the total employment remuneration and deductions earned for the year of assessment. The individual-level data are currently available for the 2008 to 2017 tax years. Firm level data are in the form of Corporate Income Tax (CIT) data, which is taken from the Income Tax Return for Companies (form ITR4, replaced with form ITR14 in May of 2013). The firm level data are currently available for the 2008 to 2014 tax years. Each tax year runs from 1 March to 28/29 February. As a result, the 2014 tax year – 1 March 2013 to 28 Feb 2014 – includes only two months in which firms where able to claim the ETI. Because the individual data include a firm identifier, we are able to construct a matched employer-employee panel spanning the 2008 to 2017 tax years. This panel includes all available individual data for 2008 to 2017, but includes firm characteristics only for the years 2008 to 2014.

The nature of the data is such that the employer-employee panel we create is unbalanced: in other words each individual/firm does not appear in each wave of the data. The SARS tax data are administrative, therefore in theory the dataset contains information for every job where the firm is registered for Pay as You Earn (PAYE) tax and the wage of an employee is above R2 000 per month (the threshold for compulsory tax filing). The data excludes public sector workers. One of the major benefits in using administrative data of this type is the large sample size we are able to obtain, as well as the ability to match individuals with the firms in which they are employed. However, a disadvantage is that the dataset does not stem from a labour market survey and provides limited labour market details at the individual level. The tax data include markers for gender and age but there are no data on race, education level, or occupation – all important pieces of information which would have added nuance to this study. The firm-level information is, however, more comprehensive and includes data on: firm age, size, trade status (i.e. exporter or importer), customs data, industry data, firm profit, revenue, and wages. Recently, the location of the firm at both the district and municipal level also became available for use.

3.1. Data Cleaning

We make a number of restrictions to the panel dataset that we use:

1. The employees in the panel are restricted to individuals who have ID numbers, which means that the panel only includes individuals who are either South African citizens or permanent residents.
2. The sample is restricted only to natural persons. This excludes clubs, estates, partnerships, and welfare organizations, etc. from the sample of individuals.
3. The sample is restricted to individuals of working age (15 to 64).
4. Individuals with no income data are removed.

The result of the data cleaning above is summarized in the table below:

Table 1: Data Sample

	2015	2016
Number of observations – raw data	14 004 366	13 740 113
Number of observations – cleaned data	10 655 513	10 409 681

We weight each individual's employment by the number of days worked in each firm in a given year. This is because administrative data captures employment over the entire year, rather than a point in time. The number of days which an individual worked in a firm is calculated using the job start and end dates provided in the data. Weighted employment is determined by calculating the number of days worked divided by the number of days in each year, as follows:

$$Employment\_Weight_{i,f,t} = \frac{Period\ Employed\ To_{i,f,t} - \ Period\ Employed\ From_{i,f,t}}{Days\ in\ Year_t}$$

However, there are a number of cases where the data on work start or end date was unreliable. We therefore dropped all jobs which we deemed to have invalid job spells. This was the case where:

1. The start date for employment was after 28 February of that year. For example, the 2016 tax year runs from 1 March 2015 to 28 February 2016. If the job start date was after 28 February 2016, this was deemed an invalid job period.
2. The job end date was before the start of the tax year. For example, the 2016 tax year was 1 March 2015 to 28 February 2016. If the job end date was before 01 March 2015, this was deemed an invalid job period.
3. The end date of the job was before the start date of the job.
4. The start or end date for the job was missing. In these cases we cannot calculate the weighted employment for these individuals.

In addition, there were some cases where the start or end date of the job fell outside of the tax year in question. Here, the start or end date was updated to reflect the start or end date of the tax year in question. Finally, some individuals have multiple IRP5s for the same firm in one financial year. Where jobs worked by the same individual in the same firm had the same start and end date, these were collapsed into one observation. Where start and end dates were not the same, the days worked were summed.

There are a number of instances where it was necessary to clean the ETI firm claim data. As the IRP5 data is unaudited, assessing the validity of the data can be challenging. We identified four types of fraudulent or incorrect data. The following table describes the fraudulent or incorrect claim and the way we cleaned the data to account for it.

Table 2: Cleaning Fraudulent/Incorrect ETI Data

Type of Fraudulent/Incorrect Data	Data Cleaning Method
Age is less than 18 and ETI claimed	ETI indicator is amended as the individual is not eligible
Age is greater than 29 and ETI claimed	ETI indicator is amended as the individual is not eligible
Employment start date is before October 2013 and ETI is claimed	ETI indicator is amended as the individual is not eligible
Value of Claim is above the maximum Rand value allowed	ETI claim is set to the maximum per year

In order to remove confounding factors in our results over the two years that we analyse, we also remove some firms from the sample for specific years. Firms that claimed the ETI in 2014 but stopped claiming in 2015 were removed from the 2015 sample. Similarly, firms claiming the ETI in either 2014 or 2015 that stopped claiming in 2016, were removed from the sample of 2016 firms.

4. Method

The key outcome of interest in this paper is change in youth employment among firms taking up the employment incentive. In addition to this aggregate effect we are interested in the extent to which these firms may be displacing ineligible workers with those in the ETI target group, as well as whether there have been any changes in the non-wage benefits offered by firms utilising the ETI. The analytical approach we use in this paper draws heavily on the work of Ebrahim et al. (2017), and Rankin and Chatterjee (2016). By making our methodological choices similar to these papers we maintain some comparability between our results. However, although these two papers inform our methodological choices, we have made certain adjustments in order to present what we believe to be the most robust analysis of the effects of the ETI. In terms of the data we use, our work adds value by including data from the 2015/16 tax year. Neither Ebrahim et al. (2017) nor Rankin and Chatterjee (2016) had the

2015/16 tax data available. We also make adjustments to the econometric strategies of previous work, discussed below, that we believe improve the precision of our results.

As in most previous research on this topic, we apply a Difference-in-differences (DiD) strategy to test the impacts of the ETI. DiD is a common econometric strategy used for evaluating the effects of a policy where there are a number of possible confounding influences that could obscure the direct effect of the intervention. The DiD allows for the measurement of the change in an outcome of interest (e.g. youth employment) by tracking changes in this outcome before and after the intervention came into effect. To ensure that the observed changes are a direct result of the policy, changes are also tracked across two closely comparable groups, where effects in the group affected by the intervention (treatment group) are assessed against changes in a similar group that is unaffected by the intervention (comparison group). DiD methods are, however, not perfect and only provide unbiased estimates if the trends in the outcome variable over time would have been the same for the treatment and comparison groups in the absence of the intervention. To help ensure that this is the case, one must identify a suitably similar comparison group. In our case, for example, the DiD estimator is likely to be biased if firms claiming the ETI have very different characteristics to the firms that do not use it.

There are a number of methods available to try to ensure that the treatment and comparison groups are similar, or well identified. One of the most common strategies is to use PSM, which allows one to match observations across groups based on numerous identifying characteristics. More specifically, the PSM technique allows for the creation of a comparison group based on the conditional probability of treatment. This can be written as:

$$p(z) \stackrel{\text{def}}{=} Pr((T = 1)\,|X_i) \tag{1}$$

where $p(z)$ is the propensity score equal to the probability of treatment T for a given set of characteristics X_i. In evaluating the impact of the ETI, a propensity score is defined as the probability of being in the group of firms taking up the ETI (Group 1), versus the group of firms that did not take-up the ETI (Group 2). To estimate the propensity scores we fit a logistic regression predicting whether a firm took up the ETI as a function of a set of observed firm characteristics (X_i).[9] The obtained propensity scores can be used as weights to obtain a balanced sample of treated and untreated individuals (Imbens, 2004). Firms in Group 2 that look very similar to those in Group 1, but very different from the individuals in their own group, will receive higher weights, while firms that look different from those in Group 1 and more similar to individuals in their own group will receive lower weights. These weights are then used to match firms taking up the ETI (Group 1) with firms that did not take-up the ETI (Group

[9] The firm characteristics used in the matching exercise include firm assets, firm size, firm turnover, firm industry, firm age, firm trade status, average employee wage, average employee age, the lag of the employment growth rate, the lag of the youth employment growth rate, and the lag of the employment growth rate of 30-35 year olds (potentially displaced workers).

2), where treated firms who are not matched with a firm in the comparison group are dropped from the sample.

When using PSM to create a comparison group for econometric analysis, it is highly unlikely that the predicted propensity scores of two observational units will be exactly the same, and as a result one needs to allow for a margin of error, or 'caliper', within which a match can occur. However, there is a trade-off introduced at this point: as the caliper increases from 0, although the number of matches can increase, the quality of the matches decreases, introducing bias into any estimates obtained on the matched sample. The norm in the literature is to use a caliper of approximately 0.25 (Lunt, 2013). To increase precision and take advantage of the large dataset that we have available, all matches in our analysis are carried out with a caliper of 0.03 or lower. The resulting sample size remains large and thus we can be confident that matching is unlikely to bias our results. This is further confirmed by the fact that we have a balanced treatment and comparison group.

Once we have a matched sample of treated firms and comparison firms, we run a DiD regression under the common support of the PSM technique[10], to test the impact of the ETI on youth employment, and examine the additional impacts outlined above. In formal terms the DiD equation is defined as follows:

$$Y_{it} = \alpha + \beta(T_t * d_i) + \theta_1 d_i + \theta_2 T_t + \mu_{it} \qquad (2)$$

where Y_{it} represents the outcome of interest (e.g. employment); T_t is a dummy variable that represents the pre- (0) and post- (1) treatment period; d_i is a dummy that represents the treatment group (firms taking up the ETI) and comparison group (firms not taking up the ETI); and u_{it} is a non-stochastic error term. The coefficient β, on the interaction term ($T_t * d_i$), represents the DiD term, and the significance and magnitude of this reports whether the ETI has had an effect on the outcome of interest, and to what extent. All estimates on employment changes as a result of the ETI are year-on-year estimates, which are interpreted as the change in the employment outcome after the passage of one year.

Rankin and Chatterjee (2016) also use PSM and DiD techniques for estimating the effect of the ETI, however, as far as we can ascertain, these methods are utilised separately and not in combination. They also note that their preferred specification is a first-differences model, which assumes that the key drivers of firm behaviour do not change during the period under consideration. We feel that this assumption is unlikely to hold over the longer time period we consider in this paper, and thus opt to follow the method used by Ebrahim et al. (2017) of a DiD estimator on a sample of firms that have first been matched using PSM techniques.

To examine aggregate employment outcomes we use two different variables:

[10] The common support was found to be for propensity scores lower than 0.8.

1. The log of employment.[11] The DiD estimator provides the percentage difference in employment changes between ETI and non-ETI firms as a result of the ETI.
2. Year-on-year difference in log of employment. The DiD estimator provides the difference in the growth rates of employment between ETI and non-ETI firms as a result of the ETI.

Firm size has been identified as an important factor in determining the impact of the ETI, with much larger employment effects observed in smaller firms (Ebrahim et al., 2017; Rankin & Chatterjee, 2016). In order to determine whether the ETI had differential effects on employment by firm size, we also divide our matched sample into size categories and obtain estimates by category. The size category into which each firm fits is chosen based on their initial size – i.e. when considering a change in employment between 2014 and 2015, the size category is based on the 2014 size rather than their size in 2015.

Our employment outcomes are estimated on the following samples:

1. Youth aged 18 to 29 earning less than R6 500 per month[12]. These are the individuals eligible for the ETI subsidy.
2. Individuals aged 30 to 35 earning less than R6 500 per month. This is an appropriate age group to test for worker displacement as they are most likely to be threatened by displacement by the ETI-eligible group because they are similar to the ETI-eligible group in terms of age and work experience, but are ineligible only because of their marginally higher age.

It is also important to note, as we allude to above, that Ebrahim et al. (2017) estimate the effect of the ETI subsidy on youth employment generally rather than on the employment of subsidy-eligible youth. It could be that the strong positive effects on the eligible sub-sample are obscured by employment trends among high-wage youth such as university graduates, and others earning above the ETI wage cut-off. To account for this, our paper focuses more specifically on impacts among youth who are eligible for the subsidy, which agrees with the sample chosen by Rankin and Chatterjee (2016). In addition, Ebrahim et al. (2017) examine total change in employment across their matched sample, which does not account for firm size. As described above, we use the log of employment to avoid this problem.

[11] This gives us a measure of changes in employment while controlling for firm size. Furthermore, in the case of estimating changes in youth employment, for example, the log of employment provides a measure that accounts for baseline levels of youth employment in the firms of interest.

[12] We choose to look at those earning less than R6 500, rather than the R6 000 cut off for the wage subsidy, due to the potential of measurement error in the way monthly wages are calculated in the data. Monthly wages are not given and must be calculated based on the wage an individual earns for a specified period of time worked. There is no data on the number of days an individual worked in that period. Therefore, monthly wages may be underestimated if individuals were not working every day within the specified period worked. This choice of cut off is also consistent with the choice made by Rankin and Chatterjee (2016).

5. Descriptive Overview

Table 3 provides an overview of our sample over the period as well as the number of individuals and firms taking up the ETI each year. Take-up of the ETI is lowest in the first year (2013/14), which can be expected given that the subsidy was only available for the last two months of this tax year. Nevertheless, the data show that in 2013/14, a total of 25 517 firms claimed the ETI, which amounts to 10.8 percent of all firms in the sample. ETI take-up increases to 35 105 firms in 2014/15 (14.6 percent of firms) before dropping to 31 141 firms in 2015/16 (13.7 percent of firms).[13]

The number of jobs that the ETI supports increased from 1.4 percent of all jobs in 2013/14, to 10.6 percent in 2015/16 – which equates to 1.1 million jobs. Note, however, that the number of jobs is not equivalent to the number of individuals employed, as individuals may work multiple jobs per year.[14] The total Rand amount that firms claimed under the ETI increased exponentially between 2014 and 2016 – from R47.55 million in 2013/14, to just over R4 billion in 2015/16. This translates to R323 claimed per job supported in 2013/14, R2 575 per job supported in 2014/15, and R3 639 per job supported in 2015/16. However, while these are new jobs, it is not clear that they were created as a result of the subsidy.

Table 3: Wage Subsidy Uptake by Year

	2013/14	2014/15	2015/16	% Change: 2014-2016
Number of Firms in Sample	236 211	241 255	226 598	-4.1
Number of Firms Claiming Subsidy	25 517	35 105	31 141	22.0
Number of Subsidised Jobs	147 200	878 020	1 100 659	647.7
Subsidised Jobs as % of Total Jobs	1.4	8.2	10.6	657.1
Proportion of Claimant Firms	10.8	14.6	13.7	26.9
Total Claims (R, millions)^	47.55	2 261	4 005	8 322.7

Source: ^Chatterjee and MacLeod (2016), own calculations using SARS TAX Data 2014-2016.

Table 4, below, breaks down the aggregate firm data by industry, showing the distribution of all firms in the dataset, and the distribution of those firms taking up the ETI. It should be noted that the industry in which a firm resides is based on information contained from the individual IRP5 tax form for each firm, rather than being classified according to the standard ISIC coding system. Therefore, industry totals are likely to differ as compared to those from household survey data. In addition, totals will differ from the table above due to some missing firm

[13] The 2015/16 tax year may reveal increased numbers of ETI claims after it is updated with data from the late filing of tax returns and resubmissions.

[14] A “job” is calculated as the number of unique individual-firm combinations per year. In other words, an individual working twice for the same firm in the same year is calculated to have one job in that year. On the other hand, an individual working for two different firms in the same year is calculated to have worked two jobs in that year.

industry information in the data. Agriculture, Wholesale and Retail, Manufacturing, and Mining firms are overrepresented in the sample of firms taking up the ETI relative to their numbers in the overall sample. In terms of the distribution by industry, firms taking up the ETI are most likely to be in Finance (28.0 percent), Manufacturing (23.8 percent), or Wholesale and Retail (17.7 percent).

Table 4: Wage Subsidy Uptake by Industry: 2016

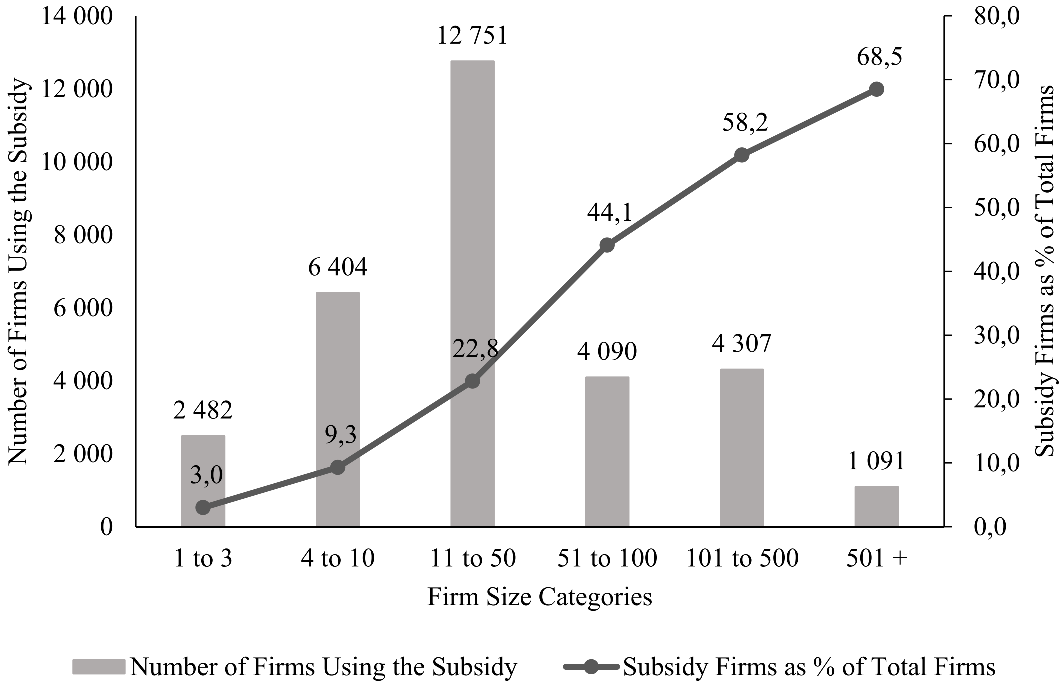
Industry	All Firms	Subsidy Firms	Ratio: Subsidy Firms to All Firms
Agriculture	7.6	10.1	1.3
Mining	1.0	1.1	1.1
Manufacturing	21.3	23.8	1.1
Utilities	1.3	1.1	0.8
Construction	6.4	5.8	0.9
Wholesale & Retail	14.4	17.5	1.2
Transport	3.1	2.9	0.9
Finance	32.0	28.9	0.9
CSP	9.4	6.1	0.6
Other	3.6	2.7	0.8
% Total	100.0	100.0	-
Total Number of Firms	212 931	30 724	-

Source: Own calculations using SARS Tax Data, 2016.

It is also useful to disaggregate the uptake of the ETI by firm size as this reveals substantial differences in uptake between smaller and larger firms.

Figure 1, below, plots the number of firms taking up the ETI within each firm size category (the bar graph), as well as the percentage that these ETI-claiming firms account for as a proportion of all firms in that size category (the line graph). For example, for firms with 1 to 3 employees, the data indicate that there are 2 482 firms claiming the ETI, which equates to 3 percent of all of the firms in this size category. Looking at the proportion of firms claiming the ETI and the proportion of total firms in each size category, it is clear that there is a strong monotonic relationship between claiming the ETI and firm size. Put simply, claiming the subsidy is far more common among larger firms, but since there are fewer large firms in numerical terms, we observe a higher number of small and medium size firms taking up the subsidy. In 2015/16, less than 10 percent of firms with 10 or fewer employees claimed the ETI, compared with 68.5 percent of firms with more than 500 employees. This may be because of lower average costs associated with applying for the incentive for larger firms, or greater visibility of the incentive for larger firms (Chatterjee & MacLeod, 2016). The figure provides a relatively unbiased estimate of the proportion of all tax-paying firms taking up the ETI. However, because small firms are less likely to be paying tax than large firms, the numbers here may overestimate the proportion of firms – especially small firms – claiming the ETI.

Figure 1: Uptake of the Wage Subsidy Scheme, by Firm Size: 2016



Source: Own calculations using SARS TAX Data 2016.

What the above figure shows, is that when taking into account the number of small and large firms in the economy, it is clear that overall there are more small and medium-sized firms claiming the subsidy than large firms in numerical terms. However, the take-up rates are relatively low amongst smaller firms. Despite the fact that only 22.8 percent of firms sized 11 to 50 claimed the subsidy in 2016, this equates to almost 13 000 firms – making it the largest group of ETI-claiming firms by size category. Conversely, while 68.5 percent of the largest firms claimed the subsidy in 2016, this category comprises the smallest number of firms claiming the subsidy, at just over 1 000 firms in total. The variation by firm size is thus an important consideration for analysis that we take into account below.

6. Econometric Results

The following section provides the conditional DiD results for the following outcomes: Job creation (and conversely, job displacement); Job creation by firm size; and Proportion of youth with non-wage benefits. First, we examine job creation without disaggregating by firm size. Here, two measures of job creation are used – the logged number of youth employed, and the year-on-year change in this variable. Table 5, below, reports our key results for the effects of the ETI on employment at the aggregate level. The results from the DiD estimation are presented for three outcome groups, for 2015 and 2016. The three groups are: (1) youth eligible for the subsidy, (2) employees at risk of displacement, and (3) all employees[15]. As discussed above, we include (2) in order to evaluate the potentially negative effects of the subsidy on workers aged 30 to 35 who are earning less than R6 500 per month – these are the sample of workers most likely to be displaced by the incentive.

Our most important result here is the DiD coefficient on the log of employment for subsidy eligible youth, where we are comparing the difference in employment outcomes between ETI and non-ETI firms in both 2015 and 2016. The coefficients indicate significant and positive effects of the subsidy – we observe higher rates of employment among firms that take up the ETI relative to matched firms that do not.[16] This effect holds for both years, with the employment effects in 2015 being slightly larger than in 2016. To be clear, these results represent the differential percentage change in employment between ETI and non-ETI firms.

For subsidy-eligible youth, then, the coefficient of 1.078 in 2015 indicates that youth employment in ETI firms was $(e^{1.078} - 1) \times 100 = 193.88$ percent higher than it would have been in the absence of the ETI (or equivalently, 293.88 percent of its non-ETI value). Similarly, in 2016, employment was approximately 97 percent higher (197 percent of its non-ETI value) in ETI-claiming firms than it would have been without the ETI. Although these results sound promising, it is critical to contextualise them in terms of actual job numbers: for non-ETI firms, average youth employment decreased by approximately 1 individual between 2014 and 2015, and by about the same amount between 2015 and 2016. This means that between 2014 and 2015, ETI-claiming firms only decreased youth employment by 0.34 on average, as opposed to 1 youth, leading to a saving of 0.66 jobs. Similarly, in 2016, the ETI led to a saving of 0.49 jobs in ETI firms compared to their non-ETI counterparts.

With 30 724 firms taking up the ETI in this period, the above result suggests that approximately 20 278 jobs were saved between 2014 and 2015 as a result of the ETI. This number drops to 15 055 jobs between 2015 and 2016. This corresponds to 2.31 percent of subsidised jobs in 2014/15 and 1.37 percent of subsidised jobs in 2015/16. Although the effects are statistically significant, they are practically small, indicating that the effects of the ETI, while positive, are marginal.

The results also suggest that for employees at risk of displacement, and indeed if we include all employees, there is a statistically significant difference in employment growth between ETI-claiming firms and non-ETI-claiming firms. Again, the results hold for both years, although the coefficients are smaller than in the case of youth eligible for the subsidy. This is an unexpected result, and there may be a number of reasons why firms taking up the ETI would hire more

[15] We have added in this third category to test the impact of the ETI on total employment. This provides an overall picture of employment shifts in ETI-claiming firms, and a point of comparison for our results on the employment of subsidy eligible youth.
[16] At this juncture, it should be noted that employment in both ETI and non-ETI firms was decreasing after 2014, however, this decrease occurred at a slower rate amongst ETI firms. In essence, then, the ETI can be said to have mitigated job losses in ETI-claiming firms. A graphical depiction of these employment trajectories can be found in Figure A1, in the appendix.

non-subsidy-eligible employees than non-ETI firms, but it is not a question we explore further here.[17]

In summary then, our aggregate results for the log of employment suggest that employment growth was higher in ETI-claiming firms for all three employment categories – eligible youth, those at risk of displacement, and all employees. Notably, the coefficients are largest for subsidy eligible youth by a factor of three or more. In addition, for all employment categories we observe that the DiD estimator decreases in the second year, indicating that the effect of the subsidy in terms of employment growth was larger in 2015 than it was in 2016. However, when contextualising these findings, the magnitude of the effect of the ETI is found to be relatively small, with less than one job being saved per firm due to the ETI. These findings are consistent with several previous studies using the administrative tax data, but provide more clarity on the impact of the subsidy at the aggregate level.[18]

Table 5: The Impact of the ETI on Employment: DiD Estimates

	Log of employment		Rate of Change in Log of employment	
	2015	**2016**	**2015**	**2016**
Subsidy Eligible Youth	1.078***	0.679***	0.382***	-0.145**
	(0.072)	(0.078)	(0.055)	(0.059)
Employees aged 30-35 Earning <6 500	0.312***	0.269***	0.071	0.011
	(0.074)	(0.081)	(0.047)	(0.051)
All Employees	0.109***	0.0648**	-0.008	-0.0217**
	(0.027)	(0.030)	(0.009)	(0.009)

Source: Own calculations using SARS TAX Data 2012-2016.

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

The last two columns of the table above report the effect of the ETI on the growth rate of ETI-claiming firms relative to non-ETI claiming firms. The reported results are consistent with those in the first two columns: namely, the DiD estimator from 2015 indicates that the difference in the rate of change in the growth of youth employment between ETI and non-ETI firms was positive and significant. In other words, youth employment grew approximately 0.382 percentage points faster for ETI firms relative to non-ETI firms in 2015. However, in 2016 this difference is negative. This does not mean that the youth employment grew by more in non-

[17] A concern arising from this finding is that it suggests that there could be some non-observable characteristic among firms taking up the ETI that are driving overall employment growth. In order to deal with this we run a series of robustness checks, which are discussed toward the end of this section.

[18] Papers that have used survey data to estimate the effect of the ETI, such as Ranchhod and Finn (2015), have found an insignificant impact of the ETI on employment outcomes, which stands in direct contrast to the results obtained when using administrative data. These discrepancies likely arise due to an identification constraint when using survey data: namely, that ETI firms cannot be distinguished from non-ETI firms, and as a result, the effects of the ETI are diluted by being estimated across all firms, irrespective of claimant status. Due to the impacts of the ETI estimated using administrative tax data (where the identification problem can be overcome) being small, it is unsurprising that the effect of the ETI becomes insignificant when estimated using survey data.

ETI firms relative to ETI firms in 2016. Rather, it indicates that the rate of increase in youth employment was lower for ETI than for non-ETI firms in 2016. This is likely due to the rapid growth in 2015, which was not matched the following year. The change in the rate of growth is not consistently significant across the other employment categories.

Given the observed difference in uptake of the ETI by firm size, Table 6, below, estimates the effect of the subsidy on youth employment across firm size. This is done by matching firms within six size categories before running the DiD regression, and is made possible by the large sample. It is immediately clear from the results that the effect of the subsidy differs substantially for firms in different size categories. While the growth in youth employment is higher for ETI-claiming firms (relative to non-claiming firms) across all size categories, as shown above, the size of the coefficient is strictly decreasing as firm size increases. Put differently, the ETI appears to have a significant effect on the percentage change in employment that is larger among smaller firms. Contained within this result is the fact that while the coefficients are smaller for larger firms, these percentage changes are likely to equate to a greater absolute number of jobs, because the base number of employees is bigger to begin with.

Table 6: The Impact of the ETI on Employment Matching within Firm Size Categories: DiD Estimates

	Log of employment		Rate of Change in Log of Employment	
Subsidy Eligible Youth	2015	2016	2015	2016
1 to 3 Employees	2.789***	2.007***	0.888***	-0.238
	(0.302)	(0.331)	(0.309)	(0.344)
4 to 10 Employees	2.021***	1.264***	0.928***	-0.375**
	(0.175)	(0.183)	(0.168)	(0.175)
11 to 50 Employees	1.020***	0.773***	0.430***	0.00227
	(0.0903)	(0.102)	(0.0767)	(0.0894)
51 to 100 Employees	0.751***	0.489***	0.403***	0.141
	(0.124)	(0.144)	(0.103)	(0.115)
101 to 500 Employees	0.617***	0.664***	0.488***	0.319***
	(0.123)	(0.136)	(0.0961)	(0.0946)
500+ Employees	0.578**	0.483*	0.121	-0.101
	(0.289)	(0.283)	(0.246)	(0.178)

Source: Own calculations using SARS TAX Data 2014-2016.
Notes: Standard errors in parentheses. ***p<0.01, **p<0.05, *p<0.1

The size of the coefficients from the regressions using logged employment as a dependent variable are once again very large, however, as shown in Figure A2 in the Appendix, employment increases for non-ETI firms are relatively small in magnitude. These small increases in employment among non-ETI firms translate into modest employment effects due to the ETI. For example, in the case of firms with between 1 and 3 employees, the coefficient for 2015 indicates that ETI firms showed 1 526 percent greater employment than non-ETI firms. However, since the increase in non-ETI employment is only approximately 0.05 youth on average, this implies that ETI firms increased employment by 0.75 youth on average. Although modest in absolute terms, an increase of 0.75 youth for firms of 1-3 employees still constitutes a substantial overall increase in workforce. Similar calculations can be undertaken for all other size categories.

Similarly, when considering the rate of change of employment, as presented in the second panel of Table 6, it is clear that the significant positive effects of the ETI were more prevalent in 2015 than in 2016. Furthermore, the effect of the ETI in 2015 was about twice as strong for small firms relative to large firms, with the largest differential in growth rates being experienced by firms with between 4 and 10 employees (0.928 percentage points) and firms with between 1 and 3 employees (0.888 percentage points).

As in the aggregate sample, negative point estimates of the effect of the ETI in 2016 do not speak to negative effects, but rather simply to a slow-down of growth between 2015 and 2016. Large firms of between 101 and 500 employees, however, seemed to experience increased growth in youth employment as a result of the ETI in 2016, with employment growth accelerating by 0.319 percentage points. This result may be correlated with larger firms being more aware of the ETI, and as such being able to claim the subsidy more consistently for their workers, leading to further long-term youth employment growth.

Overall, the results provide strong evidence to indicate that there are positive youth employment effects of the subsidy across all firm sizes, but also that the largest increases in employment, in percentage change terms, are among smaller firms.

We also run the above estimation on the sample of youth who are at the highest risk of displacement (30-35 year olds, earning a low wage) – the full table is available in the Appendix (Table A1). Again, as the aggregate results revealed, the ETI appears to have positive effects on the employment of non-eligible younger workers. However, the disaggregated results indicate that while ETI-claiming firms hire more 30-35 year old workers than non-ETI firms, this effect decreases as firm size increases, and the effect is statistically insignificant for the largest firms in our sample. Importantly, the size of the coefficients is substantially smaller than for subsidy eligible youth – again this is similar to our aggregate employment results.

Finally, Table 7 presents the results from our analysis of two non-wage benefits for subsidy-eligible youth employed in firms claiming the ETI, relative to firms that do not claim the subsidy. The results show that there is no significant difference in the provision of pension or medical aid benefits to youth in ETI and non-ETI firms, and this result holds for both years. These results indicate then that employment growth in subsidy-claiming firms is higher than for firms not claiming the subsidy, and that these youth hired as a result of the ETI are not worse-off than youth in non-ETI firms in terms of the non-wage benefits examined here.

Table 7: The Impact of the Wage Subsidy Scheme on Non-Wage Benefits: DiD Estimates

	Subsidy Eligible Youth	
	2015	2016
Pension	-0.004	0.006
	(0.007)	(0.008)
Medical Aid	0.000	0.003
	(0.002)	(0.002)

Source: Own calculations using SARS TAX Data 2014-2016.
Notes: Standard errors in parentheses. ***p<0.01, **p<0.05, *p<0.1

The fact that our aggregate results show employment growth in ETI-claiming firms among employees not eligible for the subsidy, raises questions about whether we have isolated the true effect of the ETI. It suggests that the employment effects we identify could be a result of some additional unobserved characteristic of claiming firms, and not the direct impact of the wage subsidy. In order to test the robustness of our results, we run a series of additional regressions to control for this potential problem. First, it may be that prior employment growth among firms in our sample is driving the choice to claim the ETI – where firms that are growing claim the subsidy for youth they would have hired in any case, or that firms who are shedding jobs claim the subsidy as a way to retain employees. We have tried to account for this in our original matching exercise by including the overall employment growth rate, the youth employment growth rate, and the employment growth rate for 30-35 year olds – for the previous period – as a matching variable. However, to more firmly establish that the employment effects we identify are not merely a result of an underlying firm growth path, we re-run our main estimation on two different samples – first, only for those firms that experienced employment growth in the previous year, and second, only for those firms that experienced a decline in employment in the previous year. In this way, we are able to remove prior firm growth as a determining factor from our results, and compare growing firms that take up the ETI against growing firms that do not – and the same for firms that reduced employment in the previous year.

Table 8 presents the results for the first of these estimations. The table shows the DiD coefficients where our sample matching was restricted to firms experiencing positive employment growth in the previous year. The results mirror those from Table 5 above – the DiD estimator is positive and significant in all three employment categories, and the size of the coefficients is similar. This suggests that, even within the sample of firms experiencing employment growth, taking up the ETI appears to increase employment growth overall, and this effect is strongest for subsidy-eligible youth.

Table 8: Impact of the ETI on Employment Matching Firms with Positive Growth Rates: DiD Estimates

	Log of employment	
Subsidy Eligible Youth	2015	2016
Subsidy Eligible Youth	1.073***	0.665***
	(0.089)	(0.097)
Employees aged 30-35 Earning <6 500	0.354***	0.320***
	(0.093)	(0.101)
All Employees	0.118***	0.071*
	(0.034)	(0.038)

Source: Own calculations using SARS TAX Data 2014-2016.
Notes: Standard errors in parentheses. ***p<0.01, **p<0.05, *p<0.1

In Table 9 we run the same estimation as above, but looking only at firms that experienced a decline in employment growth in the previous year. Again the significance across all of the outcome variables is the same, and the size of the coefficients is similar. This suggests that among the sample of firms that were reducing employment in the previous period, those taking up the subsidy either increased employment, or reduced employment by less, relative to firms that did not take up the subsidy. This outcome holds for both 2015 and 2016. Together, the results in these two tables indicate that our main findings are robust to the possibility that prior firm growth is driving the employment changes that we observe.

Table 9: Impact of the ETI on Employment Matching Firms with Negative Growth Rates: DiD Estimates

	Log of employment	
Subsidy Eligible Youth	2015	2016
Subsidy Eligible Youth	1.281***	0.830***
	(0.121)	(0.131)
Employees aged 30-35 Earning <6 500	0.381***	0.358***
	(0.123)	(0.134)
All Employees	0.092**	0.071
	(0.044)	(0.049)

Source: Own calculations using SARS TAX Data 2014-2016.
Notes: Standard errors in parentheses. ***p<0.01, **p<0.05, *p<0.1

Finally, we want to check whether there may be some omitted firm characteristic among ETI-claiming firms that is behind our employment results, but is unrelated to the impact of the subsidy. To account for this possibility, we take our matched sample of ETI and non-ETI firms and re-run our main employment change regression for the 2013 tax year, which was prior to the launch of the ETI.[19] We would not expect to find that firms claiming the ETI in 2015 and 2016 would have significantly different employment outcomes from non-claiming firms in 2013. If we do find positive and significant differences in employment between our treatment and comparison groups, this would indicate that it is not the ETI driving the observed increases in employment, but rather some pre-existent feature unrelated to the ETI. To reiterate, this is the same matched sample of firms in our estimates for 2015 and 2016: we simply trace them back to 2013 and test to see if there are differences in employment growth between the firms that end up claiming the ETI and those that do not.

The results from this estimation are shown in Table 10. The DiD coefficients are not significant for any of the employment categories – subsidy eligible youth, 30-35 year olds, or total employment – in either of the two specifications. This provides relatively good support for the argument that our main results above are accurately identifying the effects of the ETI on employment growth, instead of some underlying, unobservable effect confounding our estimation strategy.

[19] Note that the 2013 tax year runs from March 2012 to February 2013, therefore the ETI did not exist during this period – it launched on 1 October 2013.

Table 10: Robustness Check of the Impact of the ETI on Employment: DiD Estimates

	Log of employment 2013	Rate of Change in Log of employment 2013
Subsidy Eligible Youth	0.065	0.071
	(0.078)	(0.068)
Employees aged 30-35 Earning <6 500	0.124	0.039
	(0.080)	(0.058)
All Employees	0.005	-0.026
	(0.030)	(0.021)

Source: Own calculations using SARS TAX Data 2012-2016.

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

7. Conclusion

The number of jobs supported by the ETI has grown rapidly since 2014, reaching over 1 million in 2016. Our econometric results show that firms claiming the ETI hired more subsidy-eligible youth than firms not claiming the subsidy. Put differently, the percentage change in employment of subsidy-eligible youth is higher in ETI-claiming firms as compared to similar firms that did not claim the ETI. In practical terms, these results suggest that a total of 35 333 jobs were saved between 2014 and 2016 due to the implementation of the ETI, although the effects of the ETI seem to diminish slightly in the second year of the policy. The effect of the ETI is statistically significant in 2015 and 2016, and although point estimates of the effect appear to be large, the practical effects are substantially more modest, due, in part, to relatively stable youth employment levels between 2014 and 2016 amongst firms in the analysis sample. Although these effects are practically relatively small, they are still larger than the effects estimated by survey data and reported in other papers (see, for example, Ranchhod and Finn (2015)). Beyond the effects on subsidy-eligible youth, we find that the ETI also has much smaller, but still positive and statistically significant, employment impacts for low wage employees aged 30-35. These are employees who face the highest risks of displacement due to the subsidy. This is a surprising finding, and at this stage it is unclear what the reasons behind the result may be. We conduct a series of robustness checks to try and eliminate potentially unobserved factors driving our findings, and we are thus relatively confident that the reported results are indeed accurately picking up the impact of the ETI.

We also find that the observed employment changes differ substantially by firm size. The percentage change in employment is largest for smaller firms, but still significant among large firms. Given that we are measuring percentage change in employment, this makes sense, as an additional employee in a small firm accounts for a much larger percentage increase in employment than an additional employee in a very large firm. It should also be kept in mind that uptake of the ETI amongst small firms is low relative to uptake among large firms. Given that the ETI appears to have the strongest employment growth effects for smaller firms, this suggests that there is scope for a more systematic programme aimed at disseminating information about the ETI, with the specific goal of targeting small firms. We also test the impact of the ETI on two non-wage employment benefits – pension and medical aid – and find no difference on these measures between ETI and non-ETI firms. The subsidy does not, therefore, appear to have an impact on 'job quality' as measured by these proxy variables.

Recently, the ETI was extended for a 10-year period, and it now runs until 2029. Given the short lifespan of the policy thus far, there is little clarity on what the medium-term effects will be, and certainly there is scope for a variety of more detailed analyses as additional years of data become available. It will soon be possible to analyse the trajectory of youth in the labour market after they have left their jobs at the firms claiming the ETI, or after the subsidy lapses. Given that one of the goals of the policy is to expose youth to the workplace in order to enhance their future employability, this will be a key marker of the scheme's success. More work is also required to understand why the ETI appears to boost employment for those outside of the targeted age category.

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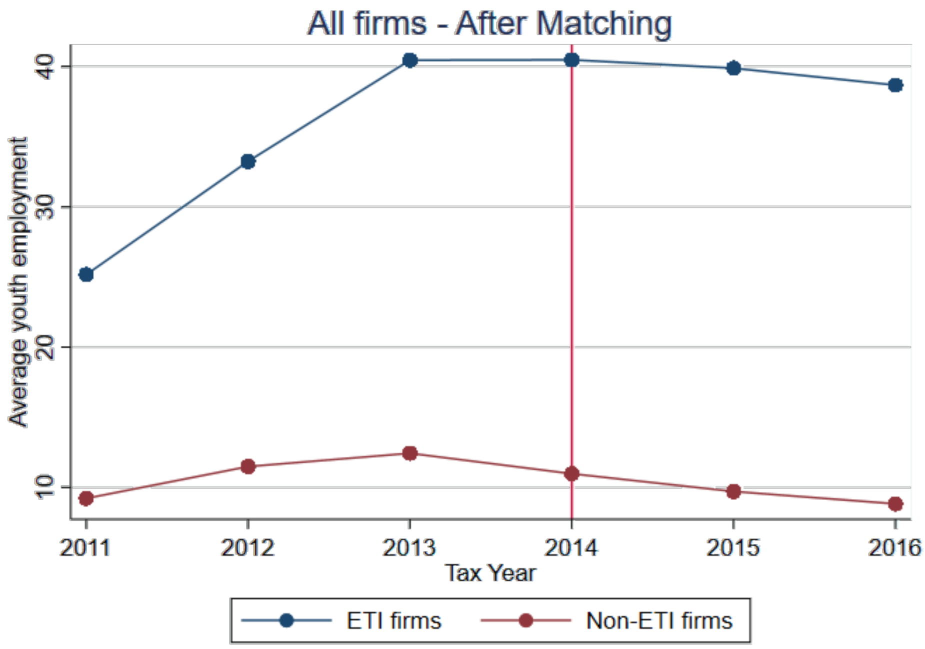
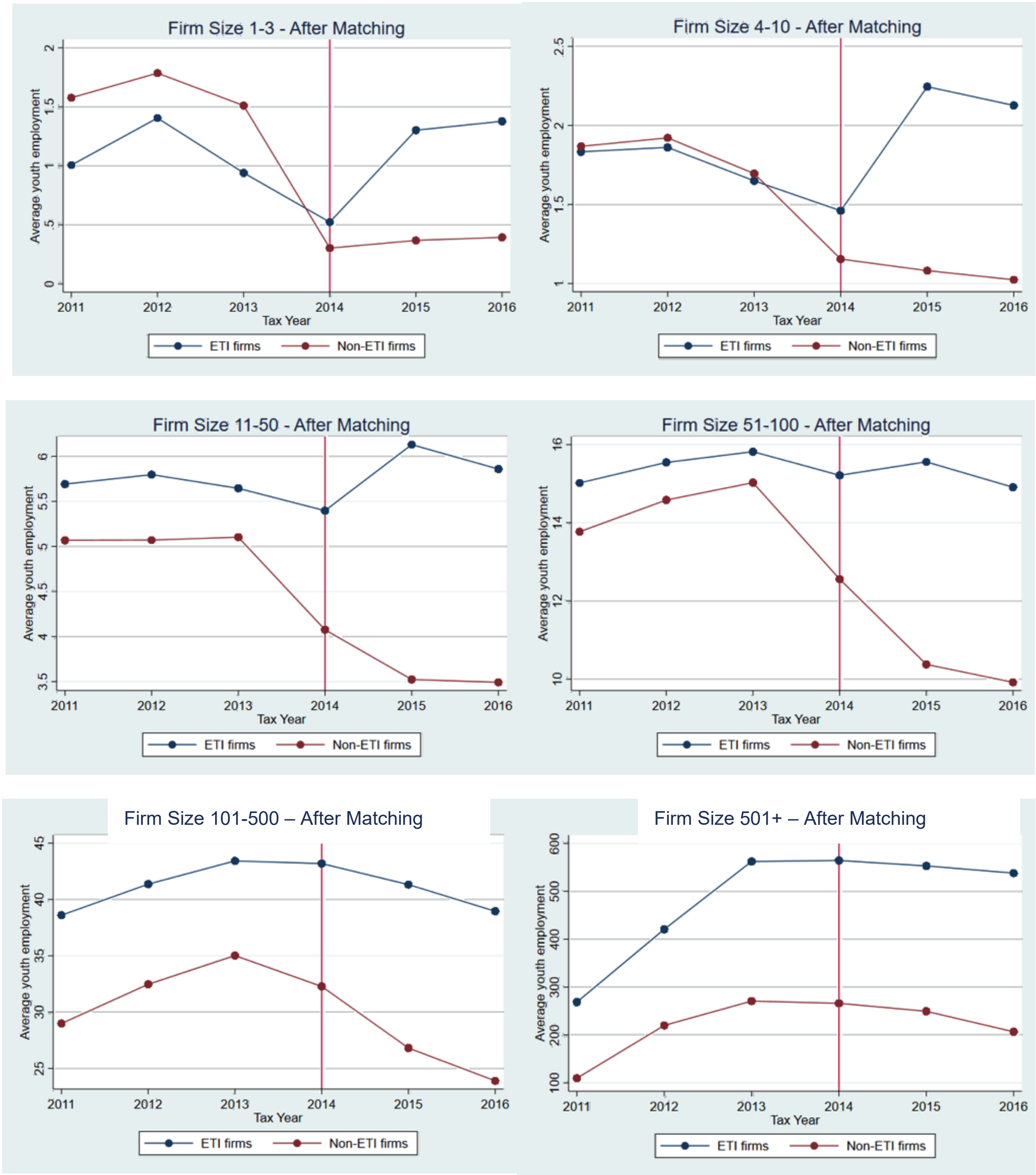
Appendix

Figure A1: Employment trajectories for matched ETI and non-ETI firms, 2012-2016



Source: Own calculations using SARS TAX Data 2012-2016.

Figure A2: Employment trajectories for matched ETI and non-ETI firms by firm size, 2012-2016



Source: Own calculations using SARS TAX Data 2012-2016.

Table A1: The Impact of the ETI on Job Displacement Matching within Firm Size Categories: DiD Estimates

	Log of employment	
Age 30-35 Earning <= 6 500/month	2015	2016
1 to 3 Employees	0.999***	0.754***
	(0.257)	(0.286)
4 to 10 Employees	0.563***	0.398**
	(0.170)	(0.179)
11 to 50 Employees	0.326***	0.338***
	(0.0971)	(0.108)
51 to 100 Employees	0.342**	0.446**
	(0.157)	(0.176)
101 to 500 Employees	0.283*	0.449**
	(0.166)	(0.176)
500+ Employees	0.110	0.538
	(0.460)	(0.422)

Source: Own calculations using SARS TAX Data 2014-2016.
Notes: Standard errors in parentheses. ***p<0.01, **p<0.05, *p<0.1

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