Persistence in employment of youth post-wage subsidy: Matched firm-employee level evidence from South Africa

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Abstract

Extreme levels of youth unemployment in South Africa prompted the implementation of the Employment Tax Incentive (ETI) in 2014 that subsidises wages of workers between the ages of 18 and 29 years for up to two years. While previous studies have mainly examined the immediate impact of the ETI on employment and wages during the subsidy period, this research analyses the policy's persistent effects on employability post-subsidy. Leveraging anonymized matched employer-employee administrative tax data spanning 2014 to 2020, the study tracks four cohorts of first-time workers and sheds light on the influence of individual and firm characteristics and gender on employment persistence probabilities of ETI participants. The findings reveal improved employability and job prospects for ETI beneficiaries relative to non-beneficiaries that partly works through incentivising retention by firms of subsidised workers for the two-year duration of the subsidy, and enhanced job mobility.

Keywords: Wage Subsidies, Employment Sustainability, Firms, Labour Market

JEL Codes: J08, J21, J23, D22

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1. Introduction

Unemployment among the youth population is a pressing issue in emerging economies, with far-reaching implications for economic growth, social cohesion, and individual well-being (Heintz & Naidoo, 2021; Francis & Webster, 2019; Banerjee et al., 2008). South Africa, in particular, is grappling with youth unemployment rates as high as 43.6% according to Statistics South Africa (2021). The persistence of these high unemployment rates points to structural constraints to employment creation in South Africa (Banerjee et al., 2008). To solve this challenge, active labour market interventions are required to shift the unemployed into employment. One such intervention is a wage subsidy, which is often used in both developed and developing economy contexts to reduce unemployment rates (Smith, 2006; Card et al. (2018).

While the literature on the impact of wage subsidies on labour market outcomes is widespread, most of the attention internationally has been placed on employment and wage impacts during the subsidy period (e.g., Betcherman et al., 2004; Jaenichen & Stephan, 2011). Although informative, such research only offers a partial assessment of the desired wage subsidy effect. What the current literature often ignores is the effect of wage subsidies on the persistence in employment of participants post the wage subsidy period. More research on this topic is required, as the potential long-run effects of wage subsidies are ambiguous.

By providing financial incentives to employers, wage subsidies reduce the cost of hiring workers, thereby encouraging employers to offer employment opportunities to unemployed individuals (Brown, 2015). Moreover, wage subsidies can help individuals gain work experience and develop skills through 'learning-by-doing", which ultimately improves their long-term employability (Almeida et al., 2014; Millan et al., 2017). Early work experience has been shown to translate to future career benefits that include higher hourly wages, increased annual earnings and less time spent out of work (Baum & Ruhm, 2014).

On the other hand, wage subsidies can have negative 'stigma' effects if they are seen by firms as verifying a disadvantaged status (Burtless, 1985). Further, firms may engage in "destructive churning" where they use the subsidies to substitute ineligible workers, including prior subsidised workers, with eligible workers (Malikane, 2012).[1] Finally, Heckman et al. (2003) show that in some cases subsidies may reduce incentives by the firm to invest in training of the subsidised worker, leading to detrimental longer-term employment effects.

This paper examines the impact of a wage subsidy on the employability of youth post-subsidy period. To explore this, we use the South African employment tax incentive (ETI) that was introduced by the South African government in 2014. The ETI is a firm-side targeted wage subsidy policy, an active labour market policy (ALMP), that was introduced to enhance the labour market prospects and reduce unemployment of the youth. Subject to age (between 18 and 29) and wage conditions (earn less than R6 000 per month), firms can claim a wage subsidy for employees for up to two years, with the subsidy halving in the second year. The claimable income is calculated based on monthly remuneration, with rates varying from 25% to 50% depending on the employee's salary and duration of employment.

[1] Jaenichen and Stephan (2011), exploring the effect of wage subsidies in Germany, highlight the potential for implementing punitive measures, such as the loss of subsidies, to deter this behaviour.

To conduct the analysis, we draw on anonymized matched employer-employee tax data from the South African Revenue Services (SARS). The data set combines four sources of administrative data, namely: corporate income tax data; data from employee income tax certificates submitted by firms for each worker employed; value-added tax data from registered firms; and customs records from traders (Pieterse et al., 2018; NT & UNU-WIDER 2020a, b). The data enables the tracking of workers and their respective firms over the period 2014 to 2020. We follow four cohorts of first-time workers including those that have been employed under the ETI for the first time in 2014, 2015, 2016, and 2017. The advantage of the data is its national representation of formal workers and firms and the plethora of firm characterises that allow for the in-depth exploration of the link between wage subsidy (ETI) and firm characteristics, while taking into account individual labour market prospects.

The empirical analysis of our paper utilises binary logistic models to examine the impact of ETI on the persistence (or survival) of first – time workers in jobs and employment in the labour market post-subsidy. To identify the ETI effect, we compare first-time ETI workers against a sample of first-time non-ETI workers who also fulfil the age and income requirements for the subsidy. We are also interested in whether ETI outcomes differ by gender and the type of firm (size, productivity, training expenditure, industry) in which workers are initially employed. We therefore estimate the marginal contributions of these firm and individual characteristics to ETI worker outcomes relative to non-ETI workers.

While this study aims to tease out the wage subsidy effect, there are empirical concerns that need to be addressed as well. Bias can be induced if workers with prior work experience are better able to signal their productivity capabilities to firms, and are thereby more likely to be selected for the wage subsidy. To mitigate these concerns, our study employs a pre-conditioning strategy by restricting our sample to first-time ETI and non-ETI workers who meet the ETI-regulated income and age conditions. In selecting first-time workers, we avoid potential biases associated with selection based on prior work experience.

In addition, there is potential selection bias arising from firms self-selecting into the Employment Tax Incentive (ETI) program, as discussed by Richardson (1998). To deal with this, we introduce characteristics of the firm (value added per worker, capital-intensity, firm size, and foreign ownership, amongst others) in which the worker was first employed as controls. Further, in estimates of job survival we introduce firm fixed effects. These estimates therefore estimate the effect of the ETI on job survival by comparing ETI relative to non-ETI workers that were employed within the same firm. Finally, we conduct robustness tests using propensity score matching (PSM) as an alternative method such that, in as far as the available covariates are concerned, selection bias will not be due to any of them, leading to a balanced distribution between the subsidised and unsubsidised workers.

This paper resolves three issues that have not yet been fully addressed in the available literature on the ETI. First, our study specifically focuses on the employment prospects of workers post-wage subsidy in an emerging economy context, and in relation to the ETI in particular. The work therefore extends available research that primarily estimates the impact of the ETI in generating new jobs (Ebrahim et al., 2017; Ebrahim & Pirttilä, 2019; 2025; Ranchhod & Finn, 2015, 2016). Second, the study controls for firm heterogeneity using the rich firm-level data available from the corporate income tax submissions in explaining the employment outcomes of the ETI. The importance of firm characteristics, other than firm size (Woldemichael et al., 2022), in driving both the selection of ETI workers and the persistence in employment has not been widely analysed in the case of South Africa, or internationally.

Yet, firm characteristics are important (for example, see Betcherman et al., 2010; Kaiser & Kuhn, 2016; Kangasharju, 2007). Understanding the link between firm characteristics and wage subsidy outcomes can aid in the design or targeting of wage subsidy programs. Thirdly, we unpack some of the channels through which the ETI affects persistence in employment post-subsidy, focusing specifically on the ability of workers to find alternative jobs. The across-job dynamics underpinning employment survival post-subsidy has not (to our knowledge) been explored. Finally, our study relates to the literature on the long-run labour market effects of early part-time work experience (Baum & Ruhm, 2014), vocational education and training (Oswald-Egg & Renold, 2021) and temporary job placements (Kvasnicka, 2009; Autor & Houseman, 2010). Our study contributes by analysing how subsidised first-time work experience outside of school influences medium-term labour market outcomes.

Our findings reveal that the medium-term employability and job prospects of first-time workers are more favourable for beneficiaries of the Employment Tax Incentive (ETI). ETI beneficiaries have a 15 percentage point higher probability (55% vs. 40%) of still being employed six years after their first year of employment, compared to non-beneficiaries. This gap persists even after controlling for individual and firm characteristics, and using alternative estimation approaches. We also find that employment survival is lower amongst first-time male workers, but the ETI offsets this disadvantage. Further, younger ETI workers have higher employment survival compared to older beneficiaries.

A key factor behind the higher employment survival of ETI workers is their ability to transition into new jobs and often multiple jobs. Very few first-time workers (ETI and non-ETI) remain in their first job over a sustained period, implying that employment survival in the formal private sector can largely be attributed to the finding of alternative jobs.

We argue that by incentivising firms to retain first-time workers for two years, the duration of the subsidy, the ETI exposes first-time workers to more job opportunities. In comparison to ETI workers, regular youth workers tend to exhibit longer job tenures within a specific job, and are less successful in finding alternative jobs. Conversely, ETI workers are observed to engage in job mobility, transitioning between multiple jobs, leading to higher prospects of sustained employment. Heterogenous firm effects also emerge, where we find that firm size, training, exporting, and productivity all lead to better employment prospects in general, however, the ETI effects are only more pronounced in large firms, while, interestingly, non-ETI workers seem to benefit more in more productive firms, and firms that provide training.

The remainder of the paper is structured as follows: Section 2 presents the overview of theoretical insights and related empirical literature review on wage subsidies and labour market outcomes. Section 3 presents the background context of the ETI. Section 4 discusses the methodological framework, which includes a discussion of the estimation strategy and the data. The discussion of the empirical findings is done in section 5 and finally, section 6 concludes.

2. Literature Review

2.1 The concept of wage subsidies

Wage and hiring subsidy programmes have been widely used in active labour market policies to generate employment opportunities and reduce unemployment. Wage subsidies serve as transfers to employees or employers to cover part of the eligible individuals' wage or non-wage

costs (Bördős et al., 2016). Worker-side wage subsidy initiatives involve providing subsidies to workers upon securing employment. Firm-side wage subsidies, on the other hand, are payments to employers that reduce the cost of hiring and retaining workers. One such firm-side policy is the Employment Tax Incentive in South Africa (see Ebrahim et al., 2017; Ranchhod & Finn, 2016). Our study focuses on firm-side wage subsidies, prompting us to direct our attention towards exploring the theoretical mechanisms through which these subsidies can impact labour outcomes.

Firm-side wage subsidies affect labour market outcomes through different theoretical channels (Heintz & Bowles, 1996; Burns et al., 2013; Hamersma, 2008). The subsidy lowers the effective wage paid by employers, which makes labour cheaper relative to other inputs. This may induce firms to substitute labour for capital and increase their output and employment levels, leading to a rise in overall employment levels as firms are incentivized to hire more workers than they would have without the subsidy (Burns et al., 2013). In addition to direct effects on employment within the firm, wage subsidies may indirectly impact labour market outcomes by affecting aggregate labour supply and demand dynamics. As the demand for labour increases due to subsidies, it can exert upward pressure on wages, resulting in higher wages for workers in subsidised firms and spill-over effects on wages in related industries.[2] The positive effects on wage levels and income distribution within the labour market may lead to an increase in consumption and investment spending. This may in turn stimulate aggregate demand and output increases in the economy, creating a positive feedback loop for employment (Pollin et al., 2006; Lewis, 2001). However, the overall effect of this channel is hinged on the wage elasticity of labour demand and the extent of the wage subsidy (Heintz & Bowles, 1996; Boockman, 2015).

Wage subsidies may also have negative effects on labour market outcomes. Wage subsidies may diminish the employment prospects for workers who are not eligible for the subsidy, such as older or more skilled workers. Firms may also substitute subsidised workers for non-subsidised workers within the firm, bringing about high levels of churn (Aislabie, 1980; Boockman, 2015). The churning effect, where subsidised workers may be replaced by new subsidised workers once the subsidy period ends, is another risk of inadequately monitored wage subsidies.[3] The net effect of a wage subsidy on employment within a firm, may therefore be substantially lower than the gross effect.

Imperfect market conditions may also diminish the employment-generating effectiveness of a wage subsidy program. Firms that hold market power can potentially exploit the subsidy as profit, reducing the motivation for them to increase employment or lower output prices. Additionally, unionized workers may bargain for higher wages, thereby diminishing the impact of the wage subsidy on employment levels (Burns et al., 2013; Go et al., 2009; Pauw & Edwards, 2006). Further, the subsidy may have no net impact on employment if it is paid to firms that would have hired workers anyway without the subsidy. This presents a deadweight loss and waste of public resources that could have been used for other purposes.[4] The net

[2] It's possible for the effect to be opposite. Given the subsidy, firms are not willing to pay the same wage for the same level of labour from the previous time period. This can potentially contribute to churn.

[3] In some countries, where wage subsidies have been used extensively, there are punitive measures in place for firms that intentionally substitute, in a rolling manner, subsidised employees for unsubsidized individuals (Bookman, 2015).

[4] A deadweight loss can occur if subsidized workers would have been hired even without subsidies. In this case, wage subsidies are superfluous and crowd out private employment, and the employment funds released by

effect of firm-side wage subsidies on labour market outcomes, therefore, depends on the relative strength of the channels discussed. These will vary according to the design and implementation of the subsidy program, as well as the characteristics of the labour market and the economy.

2.2. Empirical literature

Evaluating the effectiveness of wage subsidy programmes is complicated by enormous differences in subsidy programmes across countries. Wage subsidy programmes differ in terms of who is targeted (e.g., youth, unemployed, gender), the duration and size of the subsidy, whether the subsidy is paid to the firm or employee, and the conditionalities associated with the subsidy (e.g., training requirements, dismissal conditions) (Bördős et al., 2016). A meta-analysis of microeconometric evaluations of active labour market policies, including wage subsidy programmes, in advanced economies by Card et al. (2018)[5], finds that job search assistance generally has favourable impacts in the short run, while classroom and on-the-job training programmes are marginally effective in the short run, but have more positive impacts after two years. Subsidised public sector worker programmes are found to be relatively ineffective.

In the case of emerging economies, Bruhn (2016) uses monthly administrative data to examine whether firm-side wage subsidies can effectively enhance employment levels during periods of economic downturn in Mexico. Using difference-in-difference (DiD) estimation, together with the Euclidean distance matching, the study finds a positive but insignificant relationship between wage subsidy and employment during the programme. However, they show that employment levels recovered fast for eligible industries in comparison to non-eligible industries, suggesting the wage subsidy program implemented in Mexico is effective. Similarly, Betcherman et al. (2010) analyse the impact of two firm-side employment subsidy programmes targeting specific provinces in Turkey and estimate increases in employment of between 5% and 15% in the targeted provinces relative to other provinces. However, they find the cost of the actual job creation was high because of high deadweight losses. The dominant effect of subsidies was also to increase social security registration of firms and workers rather than boosting total employment and economic activity.

Results from studies conducted in advanced economies have found mixed outcomes. Some of the earlier wage subsidy programmes to be evaluated include the United States New Jobs Tax Credit of the 1970s and the Targeted Jobs Tax Credit (TJTC) of the 1980s. Katz (1998), for example, estimates that the TJTC increased the employment of disadvantaged 23–24-year-olds by 7.7%, although Bishop & Montgomery (1993) found that only a quarter of the subsidized jobs represented net new job creation. In the case of Finland, Kangasharju (2007) follows 31 000 firms between 1995 and 2002 and finds that wag subsidies targeting long-term or young unemployed individuals are effective in increasing employment in subsidized firms (by around 9%), with no significant effects on non-subsidized firms in the same industry or geographical area in Finland. In Germany, Jaenichen & Stephan (2011) use propensity score matching to investigate the effect of wage subsidies on the employment and unemployment

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the subsidy are used elsewhere in the firm (instead of employment). If, however, the funds released by the superfluous subsidy are used to employ other workers, the subsidy may be accomplishing its purpose.

[5] Their active labour market policies cover classroom or on-the-job-training, job search assistance, and subsidized jobs in the private and public sector.

rates of hard-to-place workers. They find that the share of employment for the targeted group rose from 25% to 42% as compared to the control group three years after the start of the programme.

In addition to potentially improving employment levels and wages, one of the important objectives of targeted wage subsidies is to enhance the long-term employment prospects of the participants. Not many studies have focused on this outcome of wage subsidies. Richardson (1998) analyses the impact of participation in the Special Youth Employment Training Program (SYETP) on the employment prospects of unemployed Australian youth and finds that wage subsidies extend short-duration jobs for young individuals as well as enhance their subsequent employability post-subsidy. Similarly, Caliendo et al. (2011), using a quasi-experimental estimation approach, observed large (10 to 20 percentage points) long-term effects (60 months after programme entry) for unemployed youth (aged 18 to 24) in Germany. Similarly, Eppel & Mahringer (2013) find persistent employment effects (7 years), but no wage effects, for beneficiaries of a wage subsidy paid to employers for recruiting long-term unemployed individuals in Austria. The impact was particularly large for older workers and the long-term unemployed.

Other studies are more sceptical about the sustained employment effects of wage subsidies. Kaiser & Kuhn (2016), for example, find that the Danish wage subsidy programme for highly educated workers had positive effects on employment and annual earnings during program participation, but no positive effects for the years after program expiration. Similarly, Groh et al. (2016) find positive, but short-lived employment effects of wage subsidies in Jordan, while Card & Hyslop (2005) find no long-term effects from the Self Sufficiency Project (SSP) in Canada that was designed to test whether a three-year earnings subsidy could help long-term welfare recipients transition from program dependency. While the SPP had significant short-term impacts on welfare participation and work within the first 18 months of the experiment (raising the full-time employment rate and lowering welfare participation by 14 percentage points) by a year and a half after all subsidy payments ended, the welfare participation rates of the two groups were equal. What the literature clearly suggests this, there are two distinct channels of wage-subsidy impact, namely improved employment, and sustained employment. A positive outcome in one channel does not imply a positive outcome in the other.

2.3. Literature on the South African context

There is a growing literature that examines the impact of wage subsidies (in some cases the ETI) on labour market outcomes in South Africa (Bhorat et al., 2020; Budlender & Ebrahim, 2021; Ebrahim & Pirttilä, 2019, 2025; Ebrahim et al., 2017; Ranchhod & Finn, 2015, 2016; Woldemichael et al., 2022). Ranchhod & Finn (2016), in one of the first papers to assess the impact of ETI on employment, applied a Difference-in-Differences (DiD) approach using labour force survey data and found no significant effects of ETI on employment and job churning six months after its implementation. They also found no result in their subsequent study (Ranchhod & Finn, 2015) that extended the window period to four quarters. However, as recognized by the authors, the low sample size given low take-up rates of the ETI in the first year reduces the statistical power of their analysis lowering their ability to identify any effects (Ranchhod & Finn, 2015).

Other studies have therefore examined the effectiveness of the ETI in South Africa using the much larger administrative tax database, but with mixed results. In one of the first studies

using this data, Rankin & Chatterjee (2016) estimate the impact of the ETI in the 2014/15 tax year. Their results reveal that firms that received the subsidy experienced a 10-percentage points higher expansion in employment compared to similar firms that did not receive the subsidy. They also observe stronger effects among firms with 800 employees or fewer, as well as positive wage effects for subsidised youth.

Other studies using the administrative tax data tend to find smaller effects of the ETI. Bhorat et al. (2020) combine a Difference-in-Differences methodology with propensity score matching and find a statistically significant but small impact on employment for the period 2013 to 2016. Small firms of fewer than 10 workers experienced the most employment growth from the ETI. They also find no evidence of displacement of non-ETI workers by ETI workers. In a similar vein, Budlender & Ebrahim (2021) employed a partial identification method utilising a DiD and find that the ETI led to a rise in youth employment at the firm level.

In contrast, Ebrahim et al. (2017) use the administrative tax data from 2012 to 2015 and find, on aggregate, no significant effects of ETI on changes in aggregate youth employment in the first 14 months of the policy initiative. The employment effects, however, are heterogeneous across firms, with positive and significant effects in small and medium firms, as is also found by Rankin & Chatterjee (2016) and Woldemichael et al., (2022). Ebrahim et al. (2017) also find a high percentage (92%) of deadweight loss that is arguably associated with large firms claiming the subsidy but not contributing to new employment creation.

Ebrahim & Pirttilä (2025) extend all these studies using a triple-Difference-in-Differences approach applied to a combination of survey data and administrative tax data over the period 2010 to 2018. They compare changes in employment, unemployment, entry, exit and earnings of targeted individuals (young, low wage workers) after the implementation of the policy, with two controls groups: young, higher-paid workers and older workers at the same earnings level. They find minimal general employment impacts for targeted individuals, but noteworthy heterogeneity with women's employment increasing, and earnings for men increasing due to the policy. Their study, however, only provides limited insight on the persistence of the employment effect.[6] The positive impact on employment of women is short-lived, and vanishes by the end of their analysis period. They also find a small positive increase in the number of targeted employees working for more than one year, but this could be driven by the eligibility of workers for two years of subsidy. Whether the policy increases employment beyond the subsidy window cannot easily be inferred from the result.

Overall, a key limitation of the available empirical literature on the ETI is that there has been little analysis of the employment dynamics of ETI beneficiaries post-subsidy period.[7] While there is some evidence that the subsidy has had a marginal positive impact on employment, there is little research on whether this employment is sustained once the subsidy ends. Further, there is little research conducted on how ETI workers respond to the ending of the subsidy in terms of remaining in the firm, finding alternative jobs, or exiting the formal job

[6] A potential limitation of their study (and others) using the administrative tax data is that workers may transition into formal employment in the private sector from the public sector, self-employment or informal sector, and not from unemployment. This would reduce the net effect of the policy on aggregate employment. Our study faces a similar limitation in that we are unable to observe if workers that leave the sample, enter into sources of employment not captured in the administrative tax data.

[7] Levinsohn et al. (2014) report on a randomized control trial of a wage subsidy programme in South Africa. They find evidence of persistence, with workers allocated a wage subsidy voucher that firms could claim for six months more likely to be in wage employment two years after allocation.

market. This paper contributes to the literature by providing new insights on some of the channels, namely subsidy-induced retention, and the ability to find alternative jobs that drive employment survival prospects, and how a wage subsidy influences this.

3. Estimation approach and data

3.1. Data source

This study draws from anonymised matched employer-employee South African Revenue Services (SARS) administrative tax data that has been made available through the UNU-WIDER/National Treasury initiative (SARS-NT) (NT and UNU-WIDER 2020a, b). The data includes Company Income Tax (CIT) and Employee Tax Certificate (IRP5), among others, and is accessed through a secure data lab at the National Treasury in Pretoria. The IRP5 tax forms are submitted by firms for each tax year and contain information on the firm name (anonymized), an anonymized individual identifier, the job spell, income, gender, and age. Importantly, the IRP5 form contains information on whether a worker was hired under the ETI initiative. Using the anonymized firm and individual identifiers, we are able to merge firm-level information from the CIT data with the IRP5 worker-level information, thereby producing a matched employer-employee database that tracks individuals and firms over time. We also use the prior years of data to identify first-time workers from 2010 to 2020.

There are several limitations with the data. It does not encompass informal workers, employment in the public sector, or self-employed individuals, although these are not severe limitations as the wage subsidy does not apply to these workers. While the dataset provides rich information on the firm, it is limited in terms of data on individual characteristics, covering only age and gender. Additionally, the identification of first-time workers is constrained by the administrative tax data, which only becomes comprehensive from 2011 onward, restricting the ability to track earlier formal employment histories. This may limit the depth of the analysis, particularly when investigating heterogeneous effects of the wage subsidy across different demographic groups. Finally, the data does not include individuals without a job. We are therefore unable to track how workers enter into or exit from unemployment in response to the subsidy.

3.2. Creating the study sample

A key empirical obstacle in evaluated the employment impact of the ETI is finding an adequate counterfactual to compare outcomes of beneficiaries to. Our focus is on whether the ETI has a differential effect on post-subsidy employment survival of beneficiaries compared to non-beneficiaries. Our approach is, therefore, to select a counterfactual sample of first-time employed youth in the data that closely mimics the characteristics of those who qualify and have benefited from the ETI.

As a first step, we restrict the sample of all workers to first-time workers falling within the targeted age group of 18 to 29 years and earning between R2000 and R6000 monthly income in the first year of employment. These are defined as ETI-eligible workers, some of whom benefit from the ETI. We focus on first-time workers, to exclude possible effects of prior employment experience on future labour market outcomes. The first year of employment (base year) is identified as the year we first observe the individual in the employment database. The administrative tax data commences from tax year 2009 providing us with a minimum of 5 years

(2009-2013) of information prior to the commencement of the ETI programme from which to identify prior work experience. To ensure sufficient length of period post-ETI, we restrict the sample of ETI-eligible workers to first-time workers in the tax years 2014, 2015, 2016, and 2017. We are therefore able to track our sample of first-time workers between 6 (2014 cohort) and 3 (2017 cohort) years after their first job, be it an ETI or non-ETI job. In some instances, first-time workers were employed in multiple firms. We exclude those individuals that benefit from ETI in one firm in their first year of employment, but not the other. Finally, we restrict the sample to cover only those workers for which we are able to match with firm data obtained from the CIT database. Further details on the data cleaning process are provided in the Data Appendix.

3.3. Data overview & Empirical analysis

Table 1 presents an overview of the final cohorts of first-time workers in each base year (2014 to 2017) split according to ETI status. Additional information is provided in Tables A1 to A2 in the Appendix. The number of first-time workers averages 270.8 thousand each year, ranging from 254.4 thousand in tax year 2014 to 283 thousand in tax year 2015. Given multiple jobs in different firms by workers, the number of first-time jobs in each base year is higher averaging 278 thousand each year. The table shows a rising trend in the share of ETI workers with employment from 19% in 2014 to 59.3% in 2017. The proportion of jobs held by ETI workers also rose from 18.5% in 2014 to 59% in 2017. These trends reveal a rising uptake by firms of the ETI subsidy.

Table 1*: Number of jobs and employment by ETI status in year of first employment.*

Base Year	Total	ETI	non-ETI	Share ET (%)
		Employment		
2014	254,416	48,314	206,102	19.0
2015	282,996	159,994	123,002	56.5
2016	270,234	154,179	116,055	57.1
2017	275,583	162,764	112,819	59.1
		Jobs		
2014	261,505	48,438	213,067	18.5
2015	290,165	164,171	125,994	56.6
2016	277,373	158,478	118,895	57.1
2017	283,599	168,052	115,547	59.3

Note: Own calculations using SARS-NT administrative tax data.

Table 2 presents a side-by-side comparison of average individual and firm characteristics of ETI workers and non-ETI workers in their first year of employment. The average ETI worker is more likely to be employed in firms that are larger in terms of sales, value-added, capital stock, profits and total workers, compared to non-ETI workers.[8] However, firm value added per worker (productivity), average wages and capital per worker are similar for ETI and non-ETI workers. The individual characteristics are similar across ETI and non-ETI workers. 56% of non-ETI first-time workers are male compared to 52% of ETI workers. Monthly incomes per

[8] For linear probability estimates explaining ETI status, see Table A3 in the Appendix.

job are similar ranging between R2235 (non-ETI) and R2264 (ETI), as is age (22.7 and 24.2), number of jobs per worker in base year (1.1) and months employed in each job in the base year (4.8 and 4.2).

The comparator data in Table 2 suggests that different firms select into employing ETI workers, with larger firms more likely to make use of the ETI programme. These selection effects may induce bias into our estimates of the employment outcomes of ETI workers post-subsidy period as firms may voluntarily choose to participate in the wage subsidy program based on certain characteristics that are correlated with employment outcomes. For example, larger firms that are more able to apply for the wage subsidies, may also select ETI workers with more productive attributes.

Table 2*: Comparison of individual and firm characteristics by ETI status of worker in first year of employment*

Definition of Variable	non-ETI	ETI
Firm Characteristics		
Foreign broad (%)	0,43	0,37
Exporter (%)	0,14	0,18
Importer (%)	0,15	0,19
Ln Value added	15,29	15,76
Ln Gross sales	16,16	16,64
Ln Capital	13,23	13,73
Ln Employees	3,04	3,47
Ln Input	15,47	15,98
Ln Labour costs	14,43	14,89
Ln Wages	15,36	15,68
Ln Profit	15,29	15,75
Ln of Capital per Worker	9,89	9,91
Ln Value added per worker	11,95	11,94
Worker Characteristics		
Share Male (%)	0,56	0,51
Ln Age at first employment	3,18	3,12
Number of Jobs per Worker	1,07	1,14
Months per job	1,58	1,44
Ln Monthly income	7,71	7,73

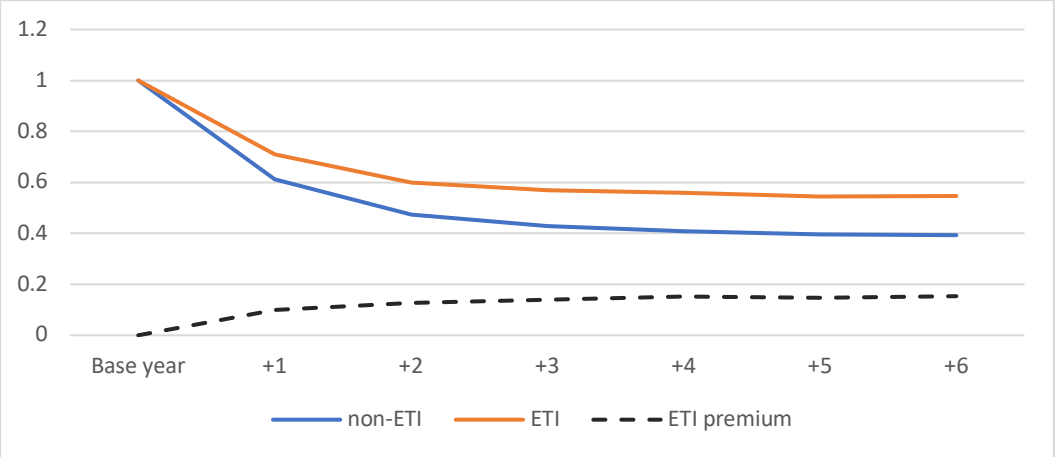
Note: Values are the simple average of firm and individual characteristics using the job-level data in the year of first employment. Ln denotes natural logarithm. Firms may employ both ETI workers and non-ETI workers.

We adopt three approaches to deal with this selection bias problem. First, we include controls for firm characteristics in the estimates. Secondly, in some estimates, we include firm fixed effects, such that we compare outcomes of ETI workers against non-ETI workers within the firm. These firm fixed effects control for unobserved time-invariant firm characteristics that may be correlated with the use of ETI subsidies. Second, we adopt a well-established approach in the literature – the propensity score matching (PSM)– to deal with the selection problem, balance covariates across groups, and reduce bias that may be due to pre-existing differences due to these covariates (Rosenbaum & Rubin, 1983; & Arendt, 2010; Ebrahim et al., 2017).

4. The nature of sustainable employment

For background analysis, *Figure 1* plots the employment survival rates for ETI and non-ETI workers in each year following their year of first employment (base year). Also presented is the ETI-premium that measures the difference in outcome for ETI workers compared to non-ETI workers. The survival rates are constructed by pooling the 2014 to 2017 cohorts of first-time workers. Employment here is defined as the status of having any remunerated job in any formal private sector company. An important caveat in the employment survival rates is that we are only able to track workers in registered firms that submit PAYE tax certificates to SARS. Workers who exit into informal, public sector or self-employment are not captured in the data.

Figure 1: Employment survival by ETI status



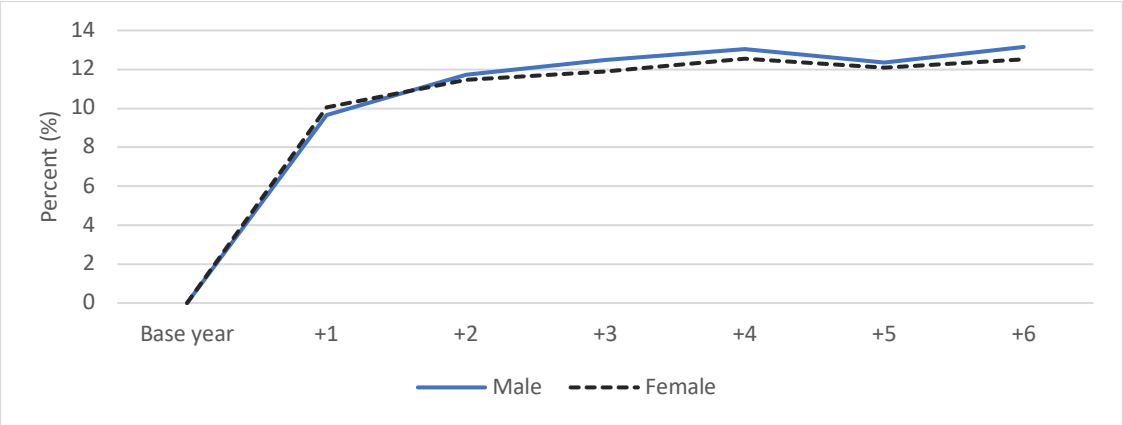
Notes: Based on pooled sample of first-time workers from 2014 to 2017. The ETI-premium is calculated as the difference between the ETI and non-ETI shares.

The figure shows a stark divergence in the share of subsidised and non-subsidised workers that remain employed (in any company) subsequent to their first year of employment. The survival rate of ETI beneficiaries is substantially higher than for other first-time youth employees. By year 6 after their first year of employment, 54.6% of ETI beneficiaries are still employed in a PAYE registered firm, whereas only 39.3% of non-ETI workers remain employed (a 15.3 percentage point gap). This gap is driven primarily by the relatively high exit rates of non-subsidised workers in the first and second years after their initial year of employment. For example, 29% of subsidised workers compared to 39% of non-subsidised workers lost employment in the year following the year they were first employed.

Figure 2 plots the ETI employment survival premium by gender. There are no significant differences in survival rates between male and female workers in the sample. Subsidised workers are more likely to remain employed compared to non-subsidised workers, irrespective of their gender. However, as shown in *Figure 3*, employment survival rates differ substantially for the different age cohorts, as does the ETI-premium. Younger workers in the age group 18-21 years are more likely to survive in employment 6-years after their first job (47.5%) compared to workers in the age group 22-25 years (41.7%) and 26-29 years (38.9%). The ETI appears to accentuate this gap, as shown by the ETI employment premium in part (b) of *Figure 3*. By year 6, the subsidised workers in the 18-21 years age group are 17.8 percentage points more likely to still be employed compared to non-subsidised workers. The ETI employment premium for the 22-25 years age group is slightly

lower at 15.4 percentage points but is substantially lower for the 26-29 years age group (8.4 percentage points). The data, therefore, suggest that while the ETI subsidy was raised employment survival of all beneficiaries, it has been particularly advantageous to younger first-time workers.

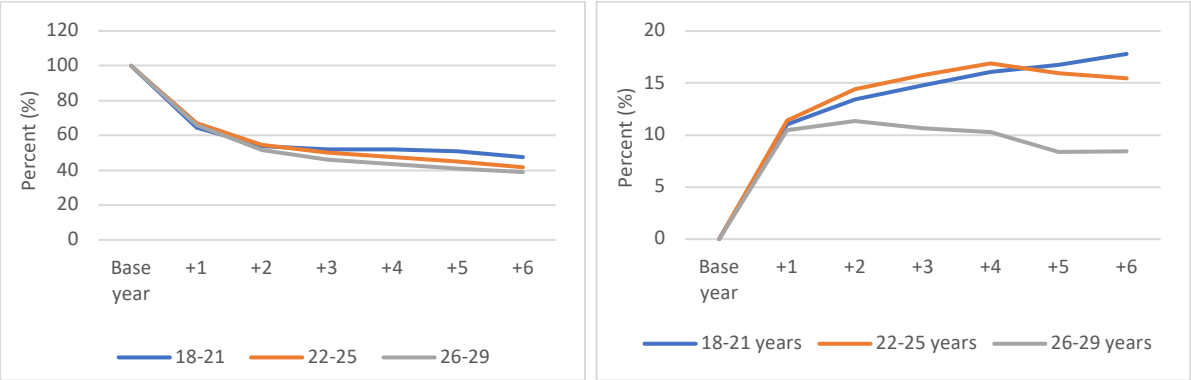
Figure 2: ETI employment survival premium by gender



Notes: Based on pooled sample of first-time workers from 2014 to 2017. The ETI-premium is calculated as the difference between the ETI and non-ETI shares.

Figure 3: Employment survival rates and ETI-premiums age cohort

(a) Survival rate *(b) ETI-premium*



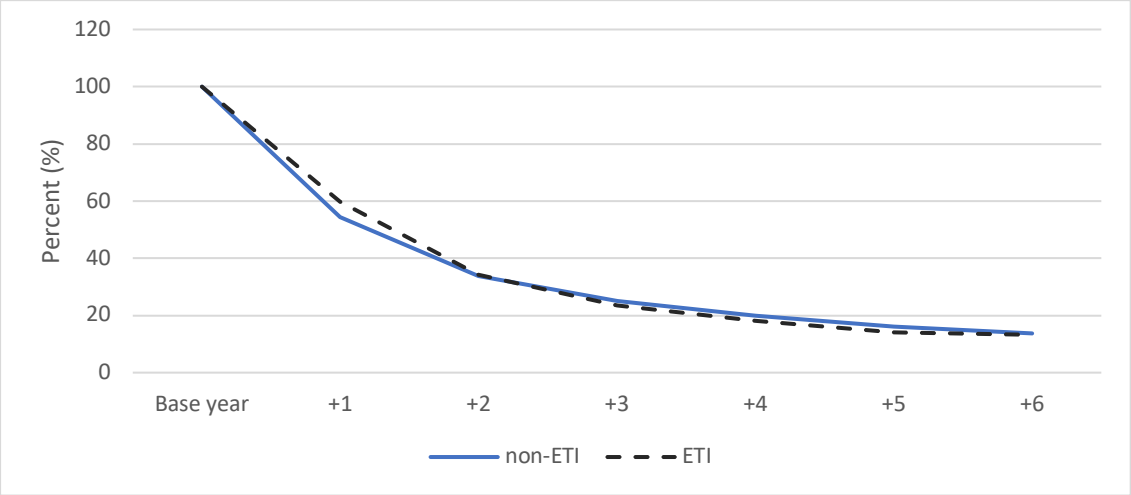
Notes: Based on pooled sample of first-time workers from 2014 to 2017. The ETI-premium is calculated as the difference between the ETI and non-ETI shares.

The higher employment survival rates of subsidised workers may be driven by several factors. Firstly, subsidised workers may be more likely to retain their initial jobs in their initial firms. Secondly, subsidised workers may be more likely to find new jobs in different firms. Thirdly, subsidised workers may be more likely to find new jobs and retain existing jobs. We explore each of these channels further.

4.1. Jobs as drivers of sustained employment

How long do subsidised workers stay in their initial jobs relative to non-subsidised workers? And what is the nature of job(s) after this initial job transition? Figure 4 presents the survival rates of first-time workers in their first job(s). A job here is defined as an employment position within a specific firm. While most workers were only employed in a single firm in the base year, several workers had multiple jobs. On average, first-time workers held 1.01 jobs in the base year, with no significant difference for subsidised and non-subsidised workers. We construct Figure 4 by tracking whether each worker remained employed or exited the job(s) they held in their first year of employment.

Figure 4: First job survival rates of ETI and non-ETI workers



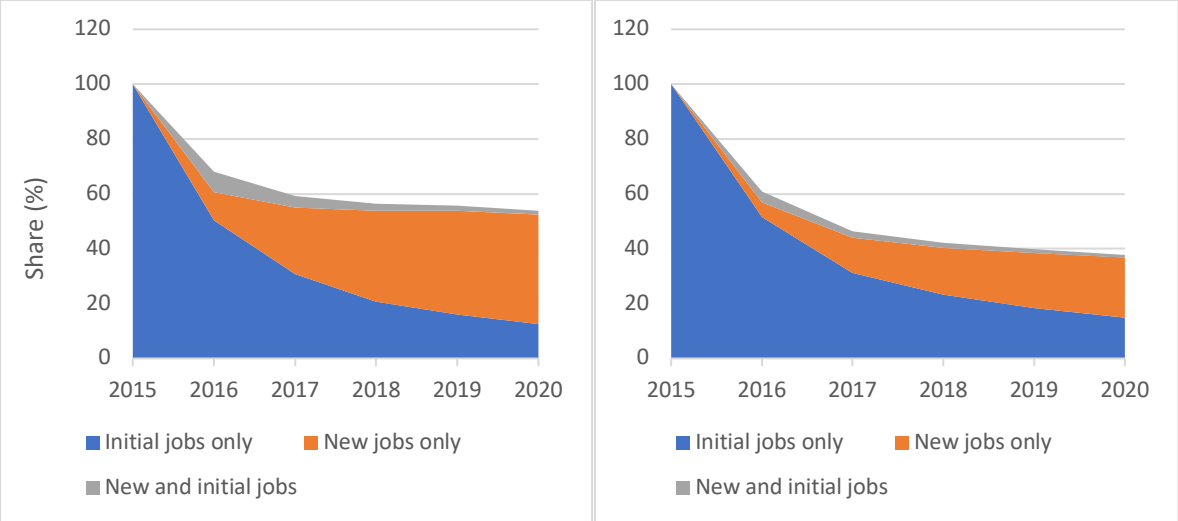
Notes: Based on job-level data pooled over the period 2014 to 2017.

Figure 4 provides four insights. Firstly, exit rates are very high (43%) for all workers in their second year of employment, but they are 5.4 percentage points lower for subsidised workers compared to non-subsidised workers. For example, 59.8% of subsidised workers compared to 54.3% of non-subsidised workers remained in their first job in their second year of employment. This is expected, as workers are eligible for two years of subsidy through the ETI programme. The ETI subsidy, therefore, appears to be relatively effective in retaining workers within the firm for the duration of the subsidy. Secondly, very few first-time workers remain in their initial job over the near-term. Only 13.5% of first-time workers remaining employed in the initial firm 6 years after the base-year. Thirdly, there appear to be no differences in the near-term survival rate of ETI and non-ETI beneficiaries. Finally, the job-survival results suggest that firms, on aggregate, do not substitute subsidised workers for non-subsidies workers within the firm, bringing about high levels of churn. If this were the case, we would expect substantially lower first-job survival rates of ETI workers compared to non-ETI workers after the subsidy period has ended. Subsidised and non-subsidised workers appear to be no different, at least in terms of the likelihood of remaining in their initial job, from one another after the subsidy period has ended.

The overall implication of these findings is that the higher employment survival rates of ETI workers shown in *Figure 1* are not determined by higher survival rates within their initial firms. The higher survival rates of ETI workers therefore must be driven by a higher probability that ETI workers are able to find an alternative job(s).

To analyse this further, Figure 5 presents the share of first-time workers by job status over time for the 2015 cohort of workers. As shown in the figure, the probability of remaining employed from 2016 onwards was substantially higher for ETI workers. In 2020, 46% of the 2015 cohort of first-time workers were no longer employed, compared to 62% of non-ETI workers. Also shown is that the share of workers that remained in their initial jobs only by 2020 is low, but there is little difference by ETI status. The gap is made up of workers who were able to find new jobs in different firms. By 2020, for example, 41% of ETI workers had found new jobs, whereas only 23% of non-ETI workers were able to do the same. The ability to find new jobs, therefore, appears to be an important factor in explaining employment survival and is particularly important in explaining the higher survival rates of ETI workers.

Figure 5: Share first-time workers by job status over time, 2015 cohort.



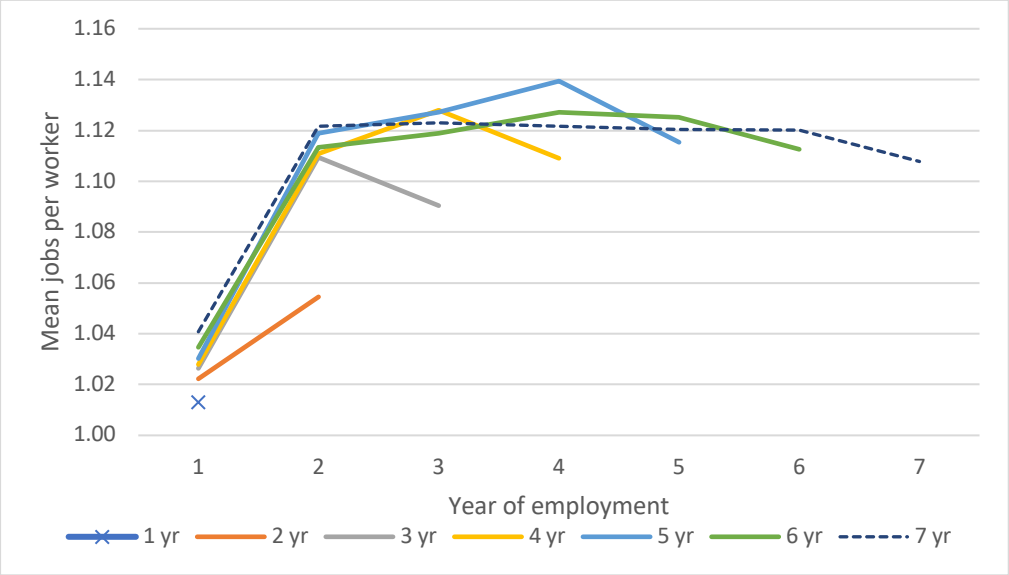
Notes: Based on pooled sample of first-time workers from 2014 to 2017.

We can gain further insight into what drives the employment outcome by looking at the evolution in the average number of jobs held by surviving workers. Figure 6 presents the average number of jobs held by workers in each year of employment. Given the possible correlation between number of jobs a worker has and probability of employment survival, we split workers according to employment spells.[9] The data shows that workers with more jobs in the base year and second year were more likely to remain employed in subsequent years. For example, workers that only retained employment in the base year had, on average, 1.01 jobs, compared to 1.04 jobs for workers that survived employment for at least seven years. Further, employment survival beyond two years (employment spells above 2) is strongly associated with a substantial increase in the average number of jobs in the second year of employment. The average number of jobs held by these workers rose from 1.03 in the base year to 1.11 in the second year of employment. Employment survival, therefore, is closely related to the ability of workers to find alternative jobs, and multiple additional jobs in particular.[10]

[9] The spell is defined as the total number of years employed. These are not necessarily sequential years of employment, as some workers exit and then re-enter into employment.

[10] What drives this process is not fully known. Workers may take on additional jobs for many reasons: because of survivalist needs, because they are only part-time employed in an existing firm, because they wish to

Figure 6: Mean jobs per worker by employment spell



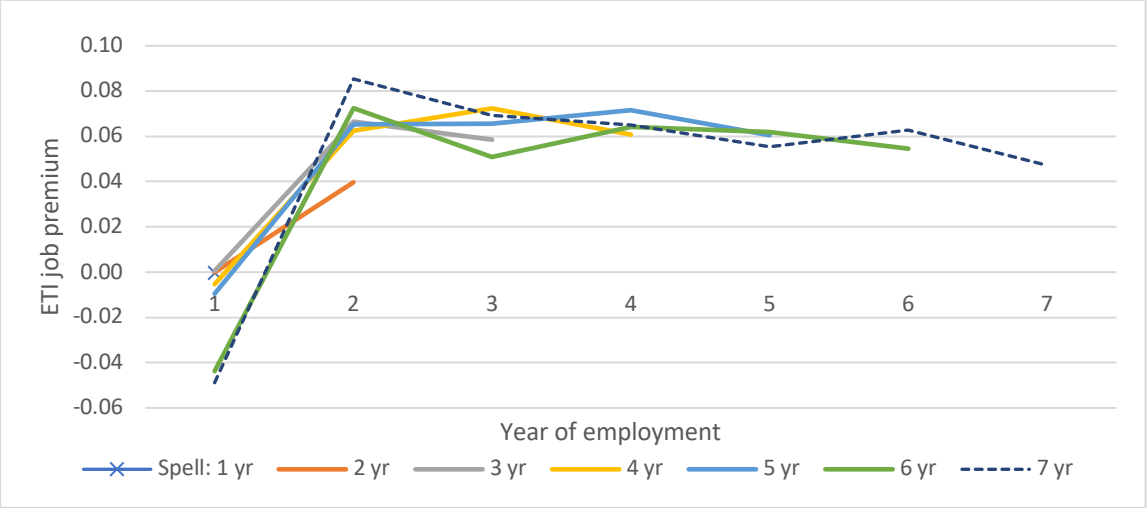
Notes: Based on pooled sample of first-time workers from 2014 to 2017. Employment spell here is calculated as the total number of years of employment per worker over the full period. To avoid bias from truncation of the data in 2020, we exclude those workers in each cohort (except 2014) that were employed for all years.

The ability to find additional jobs also appears to be a factor explaining the higher employment survival of ETI workers. Figure 7 presents the ETI-premium in terms of the average number of jobs for subsidised workers compared to non-subsidised workers. As can be seen in the figure, ETI workers who survived in employment experienced stronger increases in the number of jobs in their second year of employment compared to non-ETI workers. This premium remains throughout the remaining periods of employment. In other words, ETI workers who survive beyond the first year of employment differ from non-ETI workers in several respects: (a) they are more likely to find alternative jobs, (b) they are more likely to find additional jobs, particularly in their second year of employment, and (c) they retain more jobs.

There are several potential explanations for this outcome. By increasing the likelihood of being employed within the same firm for two years, the ETI may expose workers to additional skills and job networks, and through this to alternative employment opportunities in the labour market. Given the uncertainty of retaining the job after the subsidy period, ETI workers may also have a stronger incentive to find alternative employment opportunities in their second year of the subsidy period. Some of these relationships are now explored in more detail using econometric estimation in the next section.

increase incomes, etc. There appears to be very little literature studying multiple jobholding and employment survival.

Figure 7: ETI-premium in number of jobs per worker by employment spell



5. Empirical estimates

5.1. Specification

To empirically test if the ETI program is associated with higher employment survival, we estimate the following a linear probability regression model for each period *t* following the first year of employment (*t*=0):

$$E_{i,t} = \alpha + \beta^t ETI_i + \rho' X_i + \sigma' W_{i,t=0} + \mu_{cohort} + \varepsilon_{it} \qquad (1)$$

where the dependent variable, E_{it}, is dummy variable equals 1 if employee i is employed in period t following the start of employment, and 0 if otherwise. Our key covariate, ETI_i, is a dummy variable coded 1 if employee i is an ETI beneficial and 0 otherwise. X_i denotes a vector of individual characteristics (gender, age) while $W_{i,t=0}$ is a vector of initial (base-year) firm characteristics (firm size, productivity, trade status, and training) of the firm(s) the worker was initially employed in. We also include fixed effects for each of the 2014 to 2017 cohorts of workers to control for different take up in each year, and potential business cycles that affect outcomes of each cohort differently. Our coefficient of interest is β^t, the sign of which signals whether the ETI policy raises ($\beta^t > 0$), reduces ($\beta^t < 0$) or has no impact ($\beta^t = 0$) on employment survival of ETI workers relative to non-ETI workers.

The inclusion of variables for firm characteristics controls for the effect of firm heterogeneity on employment survival. For example, workers in more productive firms may benefit from learning- spillovers leading to higher future employment prospects. To test whether these characteristics have differential effects on ETI and non-ETI workers we extend the model to:

$$E_{it} = \alpha + \beta_1^t ETI_i + \beta_2^t F_{i,t=0} + \beta_3^t (ETI_i \times F_{i,t=0}) + \rho' X_i + \sigma' W^*_{i,t=0} + \mu_{cohort} + \varepsilon_{it} \qquad (2)$$

where $F_{i,t=0}$ is a variable measuring specific characteristics (e.g. whether a firm incurs training expenditure, firm productivity, firm size) of the firm(s) that the worker was first employed in, and $W^*_{i,t=0}$ covers other firm controls. Our coefficient of interest is β_3^t which

captures the marginal association of the firm characteristics on the future employability of ETI workers relative to non-ETI workers at year *t* following the start of the first job.

We estimate these specifications in multiple dimensions. Firstly, we estimate regressions where the outcome is the probability of being employed, regardless of any transitions across jobs in different firms. We run separate estimates for each year subsequent to the first year of entry (base year). Secondly, given that job mobility and the nature of transition between drivers is shown in the graphical analysis to be a key driver of employment survival, we estimate several regressions focusing on job transitions. These include estimates of the probability that a worker remains in their initial job in the years following the year of entry, as well as estimates of the number of jobs and the number of new jobs held by a worker in each period.

5.2. Effects of wage subsidy on employment survival

We first analyse the effect of ETI on employment survival, that is survival in any job in any firm using the specification in equation (1). Control variables include gender and age and base-year information on total income, number of jobs and other characteristics of the firm(s) (trade status, value-added per worker, size, capital per worker, ownership and whether firm incurred training expenditure) the worker was first employed in. In the case where a worker had multiple jobs in their first year of employment, we use the weighted average of the characteristics of firms the worker was initially employed in.[11] We also include fixed effects for each of the 2014 to 2017 cohorts. Table 3 presents the linear probability estimates.

The estimates corroborate the findings in Figure 1 and consistently show higher survival rates of ETI beneficiaries compared to non-beneficiaries. Looking at the first column, ETI workers have a 10.7 percentage point higher probability of remaining employed in the year following employment compared to non-ETI workers. This result is, in part expected, as ETI workers remain eligible for the subsidy, albeit at a lower rate. Persistence of the ETI effect is shown by the increase in the ETI coefficient to 11.2 percentage points in year 3 of employment (T+2) and the subsequent stabilisation between 11.2 to 11.6 percentage points up to the 7th year of employment (T+6). These results suggest that ETI workers maintain their employment status relative to non-ETI workers, despite the ending of the subsidy.

The results also provide insight on the relative importance of individual and firm characteristics in driving employment survival of first-time workers.[12] Individuals with higher average initial monthly incomes and initially employed in more productive firms are more likely to remain employed. Older workers have a higher probability of employment survival for the first 2 periods (T+1) and then lower probabilities of employment survival thereafter. Male workers are slightly less likely to remain employed for most periods, in comparison to their female counterparts, with the gap rising to 1.5 percentage points by T+7. Finally, workers employed for the first time in firms involved in international trade, specifically exporting firms, have higher probability of employment survival, whereas first time employees in foreign owned firms have lower employment survival rates.

[11] Weighted by the earned income from each job as constructed by Kerr (2020)

[12] See Table A6 for regression results explaining differences across firms in the utilisation of the ETI.

Table 3: Impact of ETI and firm characteristics on employment survival

VARIABLES	(1) T+1	(2) T+2	(3) T+3	(4) T+4	(5) T+5	(6) T+6
ETI	0.107**	0.112**	0.113**	0.116**	0.112**	0.113**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.003)
Male	-0.005**	0.006**	0.005**	-0.006**	-0.008**	-0.015**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Age	0.140**	0.056**	-0.036**	-0.110**	-0.146**	-0.111**
	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.007)
Monthly income	0.198**	0.159**	0.140**	0.130**	0.119**	0.115**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Number of jobs	0.119**	0.113**	0.114**	0.111**	0.109**	0.111**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.006)
Training	0.016**	0.022**	0.018**	0.017**	0.018**	0.022**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Value added per worker	0.010**	0.016**	0.018**	0.018**	0.020**	0.022**
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Fixed capital per worker	-0.001**	-0.002**	-0.002**	-0.004**	-0.004**	-0.005**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Firm size	-0.001**	0.004**	0.004**	0.003**	0.004**	0.005**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Exporter	0.010**	0.003+	0.005**	0.015**	0.015**	0.023**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Importer	0.002	0.008**	0.002	0.001	0.004	-0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Foreign owned	-0.009**	-0.008**	-0.010**	-0.005**	-0.009**	-0.004
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Constant	-1.591**	-1.240**	-0.854**	-0.544**	-0.364**	-0.480**
	(0.014)	(0.015)	(0.015)	(0.017)	(0.021)	(0.029)
Observations	985,436	985,436	985,436	734,591	487,772	228,385
R-squared	0.077	0.053	0.047	0.045	0.042	0.040

Note: Monthly income, age, value added per worker, firm size (measured as total firm employment) and fixed capital per worker are all in natural logarithmic form and pertain to the first year of employment. Foreign owned is defined according to the broad definition by Kilumelume et al. (2021). Robust standard errors clustered at the firm level in parentheses. ** p<0.01, * p<0.05, + p<0.1

To analyse potential heterogeneity in the impact of the ETI on employment survival across individual (gender and age) and firm characteristics (value added per worker, whether the firm incurred training expenditure, and firm size), we estimate equation (2). Table 4 presents the coefficient estimates. To simplify the presentation, only the coefficients on the interaction terms are presented. Each row and column combination presents the estimates of separate regressions.

Looking first at how the ETI effect differs by worker characteristics in regression rows (1) and (2), we find that the ETI policy has a marginally higher impact on employment survival of male workers relative to females. The ETI does not raise survival of male beneficiaries above women beneficiaries. Rather, it offsets the lower employment survival rates of males shown in Table 3, resulting in similar overall employment survival rates for this group of first-time

workers. These results, nevertheless, contrast those of Ebrahim & Pirttilä (2025) who find that the ETI policy had a small positive impact on employment of females relative to males. Looking at age, we find that the positive ETI effect on employment survival is lower for older beneficiaries. Ebrahim & Pirttilä (2025) find similar results, with younger individuals benefitting most in terms of employment from the ETI policy. The results in Table 4, also show that the employment survival gap between young and old ETI beneficiaries rises as the years following the end of the subsidy period increase.

Looking at firm characteristics in rows (3) to (5), we find that the boost in employment survival from the ETI is lower if the worker started in more productive firms, and firms that engage in training, but is higher if the first job was in larger firms (measured using firm employment size). The results suggest that the effect of the ETI on employment survival is mediated by the type of firm they were first employed in.

Table 4: ETI and heterogenous firm effects on employment survival

	(1) T+1	(2) T+2	(3) T+3	(4) T+4	(5) T+5	(6) T+6
Interaction variables						
(1) ETI x Male	0.011**	0.017**	0.019**	0.016**	0.014**	0.024**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.005)
(2) ETI x Age	-0.044**	-0.108**	-0.181**	-0.245**	-0.322**	-0.351**
	(0.007)	(0.007)	(0.007)	(0.008)	(0.011)	(0.019)
(3) ETI x Training	-0.009**	-0.010**	-0.009**	-0.007**	-0.006+	-0.007
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.006)
(4) ETI x Value added/worker	-0.025**	-0.027**	-0.027**	-0.025**	-0.021**	-0.022**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
(5) ETI x Firm size	0.010**	0.013**	0.009**	0.007**	0.005**	0.003**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)

Note: Worker age, value added per worker, firm size as measured by number of workers are in natural logarithms. Training is a dummy variable if the firm incurred training expenditure. Each interaction row presents the coefficients of separate regressions. All regressions include the full set of individual and initial firm controls. The coefficient on ETI status is positive and significant in all estimates. Robust standard errors are in parentheses. ** p<0.01, * p<0.05, + p<0.1

Despite the process of selecting first-time workers and the inclusion of several controls in the regressions, there is still a possibility of selection bias. For example, it is possible that better survival prospects of ETI workers are largely driven by larger firms with capacity to capitalize on the ETI policy. To enhance the robustness of our analysis on employment survival, we employ a propensity score matching (PSM) approach. Using this approach, we create a control group (non-ETI participants) that closely resembles the treatment group (ETI participants) in terms of observable characteristics. We then estimate a 'propensity score' indicating the probability of participating in the subsidy program and match control group members to treated individuals with similar scores.[13]

[13] We make use of the *psmatch2* command in STATA.

5.2.1 Robustness tests

Table 5: PSM sample for employment survival analysis

	(1) T+1	(2) T+2	(3) T+3	(4) T+4	(5) T+5	(6) T+6
			Base regressions (Equation 1)			
ETI	0.109**	0.114**	0.116**	0.121**	0.117**	0.117**
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)
Observations	644,774	644,774	644,774	491,668	330,352	94,774
R-squared	0.082	0.055	0.048	0.044	0.039	0.037
			Interactions (Equation 2)			
ETI	0.101**	0.106**	0.106**	0.111**	0.107**	0.097**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.005)
Male	-0.012**	-0.004*	-0.009**	-0.015**	-0.014**	-0.019**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
ETI x Male	0.015**	0.013**	0.019**	0.014**	0.009**	0.020**
	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)	(0.006)
ETI	0.118**	0.122**	0.123**	0.128**	0.119**	0.105**
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.006)
Training	0.019**	0.025**	0.020**	0.019**	0.022**	0.022**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
ETI x Training	-0.013**	-0.012**	-0.010**	-0.012**	-0.009*	0.005
	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)	(0.007)
ETI	0.472**	0.629**	0.824**	0.960**	1.142**	1.203**
	(0.026)	(0.028)	(0.028)	(0.033)	(0.041)	(0.075)
Age	0.180**	0.109**	0.048**	-0.015*	-0.033**	-0.042**
	(0.006)	(0.006)	(0.006)	(0.007)	(0.008)	(0.010)
ETI x Age	-0.115**	-0.164**	-0.225**	-0.267**	-0.327**	-0.348**
	(0.008)	(0.009)	(0.009)	(0.010)	(0.013)	(0.024)
ETI	0.311**	0.320**	0.330**	0.314**	0.256**	0.223**
	(0.010)	(0.010)	(0.010)	(0.012)	(0.015)	(0.025)
Value added/worker	0.025**	0.029**	0.032**	0.032**	0.030**	0.029**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
ETI x Value added/worker	-0.017**	-0.018**	-0.018**	-0.017**	-0.012**	-0.010**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
ETI	0.089**	0.073**	0.092**	0.104**	0.110**	0.124**
	(0.003)	(0.003)	(0.003)	(0.004)	(0.005)	(0.008)
Firm size	-0.001**	0.003**	0.005**	0.005**	0.006**	0.009**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
ETI x Firm size	0.003**	0.007**	0.004**	0.002**	0.000	-0.002+
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)

Note: Initial income, age, value added per worker, firm size (measured as total firm employment) and fixed capital per worker are in natural logarithmic form and pertain to the first year of employment. A full set of individual and firm controls are included in each regression. Robust standard errors clustered at the firm level in parentheses. ** p<0.01, * p<0.05, + p<0.1

The PSM-adjusted results in Table 5 corroborate the finding that ETI workers have higher employment survival rates than other young first-time workers, with a 10.9 to 12.1 percentage point higher probability of remaining employed in the years following the subsidy. The

interaction effects are also consistent with the prior results, although they are slightly smaller in magnitude.

As a further test, we use a Poisson pseudo-maximum likelihood estimator to regress the count of years each first-time worker is employed on the ETI dummy variable and a full set of firm and individual controls. The results are presented in Table A4 in the Appendix. ETI status is associated with a 1.2-fold increase in the number of years of employment for the 2014 cohort. This falls to 1.12-fold for the 2017 cohort given the shorter window period 2017 to 2020. Overall, the finding of higher employment survival of ETI beneficiaries compared to non-beneficiaries is robust to alternative estimation approaches.

6. How does ETI lead to more sustained employment?

What drives the higher employment survival of ETI beneficiaries? The descriptive analysis in section 4 shows that the higher employment survival rates of ETI beneficiaries are driven by a higher likelihood that they find alternative jobs. This is shown in Figure 4 by the similar medium-term survival rates within the first job between ETI and non-ETI workers. To test the robustness of these findings to the addition of controls, we re-estimate equation (1), but at the individual job level. In other words, we analyse differences in survival between ETI and non-ETI workers in their initial job.

Table 6 presents the linear probability estimates for each year following entry into the first job. The estimates include firm fixed effects, so all firm characteristic variables are excluded. The association is identified by the differences in survival between ETI and non-ETI beneficiaries within the firm. The results show that ETI workers have a higher probability of remaining within the same job for the first 3 years after entry into their initial job. In the second year of employment, the probability of ETI beneficiaries remaining employed in the firm is 16.4 log points higher than non-ETI beneficiaries. This is expected as the ETI policy provides funding for up to two years. From year 2 after entry, however, the probability premium for ETI workers falls and by the 5th year after entry, becomes insignificant.

Table 6: Probability of survival in initial job

	(1) T+1	(2) T+2	(3) T+3	(4) T+4	(5) T+5	(6) T+6
ETI	0.164**	0.081**	0.047**	0.027+	0.013	0.007
	(0.015)	(0.015)	(0.014)	(0.014)	(0.011)	(0.010)
Monthly income	0.245**	0.155**	0.108**	0.081**	0.064**	0.058**
	(0.010)	(0.008)	(0.007)	(0.007)	(0.006)	(0.006)
Age	0.227**	0.244**	0.203**	0.165**	0.128**	0.081**
	(0.029)	(0.031)	(0.028)	(0.022)	(0.021)	(0.016)
Male	-0.024**	-0.027**	-0.029**	-0.030**	-0.028**	-0.030**
	(0.003)	(0.005)	(0.004)	(0.004)	(0.003)	(0.003)
Constant	-2.133**	-1.670**	-1.255**	-0.963**	-0.739**	-0.556**
	(0.143)	(0.138)	(0.125)	(0.105)	(0.097)	(0.078)
Observations	946,257	946,257	946,257	703,061	466,202	216,974
R-squared	0.326	0.276	0.264	0.259	0.258	0.261

Note: Monthly income and are in natural logarithmic form are pertain to the first year of employment. Firm fixed effects are included. Robust standard errors clustered at the firm level in parentheses. ** p<0.01, * p<0.05, + p<0.1.

A key implication of these results is that the ETI appears to be effective in retaining ETI beneficiaries within the firm they are first employed for up to 4-5 years. Thereafter, the probability of first-time workers remaining employed in their initial is no different between ETI and non-ETI workers. Note, however that by year 7 after entry, only 13.5% of first-time workers are still employed in their initial firm. The workers have either exited formal private sector employment, or found alternative jobs. The significantly higher probability that the ETI workers remain employed compared to non-ETI workers must be driven by a higher likelihood that the ETI beneficiaries find alternative jobs.

We find support for this in Table 7 where we run estimates of equation (1), but use the count of jobs and count of new jobs held by individual workers as the dependent variables. A new job is defined as any job that was not held in the first year of employment. The relationship is estimated using a Poisson pseudo-maximum likelihood estimator, with each regression including a full set of individual and firm controls. Only the coefficients on the ETI indicator are presented. The results show that ETI workers have 0.4% fewer jobs than non-ETI workers in their first year of employment. Given the positive association between number of jobs and employment survival shown in the Table 3 regression results, we would expect the non-ETI workers to therefore have greater future employment prospects compared to ETI workers. However, in the second year of employment, we find a significant rise in the number of jobs held by ETI workers compared to non-ETI. ETI workers that remain employed experience a 45.5% increase in the number of jobs relative to non-ETI workers that remain employed. Over time, the gap in terms of number of jobs and number of new jobs between ETI and non-ETI workers diminishes, but still remains statistically significant.[14]

Table 7: ETI premiums in number of jobs and number of new jobs after year of first employment

	(1) T+0	(2) T+1	(3) T+2	(4) T+3	(5) T+4	(6) T+5	(7) T+6
(1) Number of jobs	-0.004**	0.051**	0.048**	0.043**	0.039**	0.037**	0.032**
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.003)
Observations	985,436	656,741	540,782	507,342	366,147	233,613	102,933
(2) Number of new jobs		0.375**	0.317**	0.261**	0.200**	0.171**	0.135**
		(0.006)	(0.004)	(0.003)	(0.003)	(0.004)	(0.005)
Observations		656,741	540,782	507,342	366,147	233,613	102,933
Percentage difference in jobs: ETI vs. non-ETI							
Number of jobs	-0.4%	5.2%	4.9%	4.4%	4.0%	3.8%	3.3%
Number of new jobs		45.5%	37.3%	29.8%	22.1%	18.6%	14.5%

[14] We also run estimates where we include an interaction between ETI and gender. We find that males are more likely to find multiple new jobs than women, which helps explain the larger positive impact of the ETI for men compared to women.

Notes: Dependent variable is the count of jobs held by the individual in year *t* after first entry into employment. Only workers with jobs are included. The relationship is estimated using a Poisson pseudo-maximum likelihood estimator. Each result is estimated in a separate regression. A full set of individual and firm controls are included in each regression. The percentage difference in jobs between ETI and non-ETI beneficiaries is calculated as exp(coefficient)-1. A new job is defined as any job that was not held in the first year of employment. Robust standard errors are presented in parentheses** p<0.01, * p<0.05, + p<0.1.

To test the influence of finding alternative and multiple jobs on the ETI premium, we repeat the linear probability estimates of equation (1) and test the sensitivity of the ETI coefficient to inclusion of controls for the number of jobs held in the first couple of years. As an example, Table 8 presents the estimates of the probability of employment survival in the 5th year after entry into employment. The first column replicates the regression results presented in column (5) of Table 3, and shows the ETI employment survival premium of 11.2 percentage points. In column (2) we add the number of jobs held by the worker in the second year. The ETI coefficient falls to 0.08. It falls further to 0.057 if we also include the number of jobs held in the third year of employment. In other words, the relative strong increase in the number of jobs held by ETI workers in their second and third year of employment explains close to half of the higher probability of employment of ETI workers in their 5th year after their first job.

In summary, the result of this section suggests that ETI workers are more successful in finding and maintaining new jobs and multiple jobs compared to non-ETI workers, with much of the action taking place in the second year of employment. This outcome plays a considerable role in explaining the higher employment survival rates of ETI workers in the medium-term. Several possible explanations may underpin this outcome. First, by incentivising the retention of workers for at least two years, the ETI provides first-time employees with more time to gain experience and establish employment networks with other employees and firms. Under this explanation, the ETI would help alleviate two constraints to youth employment in South Africa identified by De Lannoy et al. (2020) in their review of the literature: (a) the ETI provides workers with a longer opportunity to improve their productivity through 'learning by working' and signal their productive capabilities to new employees, and (b) the ETI enables workers to build stronger networks in the labour market assisting workers to overcome high costs and limited social capital barriers to employment arising from the apartheid-driven spatial mis-match between employer and employee.

Table 8: Probability of employment survival in 5th year after entry into employment

	(1)	(2)	(3)
ETI	0.112**	0.080**	0.057**
	(0.002)	(0.002)	(0.002)
Number of initial jobs	0.109**	0.069**	0.046**
	(0.004)	(0.004)	(0.004)
Number jobs year 2		0.193**	0.077**
		(0.001)	(0.001)
Number jobs year 3			0.268**
			(0.001)
Observations	487,772	487,772	487,772

R-squared	0.042	0.090	0.176

Note: Each regression includes controls for initial income, age, value added per worker, firm size (measured as total firm employment) and fixed capital per worker (in natural logarithms) in first year of employment. Robust standard errors clustered at the firm level in parentheses. ** p<0.01, * p<0.05, + p<0.1

A second explanation is that job search behaviour of ETI workers changes in their final year of subsidy. Given the forthcoming termination of the subsidy, ETI worker may be more proactive than other workers in searching for additional jobs to minimise the risk of unemployment. This explanation would require the ETI workers to know their subsidy status. Unfortunately, we are unable to ascertain whether this is the case given the data available.

7. **Conclusion**

This paper uses administrative employer-employee tax data to analyse the persistent effects of wage subsidies on employment prospects post-subsidy. To do so, the study uses the South African youth employment tax incentive (ETI) introduced in 2014 as a case study. Leveraging anonymized matched employer-employee administrative tax data spanning 2014 to 2020, the study tracks four cohorts of first-time workers and compares the employment survival probabilities of ETI participants relative to their non-ETI counterparts. Additionally, we explore how firm characteristics impact the effectiveness of the ETI for sustained jobs and employment, and analyse how the employment survival prospects differ by gender and age of the worker.

The findings reveal higher probabilities of employment survival of ETI beneficiaries relative to non-beneficiaries post-subsidy. For example, ETI beneficiaries have a 15 percentage point higher probability (55% vs. 40%) of still being employed six years after their first year of employment, compared to non-beneficiaries. This gap falls to 11 percentage points once firm controls are included, but remains significant and is robust to the use of alternative estimation approaches, such as the PSM method. The ETI effects are found to benefit male workers relative to female workers and younger workers relative to older workers.

Our analysis shows that the higher employment survival of ETI workers is not driven by higher probabilities of remaining in their initial job, but rather by their ability to transition into new jobs and multiple jobs relative to non-beneficiaries. We argue that this partly works through incentivising retention by firms of subsidised workers for the two-year duration of the subsidy. This may provide ETI workers with more time to improve productivity through ‘learning by working’, and build stronger job-finding networks in the labour market.

Several policy insights follow from the analysis. Firstly, duration of the subsidy period appears to be one factor contributing towards higher future employment prospects. Incorporating conditions into the ETI policy requiring firms to retain workers for the full duration of the subsidy period, and potentially for a period afterwards, may help further increase the effectiveness of the policy in raising employment survival of beneficiaries. For example, in Germany, firms participating in the wage subsidy programme were mandated to keep wage subsidised workers in their firm for a minimum period post-subsidy duration, or risk missing out on the rebate (Caliendo et al., 2011).

Secondly, the finding of alternative jobs is critical in driving employment survival in the formal private sector. Very few first-time workers remain in their initial job for sustained periods. Labour market support that goes beyond just that of finding jobs for the unemployed, but also bolsters the ability of existing workers to signal their experience and improve the search and matching of workers with employers can enhance the employment survival prospects of all workers. This could include job search assistance (support in preparing applications, recruitment fairs, information on vacancies, etc.) and certified training support within the firm, although the evidence in support of the effectiveness of these policies in emerging economies is not as strong as policy makers generally expect (McKenzie, 2017). Given South Africa's history of spatial segregation, supply-side policies may be most effective if they deal with providing information to workers (and unemployed) on job opportunities in different locations. Alternative approaches to supply side interventions may include help to firms to overcome onerous regulations and labour laws that limit firms hiring and growing (McKenzie, 2017).

Thirdly, the empirical analysis shows that larger firm are more likely to make use of the ETI programme. There are several advantages to being employed in large firms. Generally, wages are higher, and first-time workers, particularly ETI-workers, in large and productive firms have higher future employment survival rates. Large firms therefore play a key role in raising employment prospects of workers. However, the effectiveness of the ETI policy in South Africa must also account for the critical role of small and medium-sized enterprises (SMEs) in driving employment creation (Abor & Quartey, 2010). More research on why small firms are less likely to take up the ETI is required. Reducing the cost of complying with the ETI may be one avenue to achieve higher take-up.

These insights show that designing a wage subsidy initiative requires a deep understanding of employment prospects within specific labour market contexts, as such initiatives may not be directly transferable from one region to another. A key limitation of this study is the lack of an ideal counterfactual. It is not possible to track individuals before they enter the formal labour market or after they exit, making it unclear whether they transition into unemployment or the informal sector. This is particularly important in an African context, where informal employment makes up a large share of economic activity. Understanding these transitions would help in designing wage subsidies and complementary support structures that better sustain employment. Finally, wage dynamics also matter. Ebrahim & Pirttilä (2025) find positive earnings effects from the ETI policy. Whether these earnings effects persist, and how wage differentials across jobs drive job transitions, however, have not yet been explored.

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Data Appendix

Several challenges were faced in cleaning and preparing the data. The administrative tax data covers the tax years 2009 to 2020, with the ETI first implemented in October 2013. Tax years are defined over the period March to February the following year (e.g., 2010 tax year covers period from March 2009 to February 2010). We use two approaches to identify the tax year in which the worker was first employed, which we refer to as the *base-year* of employment. Firstly, we use the tax year where the worker is first observed in the IRP5 data to identify the first-year of employment. The administrative tax data commences from tax year 2009 providing us with at a minimum 5 years (2009-2013) of information prior to the commencement of the ETI programme from which to identify prior work experience. We retain all individuals whose first year of employment is between tax years 2014 to 2017. Secondly, each IRP5 tax submission contains a variable stipulating the 'period employed from'. In most cases, this corresponds with a date within the tax year, but in some cases start dates prior to the tax year of the submission are provided (Kerr, 2020). We then exclude these individuals from the sample of first-time workers. To ensure the sample of first-time workers falls within the stipulated age group of the ETI programme, individuals outside of the targeted 18- and 29-year-old group in their first year of employment are also excluded.

A further challenge arose in dealing with workers with multiple jobs across and within firms. The IRP5 tax submissions are at the job level, and the duration of the job can vary from days to a full year. Individuals can also have multiple jobs across different firms within a tax year, some of which may be supported by the ETI, and some of which are not. To ensure a clean comparison of ETI workers and non-ETI workers, we exclude all individuals who have both ETI and non-ETI jobs in different firms in their first year of employment (the base year). To constrain the sample to include first-time workers that fall within the targeted income range (R6000 or lower) of the ETI programme, we calculate the average monthly income for the worker over all jobs within the tax year. This required standardising the period worked and income within each job to a monthly unit.[15] Average monthly income is then calculated using the total months worked and total income of the first-time worker across all jobs in the base year. Workers with average incomes greater than R6000 per month in their base year are excluded.

The cleaned firm-level data are then merged with the employee data using anonymized identifiers. This provides us with a jobs-level employee panel with base-year firm characteristics data. Using this data, we can observe the employment status, income and months worked in each job of the 2014 to 2017 cohorts of first-time ETI and ETI-eligible workers up to tax year 2020. We are therefore able to track whether a first-time worker exits their initial job (s), enters a new job(s) or exits employment in any job in years subsequent to commencing employment.

[15] We use a combination of approaches to calculate the monthly duration of each job. First, we use information provided in the IRP4 covering the total periods worked and the period unit (day, month, year, etc.). Secondly, we calculate the days worked using the information on start and end date of job provided in the IRP5. The days worked using these different approaches do not always tally with each other. Our preferred measure is that based on the stipulated start and end date of the job, but when missing we draw upon the specified periods worked. We exclude individuals if any job in the base year is missing information on income or days worked.

One of our interests is to understand how firm characteristics in the base year of employment affect future employment and wage prospects of ETI beneficiaries. One consideration in this regard is how to map firm characteristics to first-time workers with multiple jobs in their first year of employment. Our approach is to construct an earnings-weighted average of base year firm characteristics for each first-time worker with multiple jobs. We exclude multiple job individuals where at least one of the jobs is not matched to a firm in the base year.

Appendix: Tables and Figures

Table A1: Employment and job numbers by year for each cohort: ETI vs non-ETI

	Tax year						
	2014	2015	2016	2017	2018	2019	2020
Cohort year	**Employment ETI**						
2014	48845	40181	31291	27939	25735	24673	22885
2015		165950	114920	97176	88344	84038	77816
2016			161027	115757	96709	90033	83392
2017				175265	126058	101561	91822
	Employment non-ETI						
2014	220731	138248	107855	96211	87978	82225	76665
2015		131787	82156	61732	55171	50912	46968
2016			125804	78318	60080	53318	48638
2017				126322	77495	59335	51841
	Jobs ETI						
2014	60309	56082	44094	40659	39628	39204	37375
2015	0	202826	153269	133230	127670	126846	120849
2016	0	0	201656	158323	135140	129927	123640
2017	0	0	0	214875	169205	141718	131858
	Jobs non-ETI						
2014	320152	205355	163504	148200	140600	138078	133202
2015	0	200875	128752	97018	88806	85195	81108
2016	0	0	191030	121607	94650	86998	82224
2017	0	0	0	190902	119188	93253	83900

Note: The tax year is between March and February the following year. For example, the tax year 2019 runs from 1 March 2018 to end February 2019. We follow Pieterse et al. (2018) in linking the financial year of the firm to the tax year.

Table A2: Survival rates of first-time Jobs & workers in each year (cohort): ETI vs non-ETI

Base Year	2014	2015	2016	2017	2018	2019	2020
	Employment ETI						
2014	100,0	82,3	64,1	57,2	52,7	50,5	46,9
2015		100,0	69,2	58,6	53,2	50,6	46,9
2016			100,0	71,9	60,1	55,9	51,8
2017				100,0	71,9	57,9	52,4
	Employment non-ETI						
2014	100,0	62,6	48,9	43,6	39,9	37,3	34,7
2015		100,0	62,3	46,8	41,9	38,6	35,6
2016			100,0	62,3	47,8	42,4	38,7
2017				100,0	61,3	47,0	41,0
	Jobs ETI						
2014	100,0	93,0	73,1	67,4	65,7	65,0	62,0
2015		100,0	75,6	65,7	62,9	62,5	59,6
2016			100,0	78,5	67,0	64,4	61,3
2017				100,0	78,7	66,0	61,4
	Jobs non-ETI						
2014	100,0	64,1	51,1	46,3	43,9	43,1	41,6
2015		100,0	64,1	48,3	44,2	42,4	40,4
2016			100,0	63,7	49,5	45,5	43,0
2017				100,0	62,4	48,8	43,9

Table A3: Linear probability estimate explaining ETI status

	(1)
Male	-0.016**
	(0.001)
Age	-0.518**
	(0.003)
Firm size	0.054**
	(0.000)
Training	0.003**
	(0.001)
Value Added per worker	-0.003**
	(0.000)
Fixed Capital per worker	-0.000
	(0.000)
Exporter	-0.018**
	(0.002)
Importer	-0.007**
	(0.002)
Foreign Owned	-0.073**
	(0.001)
Constant	1.880**
	(0.012)
Observations	958,620
R-squared	0.247

Notes: Dependent variable is a binary variable that equals 1 if the worker is an ETI beneficiary (zero otherwise). Sample is restricted to the first year of employment. Separate intercepts for each 2014 to 2017 cohort are included. Robust standard errors are presented in parentheses** $p<0.01$, * $p<0.05$, + $p<0.1$.

Table A4: Regression explaining number of years employed

VARIABLES	(1) Overall	(2) 2014	(3) 2015	(4) 2016	(5) 2017
ETI	0.168**	0.184**	0.138**	0.144**	0.113**
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Male	0.010**	-0.017**	-0.004+	-0.005**	0.020**
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Age	-0.049**	0.019*	-0.071**	-0.010	0.053**
	(0.005)	(0.008)	(0.007)	(0.007)	(0.006)
Monthly Income	0.154**	0.247**	0.220**	0.211**	0.186**
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Training	0.032**	0.036**	0.026**	0.031**	0.017**
	(0.001)	(0.003)	(0.002)	(0.002)	(0.002)
Value Added per worker	0.014**	0.030**	0.019**	0.017**	0.012**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Fixed Capital per worker	-0.003**	-0.004**	-0.004**	-0.002**	-0.000
	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)
Average Firm Size	0.004**	0.006**	0.003**	0.002**	0.005**
	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
Exporter	-0.002	0.024**	0.016**	0.002	0.010**
	(0.002)	(0.004)	(0.003)	(0.003)	(0.003)
Importer	0.001	0.007+	0.006+	0.009**	-0.018**
	(0.002)	(0.004)	(0.003)	(0.003)	(0.003)
Foreign Owned	0.004**	-0.003	-0.020**	-0.001	-0.025**
	(0.001)	(0.003)	(0.002)	(0.002)	(0.002)
Constant	-0.324**	-0.992**	-0.471**	-0.729**	-0.847**
	(0.018)	(0.033)	(0.030)	(0.029)	(0.026)
Observations	677,726	228,385	259,387	246,819	250,845

Notes: A Poisson pseudo-maximum likelihood estimator is used. The dependent variable is the count of years of employment for each first-time worker from tax year of entry to tax year 2020. Column (1) includes cohort fixed effects. The column titles in the remaining regressions denote the cohort.