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A policy for the jobless youth in South Africa

Individual impacts of the Employment Tax Incentive

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Abstract: This paper uses survey and tax administrative data to analyse the effects of a sizeable employer-borne payroll tax credit for young, low-wage workers in South Africa. We find limited impact of the wage subsidy on employment of young, low-wage workers relative to two comparison groups: slightly older, low-wage workers and slightly higher paid, young workers. We find evidence of increases in entry into employment and decreases in separations of low-wage youth, but these are too small to affect overall employment. However, the female employment rate has increased and unemployment among women has dropped because of the policy. We find evidence to suggest the policy has led to a rise in earnings, particularly for men and those earning around the maximum subsidy value.

Key words: employment, youth, wage subsidy, South Africa

JEL classification: H25, H32, J23, H40

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This study has received ethical approval by the Joint Ethical Review Board of the United Nations University (Ref No: 202104/01) on 11 May 2021.

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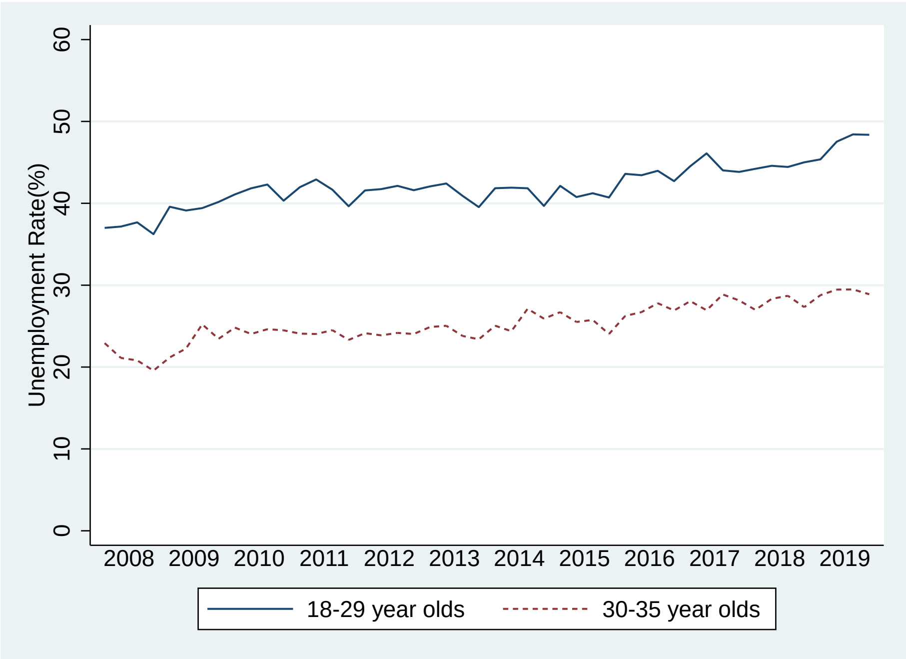
1 Introduction

Direct subsidies or targeted payroll tax cuts to reduce high unemployment rates have become popular in policy discussions. Reducing the tax bill of employers is a policy tool used to reduce the labour costs of workers with low employment probabilities. The policy debate is simple: payroll subsidies boost the labour demand and hence the employment of the targeted group. The conventional view in economics about the efficiency of wage subsidies has been, in contrast, more sceptical.

Under the assumption that labour demand is more elastic than labour supply, the competitive labour market model would predict that the incidence of a payroll subsidy falls on the workers' wages, with no change in the employers' labour costs. However, several recent studies—reviewed below—challenge this result and document significant employment gains from payroll subsidies.

The rationale for a payroll tax subsidy policy in South Africa is clear: the country suffers from very high youth unemployment, and policy makers wanted to alleviate the situation by offering a boost to the labour demand of this group. The unemployment rate for young people is illustrated in Figure 1. The graph shows that the youth unemployment rate has been persistently high, hovering above 40 per cent, for the past ten years. Around 3 million youths in the labour force are without employment. The graph also illustrates that the unemployment rate for slightly older youths is lower, about 30 per cent, but still incredibly high by international standards.

Figure 1: Unemployment rates by age group in South Africa



Note: the unemployment rate is calculated using the narrow definition of unemployment.

Source: authors' calculations based on Quarterly Labour Force Survey (QLFS) data from Statistics South Africa (2021).

In earlier work examining the impacts of wage subsidies, findings from Chile support the traditional view: the incidence of wage subsidies (or payroll tax reductions) falls on the employees, and there are no positive employment effects (Gruber, 1997). However, recent work on Colombia (Kugler and Kugler, 2009), Greece (Saez et al., 2012), France (Cahuc et al., 2019:593), and Sweden

(Saez et al., 2019) indicates the opposite: earnings are not affected, and hence the incidence is (mainly) on employers, opening up a way to positive employment impacts.[1]

The literature is still inconclusive about the optimal design for wage subsidies. According to Cahuc et al. (2019:593): 'Simulations of counterfactual policies show that the effectiveness of the hiring credit relied to a large extent on three features: it was non-anticipated, temporary and targeted at jobs with rigid wages'.

On the other hand, Saez et al. (2019:1760) conclude that:

> Some particular features of the tax cut we study may have enhanced its effectiveness. It was employer borne, salient, administered in a way to ensure near-perfect, immediate and automatic take-up, it targeted young workers but was encompassing (i.e., applied not just to new hires out of unemployment or a subset), it was intended to be permanent, and it was large. (Saez et al., 2019: 1760)

Recent literature surveys also appear to be inconclusive about the effectiveness of wage subsidies. McKenzie (2017:141), reviewing active labour market policies in the developing world, concludes that wage subsidies 'are unlikely to be very effective in generating additional employment under standard labor market conditions'.[2] Kluve et al. (2019), in turn, find positive impacts of youth labour market programmes in their meta-study and that these policies have been more effective in developing countries.

One would also need to know how large-scale employment subsidies work in an emerging economy such as South Africa. It is also vital to examine whether the positive view on the cost-effectiveness of hiring subsidies expressed in Brown (2015) and Brown and Koettl (2015) remains valid in the present case.

We contribute to this new literature by examining the earnings and employment impacts of a large-scale, nationwide wage subsidy programme in South Africa. The policy is implemented in a context where the system's capacity to administer (both in firms and within the administration) may be less efficient than in high-income countries. The sheer size of the unemployment crisis makes it pressing to evaluate the efficiency of the policy.

The Employment Tax Incentive (ETI) policy was initially planned to be temporary and last for three years starting in 2014. It was subsequently extended for another two years and has recently been extended for ten more years, ending in 2029. The intervention is targeted at the employers of young (below 30 years old) and low-wage (earning less than ZAR6,000[3] a month during our analysis period) workers. The incentives vary according to the earnings level of the employee, but they are sizeable at the maximum point (50 per cent of gross earnings). The clear eligibility rules enable us to evaluate its impacts using an augmented difference-in-differences (triple differences) design with two control groups: young, higher-paid workers and older workers at the same earnings level. Since the system is targeted at both low-wage and young workers, we can separate

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[1] The results in Saez et al. (2021) also indicate that the positive employment effects prevailed even after the repeal of the youth payroll tax cut in Sweden.

[2] His review only included three studies on wage subsidies, all relatively small pilot voucher schemes.

[3] At the time of writing, 1 USD is approximately 17 South African Rand (ZAR).

any differential trends that have affected either young or low-wage workers in our triple differences identification strategy.

There are some earlier studies which evaluate the South African ETI policy. Ranchhod and Finn (2015) compare the development of youth and non-youth employment in a difference-in-differences (DD) fashion, but only for the first year after the reform. Ebrahim et al. (2017), Bhorat et al. (2020), and Budlender and Ebrahim (2021) compare ETI-claiming firms with firms that were eligible non-claimers.[4]

While firm-level analysis is certainly worthwhile, the main goal of the policy has been to increase the labour market outcomes of the target group, and this is what our study sets out to examine. There is limited previous knowledge, due to the paucity of data available. about youths who entered jobs supported by the ETI system. Our identification is based on the eligibility of workers. We are mainly interested in the intention-to-treat (ITT) estimates, which identify the programme impacts, including the part that stems from partial take-up. Ours is also the first study to examine the earnings incidence of the policy. We use labour market survey data and the universe of payroll tax data from the South African Revenue Service to examine the impacts of the system.

Our previous study on the ETI (Ebrahim and Pirttilä, 2019) used the Post-Apartheid Labour Market Series (PALMS) version 3.2 data to investigate the incidence of the subsidy by estimating the ITT effect. We found no evidence of any change in employment for the target group but did find evidence of an impact on earnings. This result would point to an incidence on workers with a narrow pathway to an employment impact.

This paper extends the earlier analysis by including an extended period of study of both the survey and tax data. We further our research by examining year-specific impacts and further outcomes on the intensive and extensive margin and refine our estimation methods while retaining the same basic approach.

The paper proceeds as follows. Section 2 presents the policy details. Section 3 presents the data we use and some descriptive statistics. Section 4 presents the empirical strategy. The results are shown in Section 5, and Section 6 concludes.

2 Wage subsidy policy

South Africa's ETI policy has been extended twice. The most recent policy extension has been accompanied by increases in the value of the subsidy and broadens the wage eligibility criteria.[5] The maximum duration of the subsidy is 24 months, which means that the policy is a hybrid between a (short-term) hiring subsidy and a more permanent system.

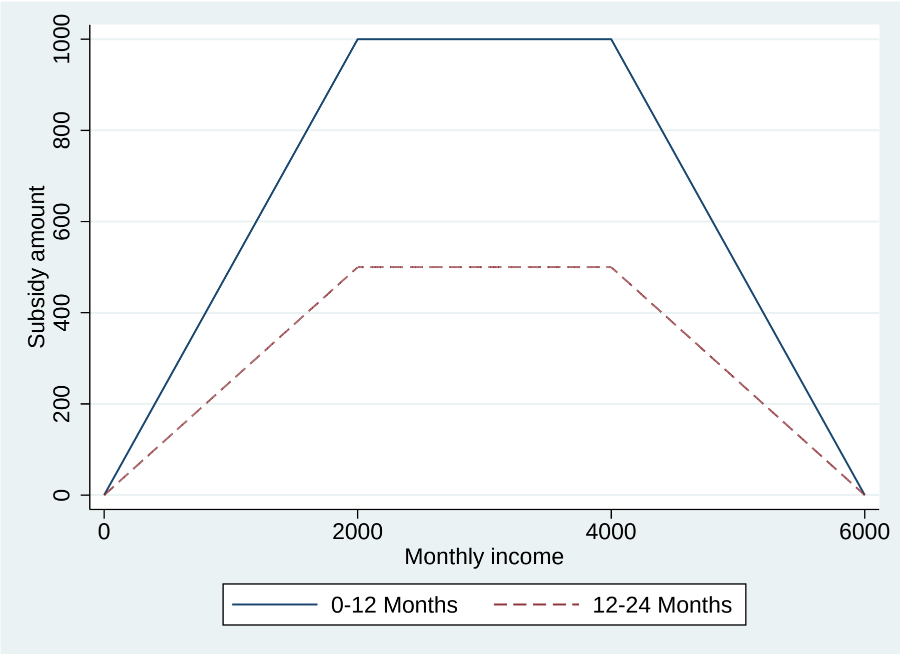
The ETI is an optional tax credit for employers who hire young, low-wage workers. In the first phase (before the amendments in 2019 under examination in this paper), firms could claim the subsidy for workers aged between 18 and 29 years, earning less than ZAR6,000 per month. The subsidy was available to both full-time and part-time workers, but public sector employees were

[4] Saez et al. (2019) use the pre-reform share of young workers to determine treatment intensity. The same strategy is not directly applicable to this policy as the subsidy is available for only new hires. Allocating treatment on the basis of post-reform use of ETI, on the other hand, leads to endogeneity.

[5] Data from the most recent extension is, however, not included in this analysis given the delays in tax reporting

excluded. Unlike similar policies in other countries, the subsidy value follows an unusual phase-in, plateau, and phase-out system. The value of the subsidy compared to the monthly income is depicted in Figure 2.

Figure 2: Monthly subsidy amount



Note: the figure shows the monthly subsidy amount relative to monthly income. The solid line shows the value of the subsidy in the first year of claims, and the dashed line shows the subsidy value in the subsequent year of claims.

Source: authors' own illustration.

The subsidy is phased in for those with a monthly income below ZAR2,000 and calculated as 50 per cent of the monthly income. The maximum subsidy amount is ZAR1,000, paid for earnings between ZAR2,001 and ZAR4,000. The subsidy is phased out in the region of ZAR4,001 to ZAR6,000. The amount of the subsidy is halved during the second year of employment. Firms have no obligation to train employees, and there is no requirement for those hired to be unemployed for a specific period as is seen in similar policies in other countries.

The aim of this hybrid type of subsidy is to incentivize firms to hire workers that would otherwise seem too risky to employ. For the subsidized worker, the period of employment provides work experience that can increase employability to find work again after the subsidized period.

This firm-side subsidy decreases the cost of hiring without changing the income of the subsidized worker. The decrease in cost to firms allows firms to hire additional workers, preferably those in the target group.

The firm-side subsidy fits the South African context as it should stimulate the demand for youth labour where there is already an excess supply. The policy includes a penalty for firms found to release older workers to hire younger workers to claim the subsidy. However, no information is available on the enforcement of the penalty. Still, firms can capture the tax credit without employing more young workers than previously planned.

3 Data and descriptive statistics

In this update, we use the PALMS data version 3.3 (Kerr et al., 2019). This version of the PALMS data gives us six years of the policy period to examine. The survey data provide information about the number of hours worked (not previously studied) and employment and unemployment rates, discussed in the previous working paper version.

We also use the IRP5 payroll tax record data available at the National Treasury Secure Data Facility in Pretoria (more information about the data is shared in Appendix B). We add two additional years of data to our panel from our previous work, giving us data covering the 2011 to 2018 tax years. The tax data are records of the population of formally employed workers and contain detailed information on actual subsidy claims. We provide some descriptive statistics in the subsequent sections before moving on to the empirical approach.

3.1 Survey data

Our sample is restricted to formal private-sector workers when using the PALMS data to make it more comparable to the tax data and to match the policy eligibility criteria. Informal workers are not part of the tax data, but informal sector workers and public servants are not eligible for the subsidy. We restrict the period in the data from 2010 to 2018 to give us the most comparable period between the survey data and the tax data.[6]

We create a low-wage indicator for those with predicted earnings[7] of less than ZAR6,000 per month and restrict the sample to those aged between 18 and 35 years to identify the target population and a comparison group.

Table 1 provides summary statistics for these, and other variables used in the analysis.

[6] While the first year of tax data we examine is 2011, these actually cover the period 1 March–31 December 2010, making the 2010 survey year comparable to the 2011 tax year.

[7] Discussed in further detail in Section 4.

Table 1: Descriptive statistics for estimation sample

Variable	Observations	Mean	Standard deviation	Minimum	Maximum
Employed	687,266	0.255	0.436	0	1
Unemployed	687,266	0.358	0.479	0	1
Hours worked	161,364	44.23	11.23	0	140
Real monthly earnings	131,325	10,714	247,922	0	1.236e+08
Predicted earnings	682,737	3,145	2,522	874.3	37,908
Years of education	682,737	10.68	2.385	0	17
Age	687,266	25.65	5.139	18	35
Female	687,266	0.511	0.500	0	1
Married	687,266	0.212	0.408	0	1
Urban	687,266	0.647	0.478	0	1
Black	687,266	0.827	0.378	0	1
Coloured	687,266	0.0829	0.276	0	1
White	687,266	0.0650	0.247	0	1
Indian	687,266	0.0252	0.157	0	1

Note: the table shows the descriptive statistics for a few variables from the estimation sample of the survey data. Informal and public sector workers are dropped due to subsidy ineligibility.

Source: authors' estimates using PALMS v3.3 (Kerr et al., 2019).

The *employed* variable takes the value one if the individual is classified as employed irrespective of the employment sector. The variable is created from the employment status question in the Quarterly Labour Force Survey (QLFS). Similarly, the *unemployed* variable takes the value of one if the individual is unemployed, defined using the broad definition of unemployment from the employment status question in the QLFS. *Hours worked* refers to the number of hours worked in the last week. The *age* variable allows us to determine whether an individual is eligible for the subsidy or not and enables us to construct a control group from those just above the eligibility age. We also use race, gender, urban or rural location, marital status, and years of education in the analysis.

3.2 Administrative tax data

We have few demographics to report in the tax data due to the lack of this information. We therefore start by examining in more detail the take-up behaviour in the tax data. The take-up rates provide a backdrop for the analysis, as they indicate among which population groups the ITT impacts may be the greatest.

During the first full year of operation, around 30 per cent of eligible youths benefitted from the system. Employers of the youngest workers are more likely to use the subsidy, and the take-up rate among 29-year-olds is, for instance, 12 percentage points less than the average. Table 2 provides a breakdown of ETI take-up by year, gender, and age. It also displays those sectors where the take-up rate has been the highest.

Table 2: ETI take-up characteristics

	ETI eligible	ETI claimed	Take-up
By tax year			
2015	2,692,550	810,834	30 %
2016	2,594,056	1,002,556	38 %
2017	2,468,684	1,101,897	44 %
2018	2,241,741	1,110,552	49 %
By industry			
Wholesale and retail	2,129,276	1,033,152	48 %
Agriculture	1,640,091	772,088	47 %
Catering and accommodation	524,519	220,028	41 %
Finance and insurance	2,185,919	909,073	41 %
Water services	21,397	8,571	40 %
By gender			
Female	4,810,189	1,938,743	40 %
Male	5,726,930	2,224,692	38 %
By age			
18	103,443	44,609	43%
19	368,572	169,196	46%
20	591,857	271,793	46%
21	796,736	356,985	45%
22	967,798	421,122	44%
23	1,087,015	461,971	42%
24	1,145,347	471,593	41%
25	1,144,478	453,438	40%
26	1,115,514	422,390	38%
27	1,070,455	385,531	36%
28	1,022,823	349,216	34%
29	972,197	308,681	32%

Note: the table shows the policy take-up characteristics using the tax data. The years in the tax data refer to the financial year (1 March to 28/29 February), while the survey data relate to the calendar years. Since the 2014 calendar year in the survey data relates to a full policy year, we keep the 2014 calendar year in the survey data analysis. The 2014 calendar year overlaps by ten months with the 2015 tax year.

Source: authors' calculations using IRP5 data (National Treasury and UNU-WIDER, 2019).

The mean take-up rate is somewhat higher among women. It is also much greater among younger workers in the eligible age group and typical low-wage sectors, such as agriculture and retail. Perhaps the most pertinent feature which emerges from the table is that the take-up rate has been steadily increasing over the years. Based on this information, we would expect the impacts to be most prominent among the younger age groups during the latest years. We consider this in the empirical approach by using year fixed effects and analysing subgroups separately.

The two data sources cannot be combined as they are collected and anonymized differently. The use of the two datasets is complementary. Both datasets allow us to examine the effects of the ETI at the extensive and intensive margins. We have three main variables of interest in the survey data: *employed*, *unemployed,* and *hours worked.* We examine *entry* into employment, *exit* from employment, *earnings*, and *job duration* in the tax data.

4 Empirical approach

This research utilizes a triple differences approach (DDD), which uses exogenous or predicted characteristics of individuals to isolate the causal effect of the tax change on employment outcomes. The strength of a DDD over a difference-in-difference (DD) approach is that trends that may differently affect the treatment and control groups in a DD estimator are differenced out in a DDD estimator. The ETI was implemented when the South African economy faced severe challenges in the labour market. It is possible that if employment downturns disproportionally affected young workers, a DD estimator would pick up this development. This would lead to a downwards biased estimate. The DDD estimate is robust to such trends since confounding impacts that only affect low-paid, young workers would bias the estimate. The key assumption in a DD estimator is the common trend assumption: that the treated and control groups evolve in the same way without the policy. The key assumption in the DDD estimator is no additional shock during the treatment period that affects the demand for the treated and control groups.

The ETI is expected to affect employment at the extensive margin (whether an individual works or not) and the intensive margin (the working hours of workers already employed). In addition, it may influence the earnings level of those in work. If it does, and hours of work do not react, the subsidy incidence is (partly) on the worker side.

As in our previous study, the primary approach is to estimate the ITT. We have kept the specification as presented in equation (1).

$$\begin{aligned} y_{i,t} = \alpha + \beta * youth_i + \gamma * low_i + \delta * after_t + \zeta * youth \\ * low_i + \eta * youth * after_{i,t} + \theta * low \\ * after_t + \boldsymbol{\lambda} * \boldsymbol{youth} * \boldsymbol{low} * \boldsymbol{after}_{i,t} + \epsilon_{i,t} \end{aligned} \quad (1)$$

In equation (1), $y_{i,t}$ is the outcome of interest for individual *i* in year *t*. The $youth_i$ variable takes the value one when individual *i* is aged between 18 and 29 years and the value zero when the individual is aged between 30 and 35 years. The low_i variable takes the value one when individual *i* earns less than ZAR6,000 and the value zero when the individual earns between ZAR6,000 and ZAR9,000. Lastly, the variable $after_t$ takes the value one for the years after the reform. Our parameter of interest is λ, the coefficient of the triple interaction between $youth_i$, low_i, and $after_t$.

The triple difference approach allows for two control groups to measure against the eligible group. The first group is the older, low-wage workers. These are workers aged between 30 and 35 years earning less than ZAR6,000 per month. The second control group—young, higher-wage workers—is those aged between 18 and 29 years, with a salary of between ZAR6,000 and ZAR9,000.

Similar to our previous analysis, we predict income based on pre-reform data in the survey data. The income levels are noticeably different between socioeconomic groups, as shown in Table 3. In the PALMS data, the low_i dummy takes a value of one if the predicted income is less than ZAR6,000 and the value zero if the predicted income is higher than ZAR6,000 but less than ZAR9,000.

Table 3: Pre-reform shares of low-wage youths

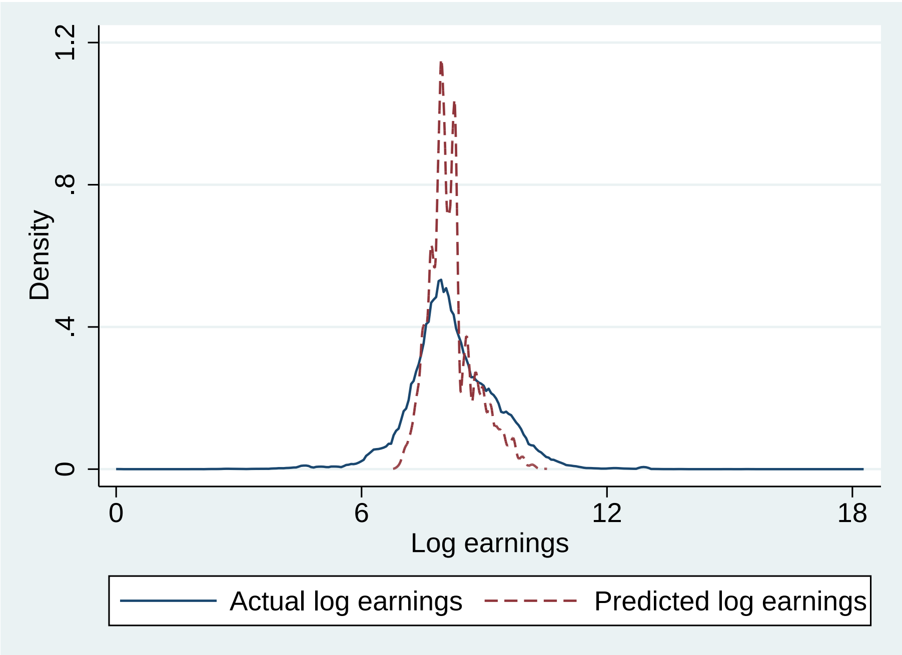
	Africans		Non-Africans	
	Men	Women	Men	Women
Low education	0.82	0.93	0.64	0.80
High education	0.55	0.63	0.24	0.31

Note: sample restricted to those with wages less than ZAR6,000.

Source: authors' own estimates based on the PALMS 3.3 data (Kerr et al., 2019).

We check the predicted earnings against actual earnings in Figure 3. We get around 75 per cent correct predictions. Those who have income are low wage if they are predicted to be low wage and are not low wage if they are not predicted to be low wage.

Figure 3: Predicted versus actual log earnings



Note: earnings density for actual log earnings and predicted log earnings.

Source: authors' illustration using PALMS 3.3 data (Kerr et al., 2019).

The model overpredicts the prevalence of low-wage workers, and the predicted earnings distribution is narrower than the actual earnings distribution.[8] In the tax data analysis, we use earnings as the data only contain those who work, so there are no missing earnings that we would need to predict.

Additionally, we report on an instrumental variable (IV) strategy where eligibility is used as an instrument for the subsidy claim. This is a Wald estimate, where the ITT (our DDD estimate) is multiplied by the inverse of the take-up rate. Hence, the IV can only be statistically significant when the ITT estimate is significant.

We include a specification using year fixed effects as the take-up of the policy increases each year described in equation (2). Equation (2) is a modification of equation (1):

[8] Robustness analysis related to the consequences of potential earnings mispredictions is discussed in Section 5.4.

$$
\begin{aligned}
Y_{i,t} = \beta_0 + \beta_1 youth_i + \beta_2 low_i + \beta_4 youth_i low_i + \sum_{\tau=-4}^{4} \delta_\tau year_t \\
+ \sum_{\tau=-4}^{4} \eta_\tau youth_i year_t + \sum_{\tau=-4}^{4} \theta_\tau low_i year_t \\
+ \sum_{\boldsymbol{\tau=-4}}^{\boldsymbol{4}} \boldsymbol{\lambda_\tau youth_i low_i year_t} + \epsilon_{i,t}
\end{aligned} \tag{2}
$$

No statistical significance for $\boldsymbol{\lambda_\tau}$ in the period $\tau \in [-4, -1]$ supports the assumption of no pre-policy trends.

We include a specification where clustered standard errors are used. The motivation for clustering is the possible correlation in unobserved parts of the outcomes for individuals within clusters (Moulton, 1990; Bertrand et al., 2004). This may be a problem in our analysis, and there is no single accepted way to address this challenge (Cameron and Miller, 2015). However, clustering at the level of treatment would provide too few clusters. Since the extent of the treatment depends on the income level, we cluster at the level of ZAR500 income groups and age (younger and older workers). Results for regressions with clustered standard errors are reported in the following tables.

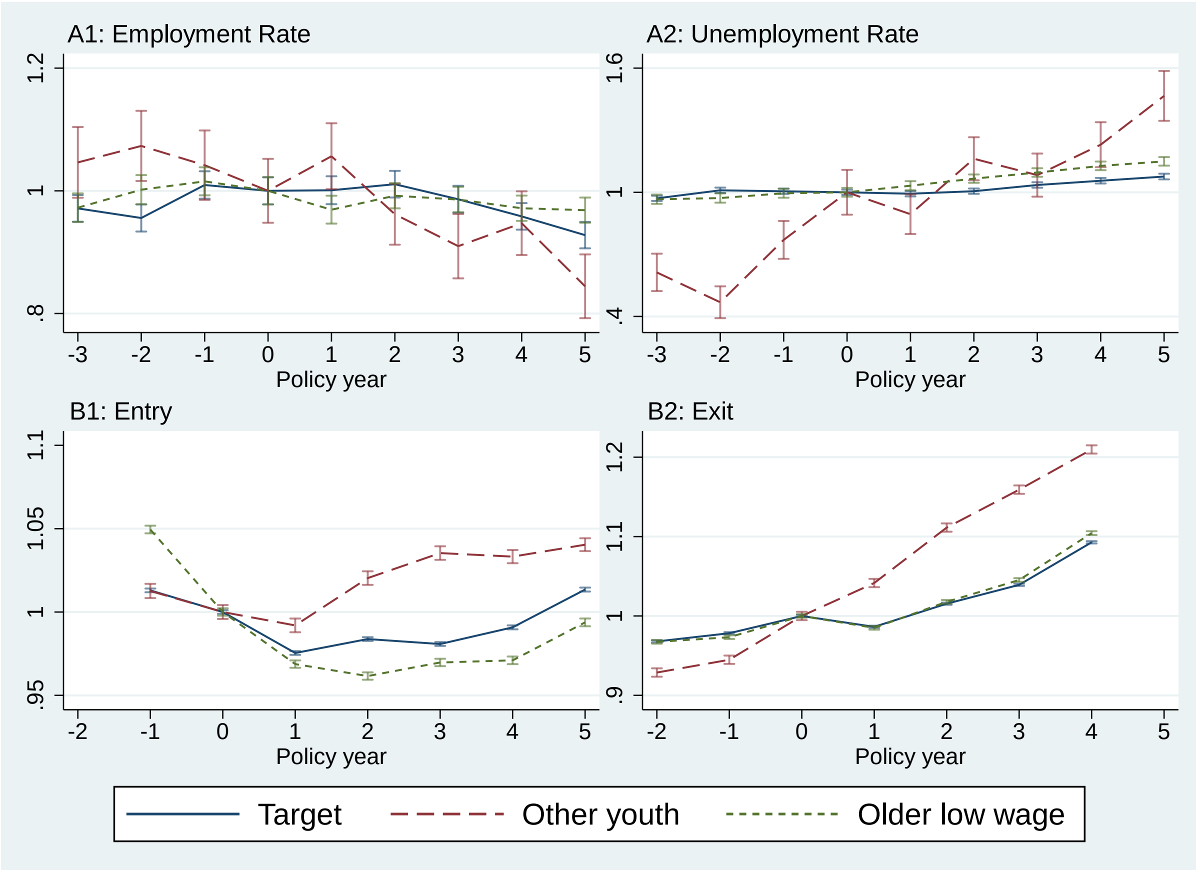
We analyse all eligible workers and various subgroups to examine whether there are any gendered, age, or earnings level effects of the policy. We do this due to the difference in take-up between genders, the difference in unemployment rates of the younger and older groups, and earnings levels in line with the kinks in the ETI subsidy value structure.

5 Results

5.1 Extensive margin

We examine the extensive margin by looking at employment, unemployment, entry, and exit from a job. Figure 4 shows the normalized mean outcomes for the target and control groups. The graphs are normalized in 2013 before the policy started. The graphs compare eligible workers and the two control groups, namely higher-wage youths and older, low-wage workers.

Figure 4: Normalized mean outcomes for target and control groups



Note: the figure shows normalized means for employment, unemployment, entry, and exit for the target and control groups (other youth and older low wage). 'Older low wage' are workers aged 30 and 35 years earning less than ZAR6,000. 'Other youth' are young workers earning between ZAR6,000 and ZAR9,000. Mean outcomes are normalized to unity in 2013 in the survey and tax year to adjust for the level differences across the three groups. The 2018 tax year is excluded from the analysis on exit from employment, and the 2011 tax year is excluded from the entry to employment analysis due to data limitations in these years. The 95 per cent confidence intervals are shown with capped spikes.

Source: authors' estimates based on PALMS v3.3 (Kerr et al., 2019) and IRP5 data (National Treasury and UNU-WIDER, 2019).

Panel A1, which describes the employment rate, suggests no trend break for the employment rates for the target group of low-wage, young workers. There appears to be a minor increase in employment rates in 2015 compared to control groups but decreases after that.

Panel A2, which describes the unemployment rate, suggests that there has been no trend break in the unemployment rate for low-wage, young workers compared to both control groups. While the unemployment rate is increasing during the treatment years, the rate of increase appears to be slower than in both control groups. Whereas it appears that there may be a pre-trend issue with respect to other youth, we note that the other low-wage workers comparison group is larger than the other youth group. Later, in the event study graph in Figure 10, the problem does not arise, as so many of the controls are from other low-wage workers.

Panel B1 appears to be an increase in entry relative to older, low-wage workers suggesting a substitution away from hiring older, low-wage workers who are ineligible for the subsidy.

Panel B2 suggests decreased exit for the target group in comparison to other youth but no change relative to the older, low-wage workers.

Following the graphical evidence, we turn to the DDD estimation to consider the impact of being eligible for the subsidy (or the ITT). The DDD estimation results are presented in Table 4. Column (1) presents the results on employment, column (2) on unemployment, column (3) on entry, and column (4) on exit. The results in panels 1 to 4 correspond to equation (1), the Wald estimate is presented in panel 5 where applicable, and the results corresponding to equation (2) are presented in panel 6.

The basic DDD estimated in panel 1 shows no significant change in employment rates, a small negative significant effect on unemployment rates, a minimal positive significant increase in entry, and an even smaller decrease in exit rates. A decrease in exit rate can be seen as a positive additional effect as retention could provide young workers with much-needed work experience. While the impacts on entry and exit are statistically significant, these effects are perhaps too small to affect employment rates to derive any meaningful economic impact. These results point to a lack in the desired effects on employment of the target group relative to the control groups as intended by the policy.

The negative unemployment effect we see taking place simultaneously with no increase in employment warrants additional discussion. Our employment indicator only includes formal private-sector employment, excluding public and informal workers. Also, a large proportion of youth is outside of the labour force. Because of this, there does not need to be a mechanical link between employment and unemployment. Zooming in to the unemployment rates, the reduction among the target group occurs mainly because of an increase in unemployment for one of the control groups, higher-wage youth (See Figure 4, panel A2). This result is corroborated by regression analyses in which only other low-wage workers are used for comparison. The negative unemployment impact for the target group is then minor in comparison.[9] However, there could still be gender differences in employment outcomes, and these are addressed by Section 5.3.

The subsequent panels consider other specifications as robustness checks. We include firm fixed effects in panel 2. In panel 3, pre-existing differential trends between the treatment and control groups are controlled. Potential differences in pre-trends are removed by estimating the trend from pre-reform data, predicting it for the post-reform years, and subtracting this prediction from actual outcomes (see example in Kleven et al. (2013)). There is no meaningful change in the results compared to the basic DDD in panel 1. Panel 4 uses clustered standard errors, where the clustering is done by income level and age. The standard errors are large with the clustering, and any significant results from panels 1 to 3 are lost in panel 4. Panel 5 accounts for the imperfect take-up by instrumenting the ETI indicator (given actual take-up) by the ITT average ETI eligibility. The result is the estimated impact of the treatment on the treated (TOT) and is only calculated for *entry* and *exit* outcomes from the tax data where we know whether the firms claimed the subsidy. The TOT impacts are expected to be approximately twice as large as the ITT estimates since they are divided by a take-up rate of below 0.5. Panel 6 displays the year-specific treatment effects for each outcome.

There appears to be a significant increase in employment for the eligible group in Year 3 and a significant decrease in unemployment in the same year. These effects correspond to a rise in take-up of the policy shown in Table 2. There are two possible reasons why this may be the case. The first phase of the policy was set to end in 2016 (the third policy year), and firms may have increased their take-up of the policy to ensure they could benefit from it. Late in 2016, the government

[9] These results are not reported but are available from the authors upon request.

announced the extension of the ETI, which may also have spurred a change in behaviour as firms would be able to benefit from the subsidy for a more extended period.

Table 4: DDD estimation on employment, unemployment, entry, and exit

Panel	(1) Employment	(2) Unemployment	(3) Entry	(4) Exit
1. Basic DDD				
	0.0037	-0.0495***	0.0094***	-0.0020**
	(0.0124)	(0.0105)	(0.0010)	(0.0009)
2. DDD with firm fixed effects				
	-	-	0.0137***	-0.0037***
	-	-	(0.0009)	(0.0009)
3. Control for pre-existing trends				
	0.0045	-0.0498***	0.0095***	-0.0019**
	(0.0124)	(0.0105)	(0.0010)	(0.0009)
4. Clustered standard errors				
	0.0045	-0.0498	0.0095	-0.0019
	(0.0277)	(0.0304)	(0.0171)	(0.0152)
5. Control for imperfect take-up (IV)				
	-	-	0.0251***	-0.0053**
	-	-	(0.0025)	(0.0026)
6. Year-specific treatment effects				
x Year 1	-0.0259	0.0253	0.0105***	-0.0041**
	(0.0247)	(0.0215)	(0.0018)	(0.0017)
x Year 2	0.0228	-0.0377*	0.0025	-0.0054***
	(0.0255)	(0.0226)	(0.0018)	(0.0017)
x Year 3	0.0697***	-0.0603***	0.0038**	0.0007
	(0.0257)	(0.0226)	(0.0018)	(0.0017)
x Year 4	-0.0019	-0.0387*	0.0169***	
	(0.0261)	(0.0232)	(0.0017)	
x Year 5	-0.0158	-0.0470*		
	(0.0266)	(0.0241)		
Observations	663,985	663,985	31,564,429	31,770,178
Mean	0.178	0.350	0.510	0.415

Note: the table presents DDD estimation results. Estimates are based on equation (1) in panels 1–4 and equation (2) in panel 6. The table presents estimation results for employment, unemployment, entry into employment, and exit from employment. The 2014 tax year is excluded from the specifications using the tax data (columns (3) and (4) because the policy was enacted in January 2014, at the end of the tax year. Therefore, Year 1 in the tax data represents the 2015 tax year. Panel 1 is the basic DDD estimate. Panel 2 includes firm fixed effects. Panel 3 controls for pre-existing differential trends for the treatment and control groups. Panel 4 uses clustered standard errors where clustering occurs at the level of ZAR500 income groups and age (younger and older workers). Panel 5 controls for imperfect take-up, instrumenting the ETI indicator (given actual take-up) by the ITT average ETI eligibility. Panel 6 presents the year-specific treatment effects estimations. Robust standard errors in parentheses except in panel 3. Controls include gender and age. *** p<0.01, ** p<0.05, * p<0.1.

Source: authors' own estimates using PALMS v3.3 (Kerr et al., 2019) for columns (1) and (2) and IRP5 data (National Treasury and UNU-WIDER, 2019) for columns (3) and (4).

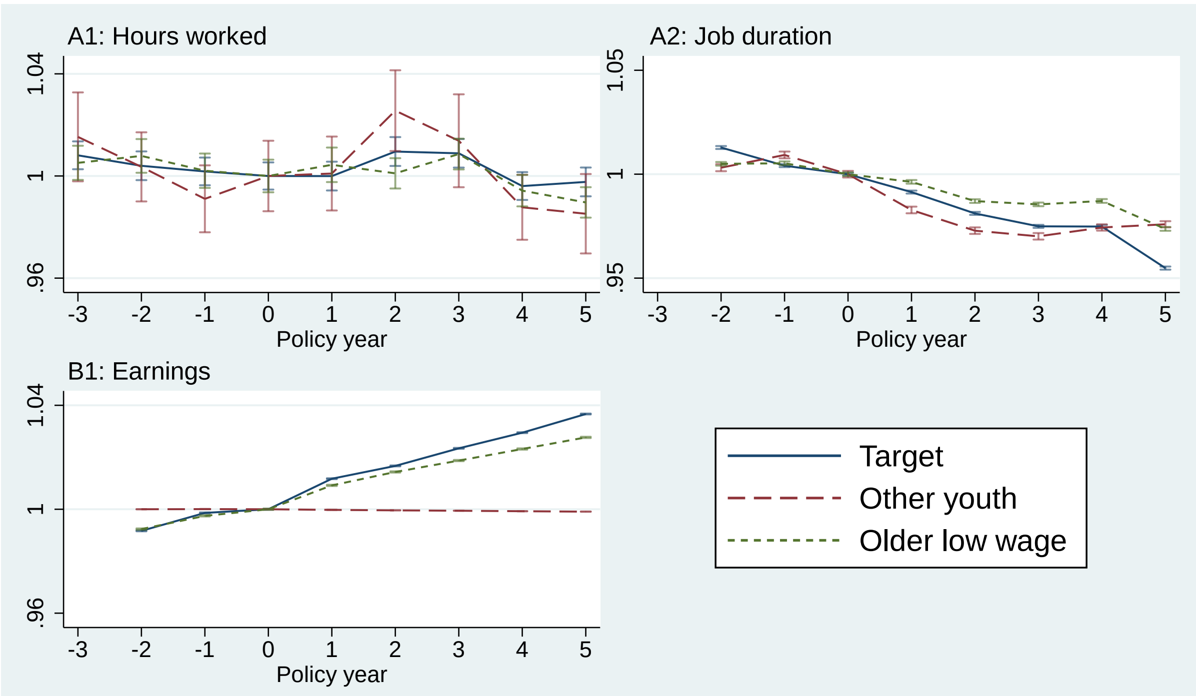
5.2 Intensive margin

Both the survey and tax data permit us to examine the effect of the policy on the intensive margin. We look at three outcomes on the intensive margin: *earnings*, *hours worked*, and *job duration*. Little or no effect on employment outcomes seen on the extensive margin leaves room for an impact on earnings which some of the earlier wage subsidy literature points to.

Since the subsidy is offered to both part-time and full-time workers, firms can respond to the ETI by employing young workers for more hours or more days than they were previously able to. Additionally, the policy design gives firms an incentive to retain workers for longer, or at least for the first 12 months where the subsidy claims are higher. We thus investigate whether there is any change in the number of hours worked or the number of days worked for the target group relative to older, low-wage and young, higher-wage workers.

In Figure 5, we graphically examine the mean outcomes for the intensive margin effects, comparing the outcomes for the target group and the two control groups used in the subsequent DDD analysis.

Figure 5: Normalized mean outcomes for target group and control groups

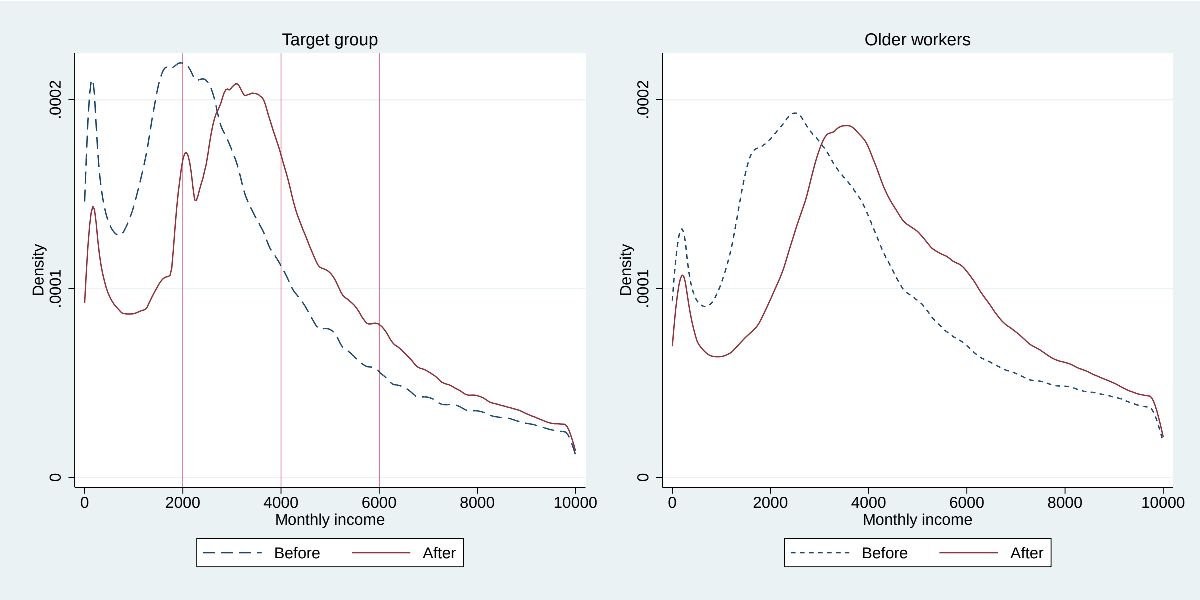


Note: the figures display the mean outcomes for earnings, hours worked, and job duration (in days) for the target and control groups (other youth and older low wage). *Older low wage* is defined as workers aged between 30 and 35 years earning less than ZAR6,000. *Other youth* is defined as young workers earning between ZAR6,000 and ZAR9,000. Mean outcomes are normalized to unity for 2013 (survey and tax year) to adjust for the level differences across the groups. 95 per cent confidence intervals are shown with capped spikes.

Source: authors' own estimates based on PALMS v3.3 (Kerr et al., 2019) for *hours worked* and IRP5 data (National Treasury and UNU-WIDER, 2019) for *earnings* and *job duration*.

We do not observe any trend break for the target group in the number of hours worked in panel A1 or the number of days worked in panel A2. However, we see that the target group experienced a faster increase in earnings after the reform relative to both control groups in panel B1. To investigate this further, Figure 6 depicts the developments in the earnings distribution for young workers between 2013 and 2018. Because of the nominal earnings growth, the distribution has shifted to the right. Early in the system, the general shift was the only marked change, whereas five years into the system, in 2018, a spike in the distribution is visible at the ZAR2,000 level, where the subsidy rate is the greatest. It is likely that employers have gradually learnt more about the system and adjusted to offering jobs according to the incentives created by the system.

Figure 6: Distribution of earnings for young versus older workers

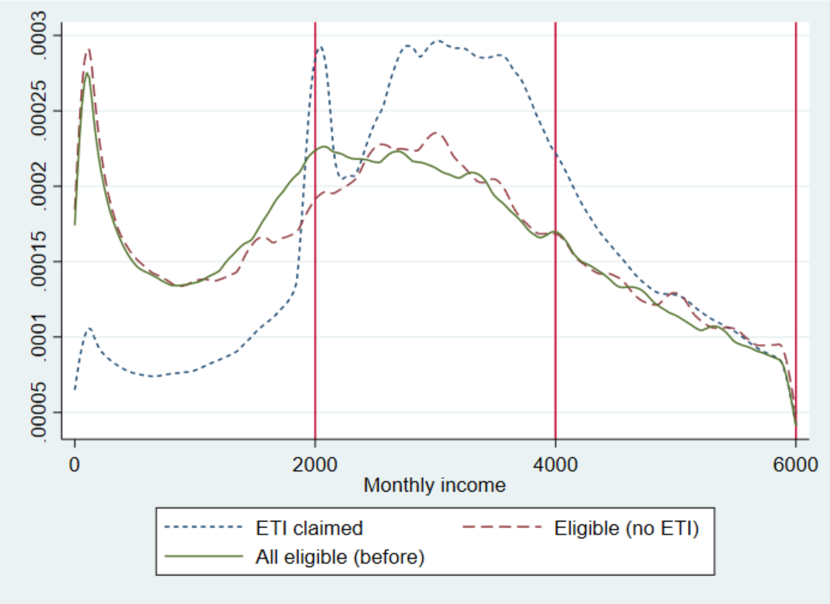


Note: the figure illustrates the earnings distribution for the target group and older workers before the policy and in the fourth year of implementation. The left panel compares the distribution of earnings in 2013 (before) versus 2018 (after) for the target groups of workers. In contrast, the right panel presents the same before–after comparison for older workers aged 30 to 35.

Source: authors' estimates using IRP5 data (National Treasury and UNU-WIDER, 2019).

Since we can establish which employers have used the ETI, we examine the wage distribution by ETI-claiming status in Figure 7. The results confirm that there is now more mass in the wage distribution for ETI-supported jobs, whereas the distribution of workers has not changed for ETI-eligible non-claimers.

Figure 7: Earnings eligible vs ETI claimers

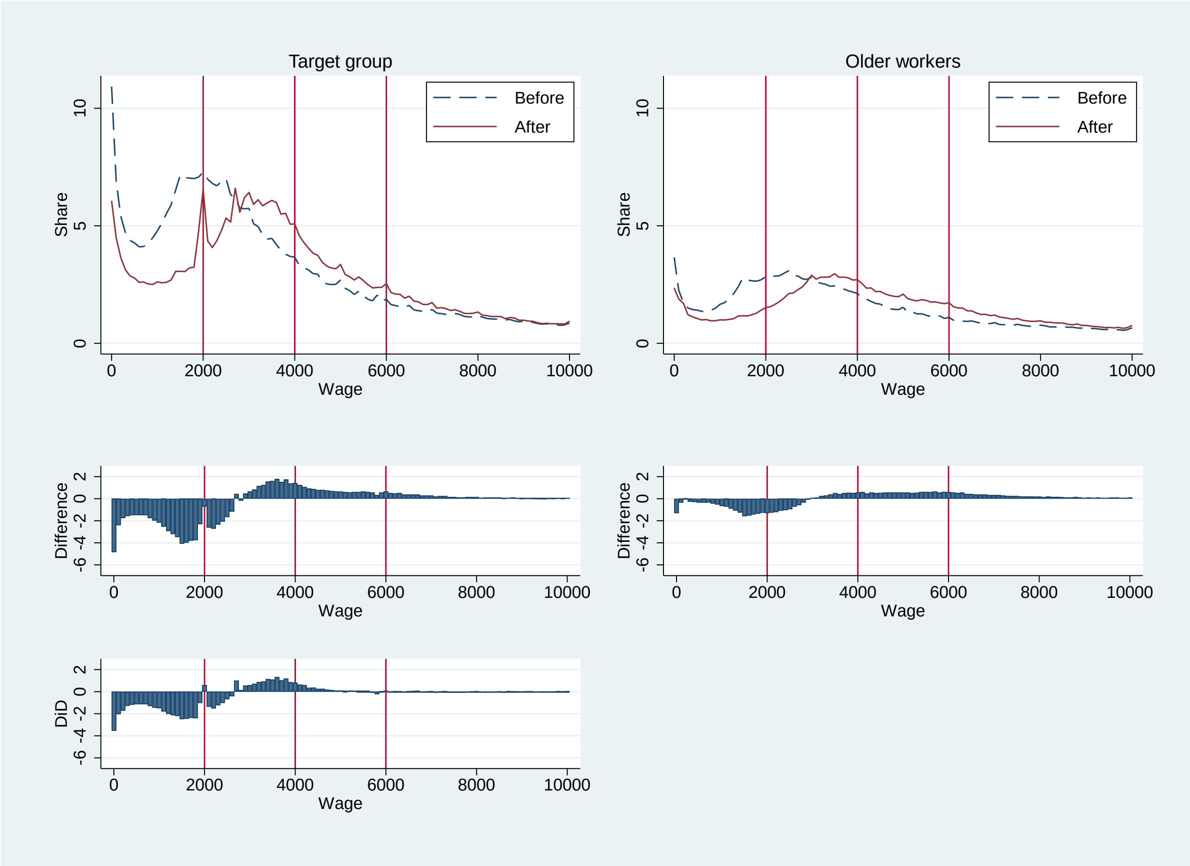


Note: the figure compares the wage distribution by ETI-claiming status, comparing the uprated wages of all those eligible (before the reform, 2013 and before) to those who are eligible but did not claim the subsidy (for 2016 and later) and those who were eligible and for whom the subsidy was claimed (for 2016 and later).

Source: authors' estimates using IRP5 data (National Treasury and UNU-WIDER, 2019).

In Figure 8 we examine the changes in wage distribution in a DD manner. The figure depicts the distribution of wages and changes over time for young and older workers, respectively. There is wage growth for younger and older workers and an initial decrease and then a decrease in wages in the ZAR2,000–ZAR4,000 range. The subsidy to wage percentage is highest (50 per cent) for those who earn ZAR2,000. The bottom panel, which displays the DD, suggests that there has been, in parts, both a negative and positive response in the wages.

Figure 8: Wage distribution for 18–29-year-olds, 2013 and 2018



Note: the figure displays the wage distribution before and after the reform and the calculated differences. The top panel displays the wage distribution before (2013) and after (2018) for the target group and older workers. The second panel shows the before-and-after differences between the two groups. The bottom left panel is the difference in the number of workers in earnings bins between young and older workers, that is, the difference between the middle panel graphs on the left and right. The sample of older workers is restricted to those aged between 30 and 35. Younger workers are limited to the target group aged between 18 and 29. Only wages below ZAR10,000 are included.

Source: authors' estimates using IRP5 data (National Treasury and UNU-WIDER, 2019).

Given that the graphical evidence suggests possible earnings changes, we proceed with DDD estimations to formally test for these changes. The triple difference estimates are reported in Table 5. The estimation results for *hours worked* are presented in column (1), *job duration* in column (2), and *earnings* in column (3).

The results in columns (1) and (2) suggest there is no overall increase in the number of hours worked by the target group and only a small (1.19 day) increase in job duration as a result of the policy. The Wald estimator, or TOT effect, of job duration in panel 5 is nearly double the estimates in panel 1 for job duration. This result means that those in subsidized jobs worked three additional days compared to older, low-wage and young, higher-wage workers.

The basic DDD estimate on log earnings in column (3) shows a small but positive and significant effect. The effect of eligibility on earnings is around 2.5 per cent ($exp(0.0248) \approx 1.025$). In terms of magnitude, there is a ZAR75 increase in wages for those with a monthly income of ZAR3,000. When accounting for the actual take-up, the TOT effect is 6.8 per cent ($exp(0.0655) \approx 1.068$) or ZAR204. Also, the impact on log earnings is more considerable for later years of the policy, which is in line with the increases in take-up and change in behaviour by firms in the later years of the policy. The most considerable effect on wages is seen in 2018, a rise of 5.87 per cent or ZAR176 for those with a monthly salary of ZAR3,000.

Table 5: DDD estimation on hours worked, job duration, and earnings

Panel	(1) Hours worked	(2) Job duration	(3) Log earnings
1. Basic DDD			
	0.3123	1.1907***	0.0248***
	(0.3078)	(0.2399)	(0.000857)
2. DDD with firm fixed effects			
	-	-0.46001**	0.00914***
	-	(0.22474)	(0.00099)
3. Control for pre-existing trends			
	0.3004	1.1483***	0.0247***
	(0.3077)	(0.2399)	(0.000856)
4. Clustered standard errors			
	0.3004	1.1483	0.0247
	(0.4670)	(5.4166)	(0.0627)
5. Control for imperfect take-up (IV)			
	-	3.1278***	0.0655***
	-	(0.6350)	(0.0023)
6. Year-specific treatment effects			
x Year 1	-0.5636	-0.2801	0.0101***
	(0.6199)	(0.4791)	(0.00172)
x Year 2	-0.7023	-0.5615	0.0262***
	(0.6654)	(0.4756)	(0.00170)
x Year 3	-1.0916	-0.1976	0.0348***
	(0.6705)	(0.4711)	(0.00172)
x Year 4	0.1125	-1.7161***	0.0570***
	(0.6554)	(0.4658)	(0.00171)
x Year 5	0.5578		
	(0.6859)		
Observations	143,311	36,098,402	36,098,402
Mean	44.5	219.26	7.44

Note: the table presents DDD estimation results. Estimates are based on equation (1) in panels 1–5 and equation (2) in panel 6. The table presents estimation results for the number of hours worked, job duration measured by the number of days worked, and the log of earnings. The 2014 tax year is excluded from the specifications using the tax data because the policy was enacted in January 2014 at the end of the tax year. Panel 1 is the basic DDD estimate. Panel 2 includes firm fixed effects. Panel 3 controls for pre-existing differential trends for the treatment and control groups. Panel 4 uses clustered standard errors where clustering occurs at the level of ZAR500 income groups and age (younger and older workers). Panel 5 controls for imperfect take-up, instrumenting the ETI indicator (given actual take-up) by the ITT average ETI eligibility. Panel 6 presents the year-specific treatment effects estimations. Robust standard errors in parentheses except in panel 4. Controls include gender and age. *** p<0.01, ** p<0.05, * p<0.1.

Source: authors' own estimates using PALMS v3.3 (column 1) and IRP5 data (National Treasury and UNU-WIDER, 2019) for columns (2) and (3)

Considering how the subsidy is designed, some of the switching between positive and negative results, or significance and non-significance in different specifications, could be driven by heterogeneous effects within the target group based on the value of the subsidy received (or monthly income on which the subsidy is based). These differences are the subject of the following subsection.

5.3 Heterogeneity

The analysis of subgroups is motivated by the ETI take-up characteristics presented in Table 2 and the kinks in the policy design. First, any differences in outcomes between men and women in the target group are considered. Next, younger and older cohorts are measured within the target group. This cohort analysis is motivated by the differences in unemployment rates for these two groups: the unemployment rate for those aged between 18 and 24 is 58 per cent at the end of 2019. In contrast, the unemployment rate for those aged between 25 and 30 is 42 per cent (Statistics South Africa, 2021).

The DDD regression results for subgroups are presented in Table 6. Columns (1) to (3) show the impact on the employment rates, and columns (4) to (6) show the effect on the unemployment rates. We report estimates for all individuals separately for women and men for the entire target group and then separately for two age groups (18 to 24 and 25 to 29 years). Figures 9 and 10 show the event study graphs for these same subgroups for employment and unemployment respectively.

The results highlight that while there is no positive employment effect for the entire sample, a positive effect emerges for women and, for them, the decrease in unemployment is also stronger. Hence the overall zero impact masks some interesting heterogeneity across men and women. The positive extensive margin results for women also appear robust (see Section 5.3).

For men, the employment impact is even negative, with a larger impact on younger men. Younger men appear to have the largest decrease in employment.[10]

Table 6: The effect on employment and unemployment, by subgroup

	Employment			Unemployment		
	All	Women	Men	All	Women	Men
	(1)	(2)	(3)	(4)	(5)	(6)
All target group	0.0037	0.0405**	-0.0339**	-0.0495***	-0.0788***	-0.0222
	(0.0124)	(0.0185)	(0.0167)	(0.0105)	(0.0156)	(0.0149)
Age 18–24	-0.0452***	0.0171	-0.0828***	-0.0431***	-0.0622**	-0.0054
	(0.0152)	(0.0278)	(0.0190)	(0.0131)	(0.0266)	(0.0167)
Age 25–29	0.0500***	0.0372*	0.0480**	-0.0425***	-0.0427***	-0.0371**
	(0.0143)	(0.0201)	(0.0198)	(0.0120)	(0.0162)	(0.0179)

Note: basic DDD estimates presented. There are 687,266 observations in the full target group. The first row in the table reports the triple difference coefficient of the full target group (18–29 years old). The next row reports the triple difference coefficient where the sample is restricted to the younger individuals in the target group (18–24 years old). The last row reports the triple difference coefficient where the sample is restricted to the older individuals in the target group (25–29 years old). The first column in the table includes both men and women, the second column is restricted to women only, and the third column includes only men. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Source: authors' estimates using PALMS v3.3 (Kerr et al., 2019).

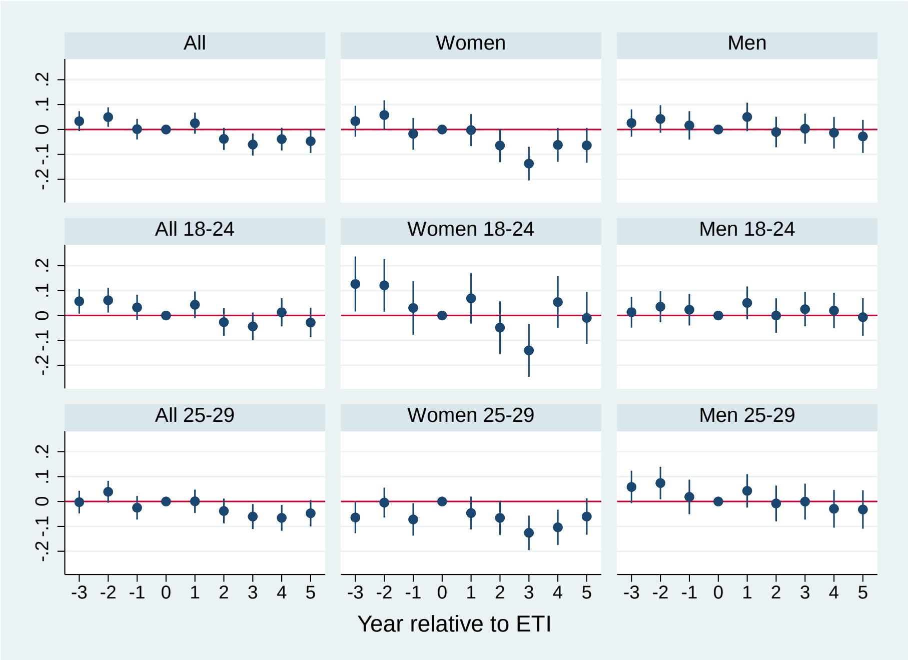
[10] The finding of a negative employment effect on men is not statistically significant if individual-level controls are added to the analysis.

Figure 9: Employment, by subgroup

Note: the first column includes both men and women, the second column only women, and the third column only men. The top row includes everyone aged 18 to 29, the second row includes those aged 18 to 24, and the bottom row includes those aged 25 to 29 years.

Source: authors' estimates using PALMS v3.3 (Kerr et al., 2019).

Figure 10: Unemployment, by subgroup

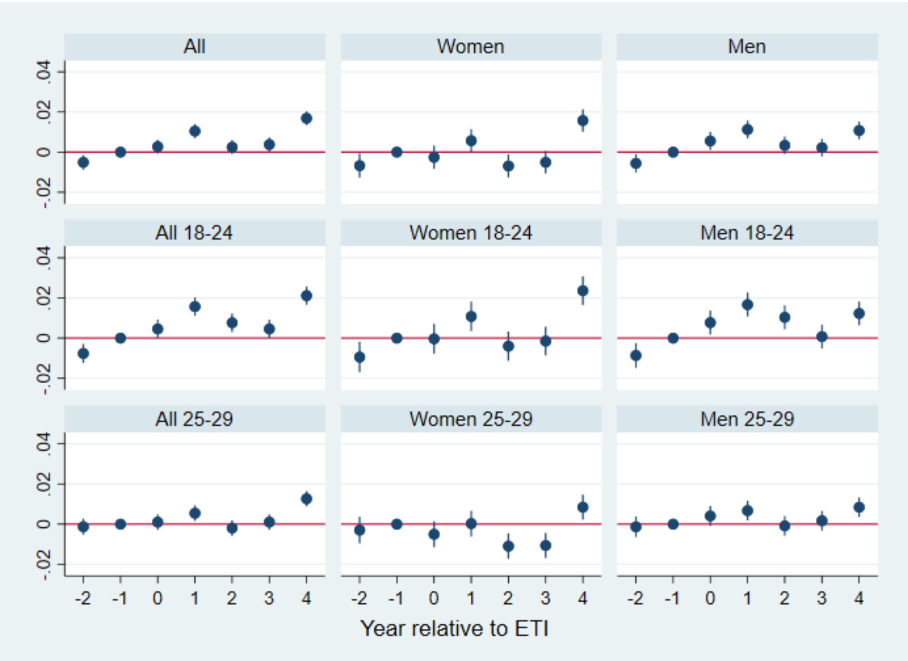


Note: the first column includes both men and women, the second column only women, and the third column only men. The top row includes everyone aged 18 to 29, the second row includes those aged 18 to 24, and the bottom row includes those aged 25 to 29 years .

Source: authors' estimates using PALMS v3.3 (Kerr et al., 2019).

We display the impact on age and wage groups for entry into employment in Figure 11. The graphs show the coefficient plots for the year-specific treatment effects.[11] The effect on entry appears stronger for the younger group, more pronounced for women in 2018, and more pronounced for men in 2015. We examine the exit from employment and job duration for these same subgroups with the graphs available in the appendix (Figures A1 to A3). No heterogeneous effects are observed for exit and job duration.

Figure 11: Impact of ETI on entry, by subgroup



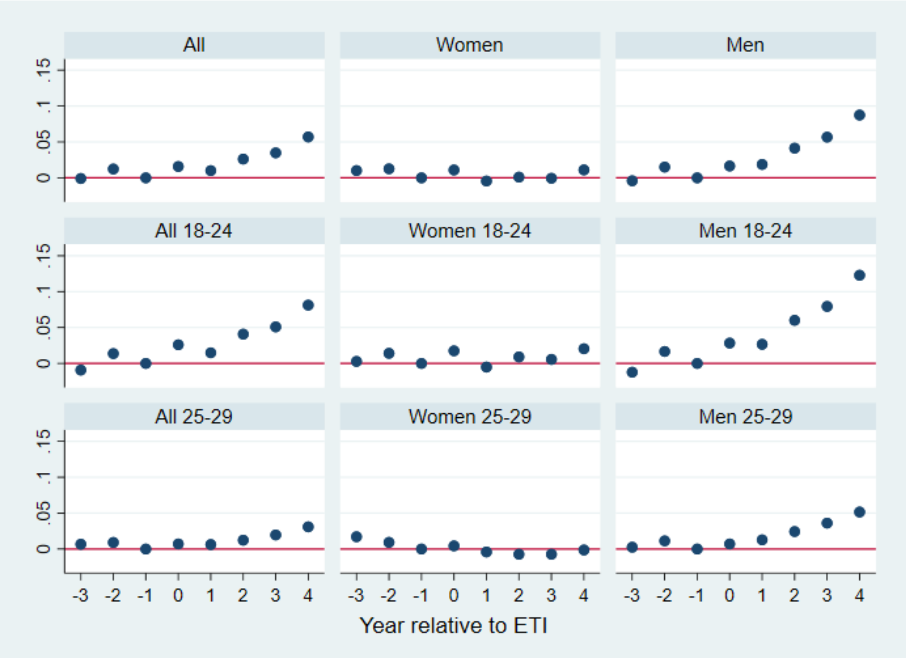
Note: the first column includes both men and women, the second column only women, and the third column only men. The top row includes everyone aged 18 to 29, the second row includes those aged 18 to 24, and the bottom row includes those aged 25 to 29 years.

Source: authors' own estimates using IRP5 data (National Treasury and UNU-WIDER, 2019).

Figure 12, in turn, depicts the impact on earnings by subgroup. Interestingly, the positive earnings result for the entire sample is driven by the sizeable increase in the earnings level for men, whereas there is no earnings improvement for women. This is a mirror image of the employment response, which was positive for women and even negative for men. These results are also in line with the idea that that an increase in earnings—incidence on workers—limits the employment gains of wage subsidy policies. From the PALMS data, we can see that men and women work in different industries and this may be one reason why results differ. When including industry controls in the tax data, the positive significant result on earnings still holds for men.

[11] The first graph, All, is the coefficient plot for column (3) of panel 4 in Table 7.

Figure 12: Impact of the ETI on log earnings, by subgroup



Note: the first column includes both men and women, the second column only women, and the third column only men. The top row contains everyone aged 18 to 29, the second row includes those aged 18 to 24, and the bottom row includes those aged 25 to 29 years.

Source: authors' own estimates using IRP5 data (National Treasury and UNU-WIDER, 2019).

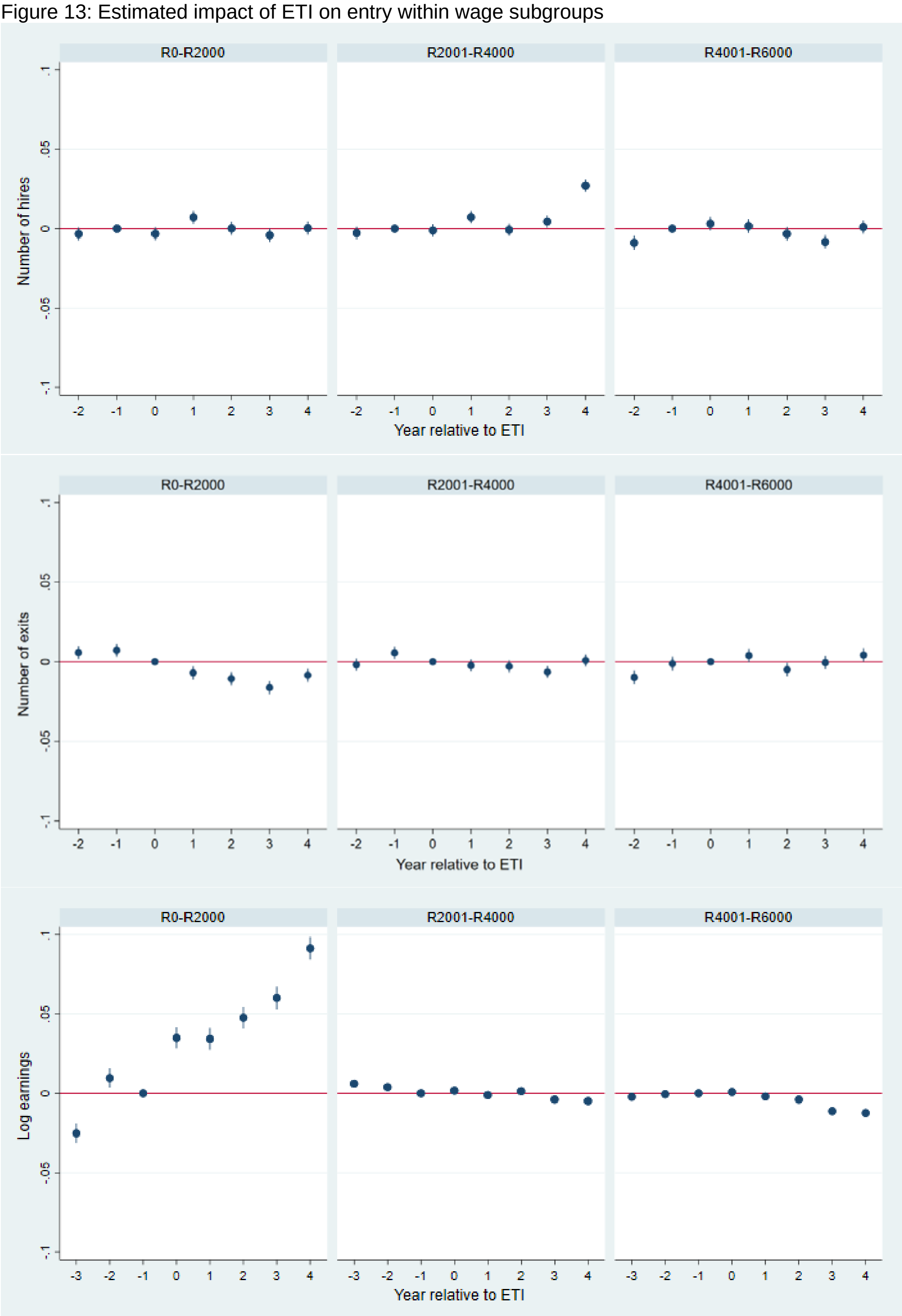
Heterogeneity by income group

We consider the heterogeneity in our results by income group as the policy has kink points at the ZAR2,000 and ZAR4,000 marks. The kink points in the value of the subsidy and the sharp bunching we see in Figure 8 suggest heterogeneous effects. There is an incentive for firms to increase the number of jobs or incomes around the ZAR2,000 subsidy point where the firm would claim the highest value of the claim.

On the extensive margin, we look at the effects on entry and exit as we know the income reported from the tax records. We use the event study type graphs in Figure 13 to present the results for income subgroups. The positive outcome on entry into employment from Table 4 appears to be driven by the ZAR2,001–ZAR4,000 income group, and the ZAR0–ZAR2,000 group drives the decrease in the exit from employment.

On the intensive margin, we examine subgroups for log earnings. The graphs suggest that the ETI leads to a 10 per cent increase in earnings for those that earn less than ZAR2,000 per month shown in the first panel. Since the subgroups are divided by earnings, we recognize that this would limit any earnings increase beyond the upper bound of each group. The takeaway point, however, is that the overall increase in earnings result may be driven by the increases for the very low earners, among whom earning exactly ZAR2,000 is now more common.

Figure 13: Estimated impact of ETI on entry within wage subgroups



Note: the graphs present the coefficients of the DDD estimation results where the dependent variable is the number of hires, number of exits, and log earnings. The x-axis is the tax year relative to the ETI. That is, tax year 2014 is zero. The 95 per cent confidence intervals are displayed.

Source: authors' own estimates using IRP5 data (National Treasury and UNU-WIDER, 2019).

Industries with high take-up

In Table 2 we described the distribution of take-up among industries and found higher take-up in some industries. Some countries have limited their wage subsidy policies to certain industries where it is believed that a wage subsidy will have the largest impact. There were similar recommendations to target the ETI policy to labour-intensive industries where job creation is more likely. The motivation behind this is also to limit any possible deadweight losses arising from firms claiming the subsidy without creating any new jobs. In this subsection, we limit our analysis to those industries with greater than average take-up of the ETI. The results for all outcomes examined in the tax data are displayed in Table 7.

We make comparisons with Table 4 and Table 5, reflecting the full population. For industries with a higher than average take-up rate, the effect on entry is smaller but still significant and all significance is lost for the effect on exits. The effect on earnings in the high take-up industries is only slightly greater in comparison to the full sample. The interesting point from the table is the large increases in job duration seen in the high take-up industries. The TOT effect on job duration in panel 3, when accounting for the actual take-up, is more than double the estimates in panel 1 for job duration. This means that those who were in subsidized jobs worked 13 additional days in comparison to older, low-wage and young, higher-wage workers in industries where the take-up was above average. The results suggest that job growth is not likely in industries with high subsidy take-up and it may not be worthwhile for the policy to be targeted to these specific industries.

Table 7: DDD estimation for industries with high take-up

Panel	Extensive margin		Intensive margin	
	(1)	(2)	(3)	(4)
	Entry	Exit	Log earnings	Job duration
1. Basic DDD				
	0.0077***	0.0006	0.0278***	5.874***
	(0.0015)	(0.0015)	(0.0012)	(0.389)
2. DDD with firm fixed effects				
	0.0131***	0.0021	0.0056***	0.4952
	(0.0014)	(0.0014)	(0.0016)	(0.3545)
3. Control for pre-existing trends				
	0.0082***	0.0007	0.0286***	5.673***
	(0.0015)	(0.0015)	(0.0012)	(0.390)
4. Clustered standard errors				
	0.0082	0.0007	0.0286	5.874
	(0.0214)	(0.0221)	(0.0878)	(7.235)
5. Control for imperfect take-up (IV)				
	0.0186***	0.0029	0.0667***	13.573***
	(0.0035)	(0.0037)	(0.0027)	(0.900)
6. Year-specific treatment effects				
x 2015	0.0103***	-0.0033	-0.0099***	1.945**
	(0.0029)	(0.0028)	(0.0024)	(0.783)
x 2016	0.0001	0.0021	0.0264***	1.810**
	(0.0028)	(0.0028)	(0.0023)	(0.764)
x 2017	0.0013	0.0103***	0.0211***	2.638***
	(0.0027)	(0.0027)	(0.0023)	(0.746)
x 2018	0.0189***	-	0.0554***	-0.195
	(0.0027)		(0.0023)	(0.737)
Observations	17 842 796	17 800 874	20 302 379	20 302 379
Mean	0.66	0.56	7.97	206.77

Note: estimates are based on equation (1) in panels 1–5 and equation (2) in panel 6. The table presents estimation results for log earnings, entry into employment, exit from employment, and job duration for industries with a high take-up rate. The 2014 tax year is excluded from the specifications because the policy was enacted in 2014 at the end of the tax year. Panel 1 is the basic DDD estimate. Panel 2 includes firm fixed effects. Panel 3 controls for differential pre-existing trends for the treatment and control groups. Panel 4 uses clustered standard errors where clustering takes place at the level of ZARR500 income groups and age (younger and older workers). Panel 5 controls for imperfect take-up, instrumenting the ETI indicator (given actual take-up) by the ITT average ETI eligibility. Panel 6 presents the year-specific treatment effects estimations. Robust standard errors in parentheses except in panel 4. Controls include gender and age. *** p<0.01, ** p<0.05, * p<0.1.

Source: authors' own estimates using IRP5 data (National Treasury and UNU-WIDER, 2019).

6 Conclusion

In this paper we set out to examine the individual-level effects of the ETI, in particular the earnings and employment impacts of the ETI. The subsidy has been available for the employers of workers aged below 30 years earning at most ZAR6,000 a month. Because of the targeted nature of the policy, we utilized a triple difference estimation strategy, with both older workers in the same earnings range and young workers slightly above the wage criteria as our two control groups. Our analysis relied on survey and administrative tax data to examine outcomes on the extensive and intensive margins.

The analysis reveals that the policy has neither increased the overall employment nor led to large reductions in the unemployment for the eligible group as a whole. However, the employment outcomes appear to be more favourable for women, whose employment rate increased after the introduction of the policy. We also document some increases in hiring and decreases in the number of exits but the magnitudes are too low to meaningfully affect the employment rate.

On the intensive margin, we find that the policy led to a moderate statistically significant increase in the earnings for the target group. This result is completely driven by increased earnings for the eligible men, whereas for women there was no earnings gain. Hence the incidence of the subsidy also has an interesting gender difference. The pattern in the result—greater earnings but no employment increase for men and no income gains but positive employment impact for women—is in line with the idea that employment gains are limited by earnings increases.

Lastly, while we find no increase in the number of hours worked in the survey data, we find a statistically significant increase in the job duration for the eligible group, with a larger impact seen in high take-up industries.

The results we derive are at odds with the most recent influential work on the employment impacts of wage subsidies, including Cahuc et al. (2019) and Saez et al. (2019). We find very limited overall employment impacts and our results also indicate that the incidence of the system was partly on workers. The results also point to novel gender differences in the key labour market outcomes.

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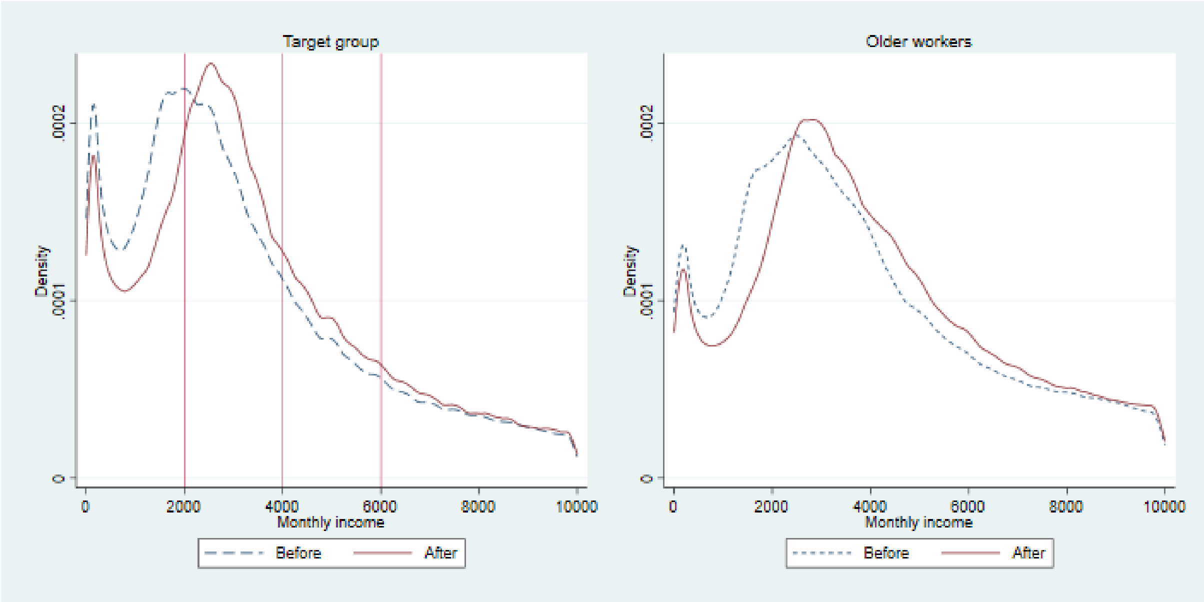
Appendix A

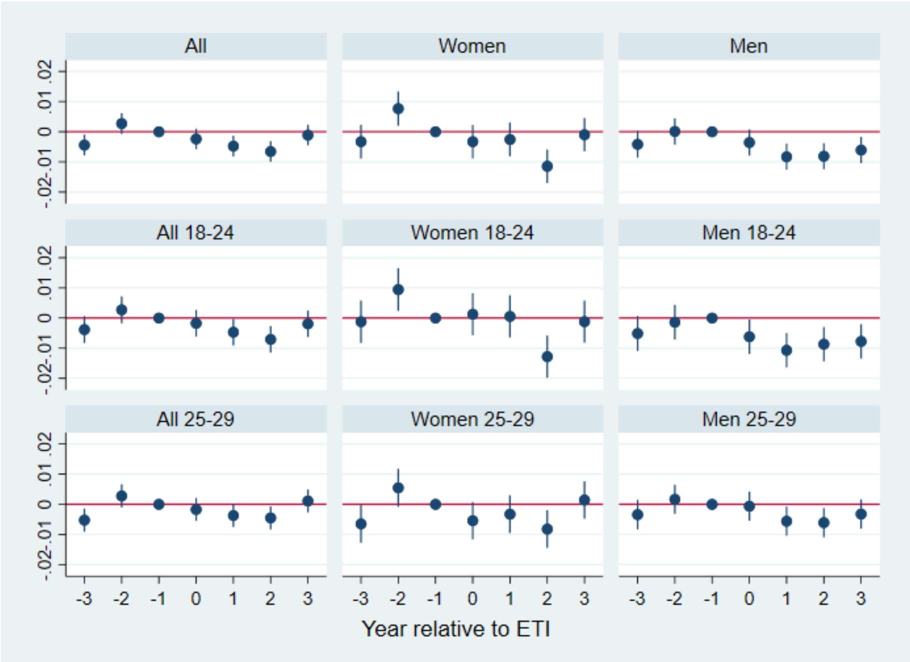
Figure A1: Distribution of earnings for workers for young versus older workers



Note: the left panel compares the distribution of earnings in 2015 versus 2013 for the target groups of worker, whereas the right panel presents the same comparison between 2015 and 2013 for low-wage workers aged 30 to 35 years.

Source: authors' own estimates using IRP5 data (National Treasury and UNU-WIDER, 2019).

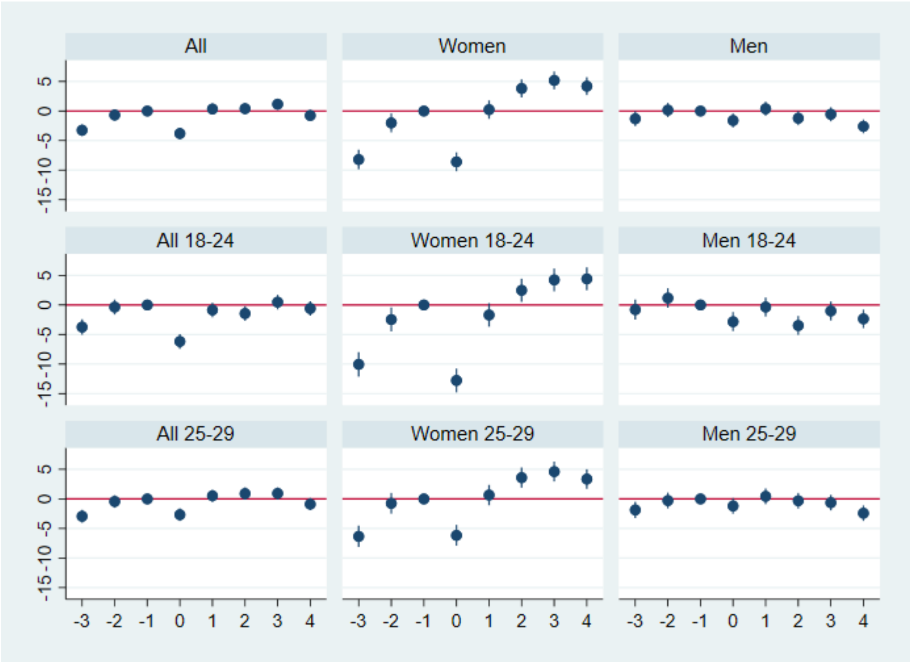
Figure A2: Impact of ETI on exit, by subgroup



Note: the first column includes both men and women, the second column only women, and the third column only men. The top row includes everyone aged 18 to 29, the second row includes those aged 18 to 24, and the bottom row includes those aged 25 to 29 years.

Source: authors' own estimates using IRP5 data (National Treasury and UNU-WIDER, 2019).

Figure A3: Impact of ETI on job duration, by subgroup



Note: the first column includes both men and women, the second column only women, and the third column only men. The top row includes everyone aged 18 to 29, the second row includes those aged 18 to 24, and the bottom row includes those aged 25 to 29 years.

Source: authors' own estimates using IRP5 data (National Treasury and UNU-WIDER, 2019).

Appendix B

The anonymized tax data used in this paper are restricted use access at the National Treasury Secure Data Facility in Pretoria. The tax data was accessed under a non-disclosure agreement and oath of secrecy between the period February 2018 through November 2022. Each output was checked to maintain the anonymity of individuals and firms in the tax data. The results do not represent any official statistics (of the National Treasury or the South African Revenue Services). The views expressed in our research do not necessarily represent the views of the National Treasury or SARS.

Data used come from the 2019_1 version of the Employment Panel which is a sub-dataset of the Individual Panel listed in the references.

Our analysis was conducted using Stata 17. For our analysis of the Wald estimate we use the `ivreg2` Stata module for extended instrumental variables regression (Baum et al., 2010).

A full description of the cleaning procedures can be found Chapter 3 in Ebrahim (2021).