Failed Promises of a Wage Subsidy: Youth and South Africa's Employment Tax Incentive *

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Abstract

Young people face unemployment rates two to four times those of older workers. Youth unemployment is particularly severe in South Africa, where 52 percent of those between ages 15 and 24 are unemployed. To address this issue, South Africa has introduced a subsidy that offsets firms' costs for wages paid to young workers hired after October 2013. Firms can receive tax credits that cover up to half the wages paid to a young worker. While similar wage subsidies have been used in other middle-income countries, there is little evidence about their efficacy, especially when implemented at a national scale. I estimate the policy's effect using a difference-in-differences strategy based on age-eligibility restrictions and the start date. During the first year of implementation, being eligible for the subsidy did not increase employment for age-eligible workers relative to a slightly older cohort. My estimates rule out effects as small as 1 percentage point. This limited result comes despite widespread utilization of the subsidy, which was extended twice past its expiration date. As the decrease in firms' costs did not induce them to hire more young workers, attention should be focused on other constraints that prevent such workers from being employed, such as their skills or labor market regulations.

Keywords: Active Labor Market Policies, Wage Subsidies, South Africa.

JEL Classification: J08, J68, O15

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1 Introduction

Youth unemployment is a global challenge. Around the world, young workers face unemployment rates two to four times those of the overall population. They struggle to transition into the labor market, even after investing in education to increase their chances of integration.[1] Addressing this challenge is a particularly important policy question for developing countries, given their relatively young populations and how the issue's significance can be important for economic growth.[2] High youth unemployment rates mean that a large share of the active population is not contributing to the growth of the economy or accumulating the experience that would in turn increase these workers' productivity. Human capital accumulated through schooling could also be depleted through long spells of unemployment. Finally, unemployment can produce negative externalities such as violence, crime, and social unrest, due to the frustration of those who are not able to get a job, conditions that further deter investment by businesses.[3] To address the issue, policymakers have considered interventions such as active labor market policies.[4] Economists have regained interest in the effectiveness of these policies due to their widespread use to mitigate the 2008 global recession; however, not enough is known about how these interventions affect employment, especially wage subsidies in developing countries (Card et al., 2015; Kluve et al., 2017; McKenzie, 2017).[5]

South Africa has the highest youth unemployment rate in the world. In the first quarter of 2018, 52% of South Africans aged 15 to 24 years were unemployed.[6] The youth unemployment rate is in part a consequence of the overall high unemployment rate in the country; in the first quarter of 2018, South Africa's unemployment rate was 27%.[7] South Africa's labor

[1] Filmer and Fox (2014) note that transition into the labor market is a multifaceted issue, with some young individuals experiencing periods of economic activity while in school, many others transitioning via their parents' or relatives' business and farms, and some making it into the formal sector after experiencing long spells of unemployment.

[2] In 2014, the median individual was 18 years old in Africa, 7 years younger than in South Asia, the second-youngest region. The population in Africa is projected to stay young, with a projected median age of 25 in 2050. (Filmer and Fox, 2014).

[3] See Cramer (2010) for a discussion of the literature on youth unemployment and violence, with a focus on developing countries. Blattman and Ralston (2015) also discuss employment and social stability, emphasizing the role of employment programs.

[4] See the literature review for a discussion of active labor market policies.

[5] In the aftermath of the 2008 crisis, a report by the International Labor Organization and the World Bank revealed that almost all countries took some counter-cyclical measures, including 23 countries that had some wage subsidies, primarily in terms of reducing social contributions by employers. See `http://ilo.org/wcmsp5/groups/public/---ed_emp/---emp_elm/documents/publication/wcms_186324.pdf`

[6] The situation is particularly problematic given the size of South Africa's population. Based on World Bank statistics, South Africa has a population twice as large as any other country with a youth unemployment rate above 40 percent.

[7] Data are from the Quarterly Labour Force Survey Statistical release for the $2018Q1$: `http://www.statssa.gov.za/publications/P0211/P02111stQuarter2018.pdf`

market in that regard remains an outlier, both among countries with the same level of development (especially Latin American countries) and among Sub-Saharan African countries.[8] Over the two decades following the end of apartheid in 1994, unemployment has continuously risen, except for a short period of strong growth in the 2000s that ended with the 2008 global recession. The situation is particularly severe for Black South Africans (30% of them are unemployed versus 11% of White South Africans), which suggests that inequality in the labor market is a major contributor to income inequality in the country.[9]

South Africa has implemented one policy known as the Employment Tax Incentive (ETI) to address its youth unemployment issue. It is a wage subsidy, initially proposed by Levinsohn (2007), which offers tax reductions to firms that hire young workers. The government adopted the proposal and signed it into law in December 2013. The wage subsidy was phased in as a pilot program until 2016 and later extended due to higher than expected take-up by firms (National Treasury, 2016), although the extension was supposed to be conditional on effectiveness. To date, a few papers have attempted to analyze the ETI's impact on young workers' labor market outcomes (Ranchhod and Finn, 2016; Ebrahim et al., 2017; Moeletsi, 2017), but they have yet to produce conclusive evidence of the ETI's causal impact. In this paper, I investigate the extent to which the Employment Tax Incentive affected labor market outcomes of young workers.

By analyzing the ETI, I contribute to the literature on the impact of wage subsidies on youth employment in developing countries and provide one of only a handful evaluations of policies implemented at a national level. Similar policies have been evaluated in the literature, including the "Stage d'Initiation à la Vie Professionelle" (Initiation into the World of Work - SIVP) in Tunisia (Broecke, 2013), the post-2008 Employment Subsidy Program in Turkey (Balkan et al., 2016), the Youth Employment Subsidy in Chile (Bravo and Rau, 2013), and the First Employment Law in Colombia (Ariza and Cedano, 2017). The ETI is different from these policies mainly because it offers larger subsidies as a share of the wages paid; its value is as high as half the wages paid, whereas other subsidies studied amount to between 10 and 20 percent of wages. Furthermore, the ETI covers a broader share of workers, compared to the policy in Tunisia that targeted recent graduates or the policy in Chile that targeted poor young workers. It also covers a wider range of the distribution of income, including earnings well above the median and the current national minimum wage. Finally,

[8]By comparison, Nigeria, the largest economy in Sub-Saharan Africa, just preceding South Africa, had an unemployment rate of 19% in the third quarter of 2017.

[9]Income distribution in the country remains largely unequal, with one of the highest Gini coefficients in the world, and inequality, if anything, has worsened over time. See Sulla and Zikhali (2018) for a detailed report on inequality in South Africa.

unlike Turkey's program that was first designed to counter the effects of a recession, South Africa implemented the ETI independently of the business cycle. There have been some evaluations of wage subsidies on youth employment in developing countries using small-scale randomized control trials (Galasso et al. (2004) in Argentina, Groh et al. (2016) in Jordan), including a pilot evaluation in the case of South Africa (Levinsohn et al., 2014).[10] There is a debate on how scaling up policies changes their effectiveness. Analyzing the impact of the ETI and comparing it the pilot's results contributes to the evidence on the importance (or not) of scaling up in studying the effectiveness of public policies.

By studying the effect of the ETI, I also contribute to the literature on youth unemployment in South Africa. The alarming youth unemployment rate not only makes any potential policy of interest in itself, but analyzing the ETI can also help policymakers rethink that policy or inform future ones. The importance of the structural features underlying South Africa's high unemployment rate casts doubt on the effectiveness of any policy that does not adequately address those features. Economists often mention four main features: minimum wage laws, skill mismatches between labor supply and labor demand, firing regulations, and unions (Banerjee et al., 2008).[11] Minimum wage laws in South Africa potentially deter employers from hiring, especially hiring young workers, as their productivity is likely below the wage that they are at least entitled to.[12] The goal of a wage subsidy is to make these minimum wage less binding, by reducing the actual cost of labor. However, high firing costs are another feature in South Africa that could make firms reluctant to hire new young workers, even when a subsidy is offered, especially a temporary one. Firms perceive firing costs as very high, due to legislation that makes firing a worker a very last resort and due to unpredictable implementation of this legislation by labor courts (Bhorat et al., 2014). In fact, the initial policy proposal suggested a "no-questions-asked" dismissal period associated with the implementation of the ETI, but this suggestion never made it into the actual policy.

Finally, I contribute to the literature on wage subsidies in South Africa by providing reliable causal estimates of the impact of the ETI on young workers' labor market outcomes. In order for a firm to be able to claim the subsidy for of a given worker, the worker had to be recently hired (after October 2013) and aged 18 to 29 at the time of the claim. These criteria suggest using a difference-in-difference strategy to estimate the effects of the policy on the employment outcomes of young workers. I study individuals born between 1979 and 1988,

[10] An additional evaluation by de Mel et al. (2019) examines subsidies for small firms to hire additional workers, but without restriction on age for the workers to hire. The firms hired workers that were 31.5 years old on average.

[11] See the section on South Africa's labor market for a more detailed discussion.

[12] This is a recurring point in evaluations of wage subsidies: firms report that they do not hire workers because of the restrictions on wages. See Galasso et al. (2004) and Groh et al. (2016).

who would have been between 26 and 35 in the first year of implementation of the policy. In this sample, the cohorts 1984-1988 are eligible for the subsidy, and I use the cohorts 1979-1983 as a control group. This is a departure from Ranchhod and Finn (2016), who uses the sample of all working age individuals for their estimation, but an approach that Moeletsi (2017) previously used. I improve on Moeletsi (2017) by controlling for experience, a key dimension that eligible and ineligible cohorts differ on. Intuitively, young workers catch up with their older peers over time, as they accrue more experience; also, the relative importance of experience early in the life cycle has been documented in South Africa (Banerjee et al., 2008; Levinsohn, 2007). However, experience is not measured in the dataset available on workers' labor market outcomes. I address this by constructing a proxy for experience at an aggregate level. Finally, I use workers' information to analyze the ETI, instead of firms' data, as the design of the policy provides a criterion that allows estimation of causal effects on workers instead of firms. Ebrahim et al. (2017) studied the effect of the ETI using firms' administrative data. There are reasons to be cautious about the endogenous nature of firms' decision to claim the subsidy, even when a conditional difference-in-difference is used. For instance, firms that take up the subsidy likely anticipate that they will grow. The results by Ebrahim et al. (2017) are to be interpreted with caution because of this endogenous decision.

My results show that overall, the ETI did not lead to a significant increase in employment for young individuals.[13] Given the precision of my estimates, I can rule out improvement in overall employment in the order of 1 percentage point.[14] I find that men seem to have been more affected, as the estimates are consistent with larger effects than those for women. Indeed, if anything, my results imply that the policy led to *decreased* employment for women, but for reasons that I discuss later, this should be interpreted with caution. I find no differential effects with respect to race or education, two other important dimensions that affect outcomes on South Africa's labor market (Kingdon and Knight, 2004; Banerjee et al., 2008).

Taken together, these results imply that reducing labor costs may not be sufficient to induce firms to hire young workers. I argue that other constraints must be addressed in addition to reducing labor costs in order to improve employment rates for young workers. One such constraint is firing regulations in the country. When firms make hiring decisions in a dynamic setting, with a wage subsidy that lasts at most two years, they take into account the probability that they might hire a worker for whom the costs of firing might exceed the value of the wage subsidy, together with the forgone profit. If those costs exceed the benefit from

[13] Although these results are similar to Ranchhod and Finn (2016), it is worth stressing that I do find this result on a sample that is optimal for identifying the causal effects of the policy.

[14] These results rule out the effects found by Moeletsi (2017), suggesting the importance of controlling for experience when estimating the effect of the policy.

the wage subsidy, then firms would be unwilling to hire young workers. This reasoning is in accordance with the literature as McKenzie (2017) noted that wage subsidies tend to be more successful in settings where firms are not required to register workers, suggesting that labor regulations are an important barrier to the success of wage subsidies.[15]

The remainder of the paper is structured as follows. First, I discuss the literature about wage subsidies, with a focus on empirical evidence in developing countries. Second, I present the main features of South Africa's labor market. Third, I describe the policy, the data and my empirical strategy. Fourth, I present and discuss the results from my estimation. Finally, I conclude by discussing the lessons learned from evaluating this policy.

2 Wage subsidies in the literature

Wage subsidies are one type of active labor market policies, a category of policies that intervene in the labor market to correct imperfections, in order to improve employment prospects of some or all workers.[16] They are in contrast to passive labor market policies that provide income replacement to individuals that have lost or do not have a job.[17] [18]

2.1 Wage subsidies conceptually

Theoretical discussions of wage subsidies date back to Kaldor (1936). As mentioned in Katz (1996), the "basic idea behind (employer-side) wage subsidies is to reduce the costs to firms of employing the targeted group of workers thereby stimulating demand for these workers and raising their employment rates and earnings." This shifts out the demand for the labor of the targeted group, and the effects on employment depend on labor demand and labor supply elasticities. However, that assumes that markets determine equilibrium wages. If wages are determined otherwise, it is not profitable for firms to hire workers whose marginal productivity is under the prevailing wage (e.g. minimum wage), but wage subsidies can induce firms to hire such workers by offsetting or reducing the perceived productivity gap

[15] A related paper is Hardy and McCasland (2017) who studies the effects of a placement service for apprentices in Ghana. Among firms who expressed interest in hiring apprentices with no entry fees unlike what is regularly done, firms assigned no apprentice did not subsequently hire any worker. The paper cites frictions in screening apprentices' ability or skills as a barrier for these firms young apprentices.

[16] See Betcherman et al. (2004) and Card et al. (2015) for general meta-analyses, Kluve et al. (2017) for a survey of policies targeted at youth, and McKenzie (2017) for a survey of studies in developing countries.

[17] These include, for instance, welfare benefits and unemployment insurance.

[18] Besides wage subsidy programs, other active labor market policies are: (1) employment services that focus on producing better matches between jobs and job seekers, (2) training programs in specific skills that increase potential workers' employment prospects, (3) public work programs, by which the government directly provides short term jobs, and (4) micro-enterprise or self-employment development, which is assistance (either financial or technical) to help individuals to develop their own businesses.

(Wolff and Stephan, 2013).

Theoretical discussions highlight implementation issues and have long recognized the trade-off between general and targeted subsidies. A well-designed wage subsidy will minimize the "windfall" that arises with firms merely receiving money for workers that they would have hired anyway. This consideration favors targeted subsidies over general ones. For this reason, Layard and Nickell (1980) suggested subsidizing "marginal employment" (subsidies conditional on increasing employment at the firm level), but recognized the difficulty of accurately identifying marginal employment. Katz (1996) made the case for subsidies administrated as tax credits, in order to ease the burden of administration. However, targeted subsidies may come at the expense of distortions in labor allocation or stigmatization of targeted workers. Targeted workers can be hired at the expense of other similar workers (substitution effects) or other workers that are fired (displacement effects).

Besides the incentive created for firms to hire targeted workers, wage subsidies can also be justified by their long run effects, as accumulated experience raises workers' productivity. Advocates of wage subsidies view this as a justification despite concerns of substitution among eligible and ineligible workers (Bell et al., 1999). This also hints at the fact that wage subsidies can be more successful if they are combined with other activities that enhance productivity, such as on-the-job training (Katz, 1996).

2.2 Wage subsidies in developing countries

McKenzie (2017) reviewed experimental evidence on the effectiveness of active labor market policies in developing countries. It follows from the discussion of experimental evaluations of wage subsidies (Galasso et al., 2004; Levinsohn et al., 2014; Groh et al., 2016; de Mel et al., 2019) that they have a limited overall impact on employment. However, they can be useful as a temporary policy tool for the creation of short-term jobs. One useful insight that potentially applies to the case of South Africa is that covering only registered firms, or requiring eligible firms to register their workers seems to lower the effect. Groh et al. (2016) found a 38 percentage point increase in employment when firms are not required to register workers hired through the wage subsidy, whereas Galasso et al. (2004) found a 1.7 percentage point increase in employment when registration is required and firms face a penalty if they fire the worker after the end of the subsidy period. This suggests that wage subsidies are unlikely to be effective if firms have conditions on hiring, and points especially to the fact that Levinsohn's (2007) initial proposal suggested a "no-questions-asked" dismissal period that did not make it to the actual policy implementation. The effect in Groh et al. (2016)

is however not long-lived. [19]

Evidence from nationally implemented policies comes in part from measures taken by countries to counter the effects of the 2008 global crisis. In Turkey, these programs were aimed at firms hiring new workers who were previously unemployed (Balkan et al., 2016) whereas in Mexico, the policy aimed to prevent firms from firing workers (Bruhn, 2016). In both studies, the policies increase employment, showing that wage subsidies can be effective countercyclical measures. In particular, Balkan et al. (2016) show stronger effect in regions with higher unemployment rates, and evidence from Bruhn (2016) shows that firms could use the subsidies to hire enough workers to return to their pre-crisis levels of employment.

Nationally implemented wage subsidies have different targeting. The policies in Chile (Bravo and Rau, 2013) and Columbia (Ariza and Cedano, 2017) targeted young low wage earners. The wage subsidy in Tunisia (Broecke, 2013) targeted recent college graduates. Unlike these studies, my paper looks at a policy that has a broader eligibility criterion, as all workers aged 18-29 were eligible. The post-2008 Employment Subsidy Program in Turkey analyzed in Balkan et al. (2016) had a larger targeting, as firms could claim subsidies for all women above 18 and for men aged 18-29. Wage subsidies sometimes have targeting based only on firms. The policy in Turkey analyzed in Betcherman et al. (2010) targeted the poorest provinces in the country while the one in Mexico (Bruhn, 2016) was available only for firms producing durable goods (e.g. firms in manufacturing and construction industries).

Among the previous studies, two have investigated whether their positive employment effects were driven by substitution or displacement effects, i.e. firms hiring workers, but not creating new jobs overall in the economy. Betcherman et al. (2010) conduct an imperfect test examining economic activity by using energy consumption, whereas Bruhn (2016) compared hiring patterns by eligible and ineligible industries. Bruhn (2016) found no effects of substitution; although one should bear in mind that these results are estimated in the aftermath of a recession, implying a large of pool of unemployed individuals to hire from. I discuss later the potential implications of displacement or substitution effects given my results.

[19] One aspect that is missing in the literature is the effect of wage subsidies coupled with other interventions, such as training for the workers. Groh et al. (2016) offered soft skills training in their context, but this did not lead to differential effects.

3 Context

3.1 South Africa's labor market and youth unemployment

Economists point to four features as the main reasons for the high unemployment rate in South Africa (Banerjee et al., 2008): minimum wage laws, skill mismatch, labor regulations regarding firing, and unions.

South Africa adopted a national minimum wage in 2018, and it has been in effect since January 2019. The level was set at ZAR 3500 a month, which was close to the median monthly earnings at the time.[20] Before 2019, minimum wages were defined at the sectoral level and negotiated between firms and workers' unions at the district council level. Studies of revisions to sectoral minimum wages found mixed results of these changes.[21] Dinkelman and Ranchhod (2012) found that the creation of a minimum wage for domestic workers who were not previously covered by such laws did not lead to a decrease in employment, whereas Bhorat et al. (2014) found that the institution of a minimum wage for agricultural workers led to a decrease in employment for these workers. The high unemployment in South Africa rate is consistent with a binding minimum wage that is higher than the marginal productivity of unemployed workers (Banerjee et al., 2008). Young workers are likely less productive than more experienced and likely older ones, because on-the-job training and experience increase productivity. As such, minimum wages may have a bigger impact on the employment of young workers. [22]

A skill mismatch between labor demand and labor supply has been reported as another reason, one that explains the rise in unemployment in the decade following the end of apartheid. Labor supply has increased, primarily through the addition of unskilled workers, at the same time that the economy became more skill biased (Banerjee et al., 2008). There are two sides of the nexus between skills and employment outcomes. The first relates to the level of education. There are high returns to completing secondary school, but many youths do not finish their secondary education. The second relates to the skills that education indeed confers to workers. Firms complain that education qualifications convey little information on workers' skills. Indeed, Carranza et al. (2019) show that skill certification increases youth

[20] ZAR = South African Rand. Over the period that the policy covered, the exchange rate fluctuated between 10 to 15 rands for a dollar.

[21] The lack of consensus on the employment effects of minimum wage is not specific to South Africa. See for instance Card and Krueger (1994) and Neumark et al. (2014) for evidence in the US.

[22] Neumark and Wascher (2004) provide cross-country evidence that minimum wages decrease youth employment, with a larger effect when there is no specific minimum wage for workers and when labor standards and union coverage are higher. Gorry (2013) further provides theoretical evidence that minimum wage interacts with the ability of a young worker to gain experience, leading to larger effects.

employment, by removing information frictions between workers and employers regarding skills, whereas Abel et al. (2019) show that a reference letter from previous employers increase callbacks and employment for women, although the latter findings likely confound information frictions about both skills and experience.

The mechanism behind the link between labor regulations and unemployment in South Africa is not clear, especially for firing regulations. The regulations appear to be no different from those in Latin American or European countries that have stringent labor regulations but much lower unemployment rates. However, firms cite high separation costs as a deterrent for hiring decisions. Benjamin et al. (2010) suggest that labor courts interpret the law inconsistently, raising the uncertainty of the outcome when a dismissal is challenged as unfair. Bertrand and Crepon (2019) further show that providing information on labor regulations to firms make these firms hire substantially more workers.[23] Because there is no age restriction on these regulations, they are likely to be more binding for firms when recruiting young workers, as these workers' productivity is likely lower on average and has greater variability.[24]

The presence and strength of unions in the country reinforce the role of minimum wage and labor regulations as constraints for job creation. Labor regulations explicitly grant each worker the right to be represented by a union member, both during internal discussions regarding dismissals and during court hearings. With unions' support, workers have better representation in courts and are more likely to have favorable outcomes during trials. This likely raises firms' costs of dismissal. Unions are also responsible for collective bargaining that sets wages at sectoral and district council levels.

Given its high unemployment rate, it is surprising that informal employment in South Africa is low, especially compared to other countries in Sub-Saharan Africa. The share of workers in informal employment is 34 percent of the total employment in South Africa, compared to 72 percent in Africa and 53 percent in Latin American and Caribbean countries (ILO, 2018). Plausible explanations include barriers to entry in terms of credit constraints, lack of skills

[23]Bertrand and Crepon (2019) report effects 6 months after their intervention. One possible explanation of their results is that providing information about regulations to firms did not make these firms create jobs, but made them more confident in their hiring decisions, and consequently make them hire at a quicker pace than without the information. As such, the effects found could be driven by quicker hires instead of job creation.

[24]Firms might be able to identify some of the older workers who have high ability or high productivity because of their experience, as these workers are more likely to get and mostly keep jobs, which would show in their experience, for example on their resume. This is not the case for young workers, as they usually do not have the experience. With this greater uncertainty about young workers' productivity, there are higher risks that firms get a worker with a low ability among young workers and that this worker might be difficult to fire later, due to the regulations.

and a lack of entrepreneurial culture due to the legacy of apartheid (Kingdon and Knight, 2004). These constraints also account, to some extent, for why firms in the formal sector do not hire more. Magruder (2012) further makes the case that regulations in the formal sector spread to the informal sector; regarding wage settings (recall the discussions above), they could raise wage expectations overall which negatively impacts employment creation by small firms, thus possibly firms in the informal sector. Young workers are usually disproportionately represented in the informal sector (Filmer and Fox, 2014), so an unusually small informal sector may contribute to high youth unemployment.

The high unemployment rate in South Africa has often raised questions about how unemployed individuals meet their basic needs without labor income. One hypothesis is that unemployed workers may rely on other sources of income and consequently that part of unemployment might be voluntary. Transfers from social programs, especially the old age pension, are one such source that is often mentioned. It is not clear however how the presence of pensioners in a household affects the labor supply of working age members. Studies that claim that the old age pension reduces labor supply (e.g. Bertrand et al., 2003) ignore the effect on migration, as the pension seems to provide a relaxation of financial constraints on migration for rural households (Posel et al., 2006; Ardington et al., 2009). However, Abel (2019) suggests that the results may not generalize to the whole country. These mixed results cast doubt on the hypothesis that income from social programs alters labor supply behavior and leads to voluntary unemployment.

These features of South Africa's labor market call into question how likely it is that a wage subsidy will increase young workers' employment. Minimum wages create a wedge or gap between marginal productivity and wages, which is likely larger for young workers since they are less experienced and less productive. A wage subsidy lowers the costs of the labor, in this case of young workers' labor. If we first ignore the dynamic aspect of their decision-making, the wage subsidy may induce firms to hire more young workers than otherwise, provided that the subsidy compensates for the gap between marginal productivity and the minimum wage or other wage paid. Skills mismatches contribute to the productivity gap; the lower the skills of young workers, the higher the wage subsidy needs to be to induce firms to hire young workers. If there are skills that young workers can acquire by on-the-job training, the wage subsidy then acts as a compensation for the employer's costs of training the worker. Firms without the subsidy would not have voluntarily incurred these costs, especially since there is no way to guarantee that the workers will stay at the same firm once they are trained. [25]

[25] In theory, a solution to the costs of training faced by the employer (together with the risk that the worker leaves once he or she is trained) is that the firms pay a lower wage at the beginning of the worker's tenure, and then increase the wage with tenure. However, wage floors in terms of a minimum wage could

Labor regulations highlight the importance of considering the dynamics of firms' decisions and their uncertainty regarding young workers' productivity. [26] In South Africa, firing regulations imply that firms are uncertain that they could fire a worker once they have hired him or her, or that they are uncertain of the cost at which they can fire the worker. In the case of a permanent subsidy, these regulations would have little influence on whether firms hire young workers. When the subsidy is temporary however, the firm needs to account for the possibility that it gets stuck with a worker that would be difficult to fire; the productivity of the worker may well be above the actual costs of labor for the firm during the subsidy period, but not afterwards. The presence of unions would tend to reinforce the point on labor regulations, as unions represent workers in courts during unfair firing trials. In the end, the decision to hire workers targeted by the subsidy will depend on whether the subsidy offsets the previously perceived productivity gap, accounting for potential on-the-job training , but labor regulations make firms less likely to hire despite the subsidy, especially given the uncertainty of young workers' productivity.

3.2 Presentation of the Employment Tax Incentive

Levinsohn (2007) first proposed a wage subsidy to tackle youth unemployment in South Africa.[27] The wage subsidy is justified by the fact that young workers face high unemployment rates, but those who experience smooth transitions to work remain employed in the future. This suggests a policy intervention that helps youths find their first job in order to propel them into the labor market. The original proposal argued for an individual account for young job seekers that they could use with any prospective employer. This framing seems to present youth unemployment as a low supply of labor issue instead of a low demand for labor. However, it is firms that need to be incentivized since they do not create jobs. In the end, the subsidy was implemented as a reduction in tax liabilities for employers. The initial proposal also included a probationary period during which a "no-questions-asked" dismissal policy would be in place, but this provision was not part of the implemented policy.

make this impossible.

[26] The fact that firms are more uncertain of young workers productivity that they are of older ones plays into how minimum wages affect the effectiveness of a wage subsidy. However, if there were no regulation preventing form firing workers, firms can experiment with a wage subsidy, assured that they can fire the worker once he or she is hired and the firm learns about his or her productivity. The uncertainty about young worker's productivity is thus more relevant in relation with labor regulations.

[27] The other policy suggested is a reform on immigration policy that would encourage an influx of educated migrants especially from other African countries. The reason for such a policy is that skilled and unskilled labor are complement rather than substitutes. An increase in the level of education of the population propelled by an immigration reform is thus likely to increase employment prospects for low skilled labor.

The proposal was enacted into law as the Employment Tax Incentive Act which was signed in December 2013. The bill made it clear from the onset that the wage subsidy was a temporary policy, active from 2014 to 2016, that would be subject to a review and considered for a non-guaranteed extension. Specifically, policymakers expected to spend ZAR 5 billion over 3 years to create 178,000 new jobs for youths at a cost of approximately ZAR 28,000 per job, while subsidizing 423,000 jobs.[28] The policy has since been extended twice, in 2016 and in 2018, and is still in implementation until 2029. The decision to extend was based primarily on firms' higher-than-expected take-up of the subsidies. During the first two years of implementation, more than ZAR 6.3 billion was claimed, with an estimated 650,000 jobs claimed in the fiscal year 2014/2015 alone (National Treasury, 2016).

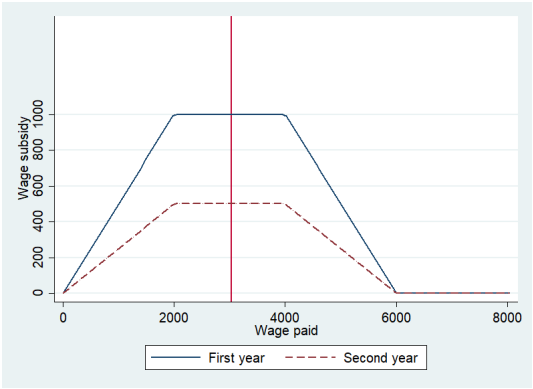
The wage subsidy has eligibility criteria on both the firm and the worker side. The firm needs to be registered for Pay-As-You-Earn (PAYE) for employee income tax purposes. Firms in the public sector are not eligible. Firms can also only claim deductions if they do not owe the South African Revenue Service any money. The bill requires that no older worker is displaced as the consequence of the young worker being hired, upon penalty of ZAR 30,000 and the possibility of losing one's eligibility to claim the incentive in the future. Finally, the employee's wage needs to be in accordance with minimum wage regulations. For their part, employees need to be between 18 and 29 years old, for the firm to claim the subsidy on their behalf. The other important restriction regarding the employee is that he or she may not be related to the employer.

The employer can claim the wage subsidy as a reduction in taxes owed the National Treasury.[29] The subsidized amount per eligible worker hired depends on the wage he or she is paid, as shown in Figure 1. The amounts shown correspond to the subsidies for full-time workers. For part-time workers, the subsidy is proportional to the earnings that would have been paid if the worker was working full time, at the same wage rate. The subsidies can be claimed by an employer for the same worker for up to two years. The value of the subsidy falls by half in the second year it is claimed by the same firm, for the same worker.

[28] As a comparison, the government social protection programs amounted to ZAR 130 billion for the fiscal year 2013/2014. These programs primarily include the old age pension and the child support grant.

[29] Employers are subject to monthly withholding, including income tax that they pay to the South Africa Revenue Service. While paying the amount that they owe, firms deduct the subsidy and pay only the net amount due for withholding for all their workers. In case their total withholding is below the total subsidy owed, the excess is paid back to the employer as part of the reconciliation that employers must submit twice a year.

Figure 1: Amount of the ETI subsidy



Notes: The vertical line denotes the median monthly earnings the year before the ETI was implemented.

3.3 Previous evaluations of wage subsidies in South Africa

Prior to the implementation of the Employment Tax Incentive, Levinsohn and Pugatch (2014) used a search model estimated using the Cape Area Panel Study data to perform a prospective analysis of the impact of the policy. The study focuses on how the search behavior of workers responds to the subsidy, but their model does not incorporate firms' decisions, even though the statutory incidence of the subsidy is on firms. Levinsohn and Pugatch (2014) show that a wage subsidy can increase reservation wages, although when combined with the increase of the value of searching for a job, the net result is an increase in youth employment. They estimated that a ZAR 1,000 subsidy would decrease youth unemployment by 12 percentage points.

Levinsohn et al. (2014) later conducted an experiment piloting a wage subsidy voucher for young workers. A sample of 4000 youths aged 20-24 at baseline was divided into a wage voucher group and a control group. Follow-up surveys show that individuals who were offered the voucher were 7 percentage points more likely to be employed one year later (the period during which the voucher was redeemable) and 10 percentage points more likely to be employed two years later. Attrition is important, however, and may be driving the medium term effects. Besides this point, the authors noted that their results may be driven by the control groups turning down more job offers, especially when they are in households that have other employed individuals.

After the ETI was implemented, Ranchhod and Finn (2016) carried out a short term evaluation using the Quarterly Labor Force Survey. Using a difference-in-difference estimation

strategy, they found no effects in the first year of policy implementation. Moeletsi (2017) later identified a flaw in their definition of the treatment group. Ranchhod and Finn (2016) defined the treatment group by current age (the below 30 group), rather than by age at the time of implementation. Moeletsi corrected this by defining the treatment group in term of fixed cohorts (born after 1985). Moeletsi's approach is an improvement, but it fails to account for workers' experience, which is likely to bias his estimates up, since young workers will catch up with the older ones over time as they gain more experience.

Ebrahim et al. (2017) use firms' administrative data to compare firms that claimed the tax incentive to firms that did not, before and during the implementation of the policy. They use a combination of matching and difference-in-difference methods. Their results indicate that, on average, firms that claimed the incentive hired more youths than to firms that did not. However, they found that hiring of non beneficiaries (workers aged 30 or above) increased by about the same amount. This most likely means that they capture the fact that firms that took up the subsidy would have hired workers even without the ETI. Using administrative records, they compute the deadweight loss due to subsidized jobs that would have been created anyway, given their estimates. Their results indicate that only 8% of claims can be attributed to newly created jobs, a very small share compared to similar policies in other countries.

4 Data and methodology

4.1 Data: the Quarterly Labor Force Survey

The Quarterly Labor Force Survey (QLFS) provides the best available information on South African labor market outcomes at the worker level, given its frequency and its national scope.[30] Since 2008, the QLFS has collected information from a nationally representative sample, at a quarterly frequency. It is a rotating panel in which one fourth of the sample is replaced each quarter. [31] The sampling unit is the residential address, whereas the unit of observation is the household. As such, in case of residential turnover, the new household is surveyed in place of the older one. Each household is interviewed in a maximum of four

[30]Other available datasets are the National Income Dynamics Survey (NIDS), a panel survey conducted every two years since 2008, and the General Household Survey, which is conducted once a year as a general purpose survey but also collects labor market information.

[31]This dataset can be compared to the US Current Population Survey, which also has a rotating panel structure. According to the United States Census Bureau, "households from all 50 states and the District of Columbia are in the survey for 4 consecutive months, out for 8, and then return for another 4 months before leaving the sample permanently". (see https://www.census.gov/programs-surveys/cps/technical-documentation/methodology.html). For the QLFS, dwelling units are in the survey for four consecutive quarters and then leave the sample permanently.

consecutive quarters.

In order to obtain repeated and independent cross-sections and avoid issues of correlated measures within households over time, I restrict the sample used in my analysis to the first time each dwelling unit in the survey is observed in the data, which I call the incoming rotation group. The rotation groups are designed such that each group has "the same distribution pattern as that which is observed in the whole sample" (Statistics South Africa, 2008) and this ensures that the incoming rotation group constitutes a nationally representative sample. Using the incoming rotation group avoids attrition issues, especially if individuals or households leave the survey in ways that are related to eligibility to the ETI.[32] Papers such as Verick (2012) have used algorithms to recover the panel structure of the dataset, specifically by tracking changes in the structure of the households interviewed in the same dwelling place. As I am not interested in changes within individuals but in comparing cohorts over time (see more on the methodology in the section below), the repeated cross-section component is suitable for the analysis. Also, given the short length of time that individuals stay in the panel (four consecutive quarters at most), the panel dimension of the dataset is not useful for my estimation strategy.

The QLFS is designed to be representative at the province level, and within the province level, at the metropolitan/non-metropolitan area level. [33] The sample drawn for the survey is based on a stratified two-stage sampling design; in the first stage, primary sampling units are chosen with probability proportional to their size, and in the second stage, dwelling units are systematically drawn with equal probability. At the first stage, the sampling is stratified by the province-metro/non-metro areas. This survey design produces inverse probability weights that can be used to produce statistics that are unbiased for population level parameters. Statistics South Africa further adjusts those weights for non-responses, so that the information from the survey is consistent with the information available at the time on the structure of South Africa's population. [34] I treat those final weights as population weights for my estimation purposes.

[32] In order to have a sample of independent cross-sections, studies in the US that use the CPS usually resort to the outgoing rotation group, i.e. households that are leaving out the survey for four months or indefinitely, but more so because these households are asked additional questions. In the case of the QLFS, since there is no additional advantage of using the equivalent outgoing rotation group, using the incoming rotation makes more sense. It further avoids attrition issues, to the extent that these issues are not properly accounted for by the weight corrections performed by Statistics South Africa.

[33] Metropolitan municipalities are important municipalities that enjoy an autonomy in their administration, as opposed to other areas. There as 8 such metropolitan areas, with three of them in the province of Gauteng. Provinces are consequently often divided into metropolitan municipalities and non-metropolitan areas.

[34] See Statistics South Africa (2008) for a discussion of the computation of the weights.

There have been changes over time in the methodology of the survey, especially in the master sample from which primary sampling units are drawn. From 2008 to 2014, the QLFS used a master sample based on the 2001 census data. A change occurred in 2015, with the master sample rebased to the 2011 census. This poses problems for using time variation that includes data before and after the update, as any break in the structure of the data can be a confounding factor that prevents interpretation of the estimates as causal effects of the policy. There is still debate about the implications of these changes in the survey on measurement of employment (see Kerr and Wittenberg, 2019, for a discussion). As such, I only use data from 2014 and earlier, which use the 2001 census' master sample, and cover the first year of the policy's implementation.

I consider as employment outcomes overall employment (employment in any firm), private employment and private formal employment. I consider workers employed in the private sector when they work for a private firm and in private formal employment when they report that their employer deducts income tax from their salary. The goal of the ETI is to increase employment for young workers. The subsidy does so by targeting private firms that are registered for tax purposes. Analysis of private formal employment gives us the direct effects of the policy on the targeted outcome. However, the underlying goal of the policy is to increase overall employment for youths, which may extend beyond private formal employment. For example, youths could respond through changes between jobs, such that overall employment does not change for young workers but private formal employment increases. Another example is that some workers may be encouraged to search and find informal jobs, but not formal jobs, which would increase employment beyond any effect on private formal employment. As such, I consider any private employment and any employment as additional employment outcomes.

I additionally consider participation in the labor market as an outcome, for two reasons. First, the subsidy may have induced young workers to look for jobs, if these workers believed that what their decreased costs to firms may increase their chances of being hired. Second, because of this first point, looking at both employment outcomes and participation will give us an idea of the effects of the subsidy on unemployment. Although it is possible to study unemployment by looking at individuals that are willing to work, the subsidy could potentially have affected this group, making the restriction endogenous.

4.2 Identification strategy

Because the policy has a clear start date and clear eligibility rules for workers whose wages can be subsidized, I use a difference-in-difference strategy to estimate the effects of the Em-

ployment Tax Incentive on young workers' employment outcomes.[35] A firm can claim the subsidy if it hired a worker after October 2013 and the worker was between 18 and 29 years old at the time he or she was working at the firm. Although this criterion is labeled in terms of current age, it is more intuitive to reason in terms of cohorts. Table 1 summarizes the cohorts who were eligible and the time when they were eligible. Using current age as the distinguishing criterion between the treatment and control groups for the difference-in-difference analysis could be misleading. Because eligibility stops once the individual turns 30, using current age rules out the effects of the policy on workers after their eligibility. The justification for a wage subsidy was to propel young workers into the labor market; if they stay employed even after their eligibility ends, this should be part of the effects of the policy. Treated workers could stay in employment after their eligibility because they keep the job that they were recruited for under the policy, or because they get other jobs, using the experience that they accumulated from subsidized employment. Arguably, these are not major concerns as I am only interested in the first year of implementation, but I adopt this approach nonetheless as it constitutes the right way to define eligible and ineligible individuals. [36]

The treated group is the group of individuals who are 29 or younger as of 2013Q4. Because the QLFS only reports the year of birth, this translates into individuals born in or after 1984 (see Table 1).[37] The control group is composed of individuals who were 30 or above at the time of implementation, meaning that they were born in 1983 or before. Plausible causal estimates require that the treatment and control groups are comparable; for this reason I use a sample of individuals 5 years above (control group) and 5 years below (treatment group) the cutoff provided by eligibility of workers for the subsidy. The main sample in my estimations is thus the group of individuals born between 1979 and 1988.

[35] There is always the concern that agents can anticipate future policies and act accordingly. This can lead to bias in the estimation of the effects of these policies. In the case of the ETI, there seems to have been a long debate at the parliament on this policy, as early as in 2011; also, there was no clear announcement that predated the signing of the ETI act. Given this, such anticipations are not a concern in this specific case. However, I provide a discussion below on how the placebo test in periods before the policy addresses to some extent the anticipation concerns.

[36] Studies of nationally implemented wage subsidies which targeted individuals based on age (e.g. Balkan et al., 2016) likely estimate a lower bound of the effects of these policies. Once an individual passes the age of eligibility, they become part of the control group in those studies' empirical strategy and thus contribute to the construction of the counterfactual. To the extent that there are returns to the experience accumulated because of subsidized employment, this overestimates what would be the employment for the control group in the absence of the subsidy, and biases the estimate of the policy down.

[37] Although the questionnaire of the QLFS asks for the exact date of birth, the publicly available dataset only contains the age in years. Discussions with staff at DataFirst suggest that this may have to do with the fact that an individual's national identification number is composed of his or her day of the birth, information that needs to be removed for anonymity purpose. Consequently, I can only consider in my analysis the age at the time of the survey or the year of birth.

Table 2 presents summary statistics on employment outcomes and other characteristics for the treatment and control groups, before and after the policy. Workers in the young cohort saw their employment increase for all measures of employment, though these before and after statistics do not necessarily reflect causal effects of the program, for reasons that I discuss below.

I estimate the following equation to obtain the difference-in-difference estimates using a linear probability model, as my outcomes of interest are binary in a nature (see the previous subsection on data): [38]

$$E[Y_{icpt}|X_{icpt}] = \beta_0 + \delta_0 d_c + \delta_1 d_c * post_t + \beta_1 exp_{cpt} + \beta_2 exp_{cpt}^2 + \gamma X_{icpt} + prov_p + Year_t + Qtr_t \quad (1)$$

Y_{icpt} is the employment outcome of interest, for individual i in cohort c, who lives in province p and was interviewed at quarter t. d_c is a dummy for young cohorts that is 1 for cohorts c =1984-1988 and zero for cohorts c =1979-1983. In my main specification, I use data for $t = 2011Q1, \cdots, 2014Q4$. $post_t$ is a dummy variable for periods during and after $2013Q4$, the first five quarters of implementation of the ETI. δ_1 is the difference-in-difference parameter, the parameter of interest. The equation controls for experience with a proxy constructed at the year of birth-province level, exp_{cpt}. Province of residence ($prov_p$) is included to capture province specific time-invariant characteristics that may be correlated with employment opportunities. X_{icpt} are additional control variables, including race, gender, and education.

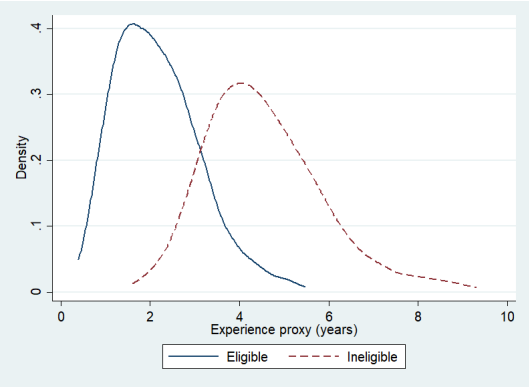
The QLFS does not report information on labor market experience. However, it is important to control for experience in the equation above. The treatment and the control group differ on the experience that they have acquired in the labor market, with older individuals having greater experience, and it is known that accumulated experience affects labor market outcomes, typically with a quadratic functional form. I construct a proxy for experience at an aggregate level. For example, in 2012, 39.2% of individuals born in 1984 and living in Gauteng were employed; this implies that, on average, the group of individuals born in 1984 and living in Gauteng has accrued its labor market experience by .39 year in 2012.[39] I use data from all labor force surveys conducted since the older cohort entered their working age then compute the average employment ratio for each year of birth cohort at the province level. I next sum these average employment ratios to obtain my proxy for experience. Ex-

[38]The absence of an error term in the equation below reflects the fact that it is a linear probability model that I estimate.

[39]I consider the frequency of the surveys (biannually until 2007, then quarterly afterwards) in computing the experience proxy. The example here uses the annual employment ratio for illustration purposes.

perience at time t for the cohort c living in province p is then the cumulative exposure to the labor market that this group has had until time $t-1$. Figure 2 below presents the distribution of this proxy for the young and the old cohorts, before the policy, with more detailed information in Figure 4.

Figure 2: Distribution of experience proxy



All the estimates that I present are weighted by the adjusted inverse probability weights that Statistics South Africa provides for the QLFS. Although the use of weights is common for the estimation of parameters describing variables' distribution, there is a long debate in economics and social science in general on whether those weights should be used when running regressions (see e.g. Solon et al., 2015, for a discussion on the subject). The consensus seems to be that weights should not matter, especially when the variables describing the sampling scheme, and thus the weights, are included in the model that is estimated. If they do not matter, then not using them should be preferred, as the OLS estimator is BLUE, while the weighted least squares estimator is not. However, in the case of difference-in-difference, the model estimated is as much statistical as it is economic. In the end, it is a difference in means that is estimated and as such, the means should be estimated to be representative of the population. Although the variables that I include in the model are guided by economic knowledge of labor markets in general and South Africa's labor market in particular, the goal is to adjust for any characteristics that may be affecting labor market outcomes differently for the young and the old cohorts. All this favors the use of weights in the present case.

4.3 Suggestive evidence of parallel trends

The identifying assumption for the difference-in-difference strategy is that the treatment and the control groups would have evolved in the same way if no policy intervention were made,

such that the changes in the outcomes of the control group are a valid counterfactual for the treatment group, in the absence of the policy. Although this assumption cannot be tested directly, it is possible to check if, before the policy, employment outcomes for the young and the older cohorts evolved in similar ways. There are two ways to implement this test. The first is to estimate differences between the two groups, conditional on observables, in periods before the policy, and test whether they are significantly different over time. I use the following equation for this test:

$$E[Y_{icpt}|X] = \beta_0 + \delta_0 d_c + \sum_{q=2011Q1}^{q=2013Q2} \delta_q d_c * 1_{\{t=q\}} + \beta_1 exp_{cpt} + \beta_2 exp_{cpt}^2 + \gamma X_{icpt} + prov_p + Year_t + Qtr_t \tag{2}$$

Y_{icpt} is the employment outcome of interest, for individual i in cohort c, who lives in province p and was interviewed at quarter t; d_c is a dummy for eligible cohorts that is 1 for cohorts c =1984-1988 and zero for cohorts c =1979-1983. I use data for the periods t = 2011-2013$Q3$. In the equation above, δ_0 is the difference between young and old cohorts in 2013$Q3$. $\delta_q + \delta_0$ is the difference for each of the quarters $q = 2011Q1, \cdots, 2013Q2$. The test of parallel trends before the policy is thus the test of joint significance of the coefficients δ_q. Figure 5 presents these coefficients. Although the figure points to differences in the coefficients δ_q for all the outcomes over time, the confidence intervals associated with the estimates do not allow to rule out that they are jointly equal to zero. As a result, I do not reject that there were parallel trends in outcomes between the eligible and the ineligible cohorts before the policy.

A similar test is whether there has been a differential change in employment outcomes between the eligible and the ineligible cohorts, in periods just prior to the implementation of the policy. I perform this test for the first three quarters of 2013, i.e. the three quarters immediately before the implementation of the ETI. This is intuitively a placebo test of a change before the policy was implemented. If the test detects a change, then it is unlikely that eligible and ineligible cohorts had the same trend. To implement this test, I run the following regression, using data for $t = 2011Q1, \cdots, 2013Q3$:

$$E[Y_{icpt}|X] = \beta_0^P + \delta_0^P d_c + \delta_1^P d_c * placebo_t + \beta_1^P exp_{cpt} + \beta_2^P exp_{cpt}^2 + \gamma^P X_{icpt} + prov_p + Year_t + Qtr_t \tag{3}$$

where $placebo_t$ is a dummy that is 1 for the quarters 2013$Q1$ to 2013$Q3$ and zero for 2011 and 2012. If the parallel trend assumptions hold, then the coefficient δ_1^P should be zero. The estimations in Table 3 show that I cannot reject that this coefficient is equal to zero. In particular, the estimates for private employment indicate that if anything, young workers may have been catching up to the older workers. This points to the importance of controlling for experience and suggests that my estimates are likely an upper bound on the true effects

of the policy.

Results of equation 3 also provide evidence against the idea that firms or workers took anticipatory actions in advance of the policy implementation. Such reactions would include firms firing young workers with the expectation of rehiring them and thus benefiting from the wage subsidy, or firms postponing their hiring decisions regarding young individuals. Both actions would show up as a relative decline in employment outcomes for young workers, leading up to the policy start date. While the policy had a provision against displacement of older workers (see the earlier presentation of the ETI), there was no explicit mention of re-hiring, leading to concerns that there might have been some temporary separations between firms and workers. The presence of such effects would imply a negative coefficient δ_1^P. The fact that this coefficient is not statistically different from zero, and if anything slightly positive for employment and private employment, helps rule out these concerns.

5 Results

5.1 Main results

Table 4 presents the results of the estimation of Equation 1. The specifications in the first two columns do not control for potential experience, while the two last columns do. The first two specifications imply that the overall employment for young workers increased by between 1.3 and 1.8 percentage points, relative to an employment ratio of 44 percent before the policy began. Although these results are not statistically significant, I cannot rule out effects as large as 3 percentage points, and this includes an increase of 2 percentage points, the size of the effects found in Moeletsi (2017). Once I control for potential experience, my estimates suggest that the policy led to a *decrease* in the probability of employment, but not a statistically significant one, of between .2 and .7 percentage points. More importantly, I can rule out benefits as small as a 1 percentage point gain. Notably, controlling for experience reduces the estimated impact of the ETI.

The estimates for private employment and private formal employment are similar in their magnitude and their precision to the estimates for overall employment. Even though only firms that were private and registered for income tax purposes were eligible to claim the subsidy for workers that they hire, the policy could have resulted in reallocation of labor instead of job creation, such that effects on private employment or overall employment are different from the effects on private formal employment. Similar results for all these outcomes imply that the policy did not lead to such reallocation.

In the previous section on parallel trends before the implementation of the ETI, I noted that if anything, young workers' outcomes were catching up to those of workers a few years older. If that trend continued even in the absence of the policy, young workers may have continued catching up with older workers. Then, the effects estimated with a difference-in-difference strategy would tend to over-estimate the effects of the policy. This further reinforces the interpretation that the policy did not have meaningfully large positive effects.

One concern with the preceding analysis is that I use as a control for experience a proxy constructed at an aggregate level, the birth year-province (cp) level, while analyzing an outcome at the individual level. Measurement errors in aggregation could lead to bias in the implied effect of the policy. For this reason, I re-estimate Equation 1 at the level at which the proxy for experience is constructed. Table 5 presents the results of the aggregated level regression. There are 10 birth cohorts in my sample (5 in the eligible group and 5 in the ineligible cohort) and 9 provinces in South Africa, which results in 90 units of observation. I use 16 quarters for my main estimations (2011-2014); this amounts in the end to 1440 observations, as shown in Table 5. The estimates in Table 5 are similar to the ones at the individual level (Table 4, columns 3 and 4), suggesting that measurement errors due to aggregation may not be an issue biasing my results.

Table 6 presents the effects of the policy on young workers' labor force participation. I look at this outcome in isolation from the others as it reflects the reaction of individuals to the prospect of a wage subsidy. Given that they now represent a cheaper labor to hire, young workers may have internalized the increased value of job searching and responded by increasing their labor force supply.[40] The estimates however show that there was no increase in labor force participation for young workers as a result of the ETI. In fact, the estimates for labor force participation are similar to the ones for employment outcomes, suggesting that young workers did not increase their labor force supply when the ETI became available. One possibility is that these young workers may have correctly anticipated that the policy would not increase their chances of employment. By combining the results on employment and labor force participation, it follows that the ETI did not affect unemployment for young workers. [41]

Overall, my results suggest that the Employment Tax Incentive did not lead to the (large)

[40] By looking at labor force participation, I investigate the effects of the ETI on the extensive margin of the labor supply.

[41] Unemployment is defined as employment among workers who supply their labor force, and neither employment nor participation is affected by the policy. Looking at the the effects of the policy on employment and labor force participation separately to infer the effects on unemployment avoids analyzing a sub-sample (active workers) that is potentially changing over time because of the policy, if the policy had an effect.

effects expected from the policy. My results are similar to those in Ranchhod and Finn (2016), despite using a more appropriately defined sample that does not include much older and more experienced workers than those targeted by the ETI. These results, however, stress the importance of controlling for experience when accounting for differences between the eligible and the ineligible cohorts. Not doing so leads to an over-estimation of the effects of the ETI.

There have been a number of concerns raised in the literature about wage subsidies that I investigate here to explore the extent to which they may confound or qualify my findings (McKenzie, 2017; Card et al., 2015). The first concern is displacement effects. Because of the targeting, the wage subsidy can lead to displacement effects, whereby workers that are not targeted by the policy are fired and replaced by targeted ones. This may be less likely in South Africa given the design of the ETI and features of the labor market. The ETI Act includes a penalty for firms that fire an older worker to hire one who qualifies for the subsidy, which should deter such displacements. There are also high perceived dismissal costs in the country. Workers have the right to challenge unfair dismissal, and in this specific case, older workers could challenge unfair dismissal to hire a young worker. Previous research (e.g. Benjamin et al., 2010) suggests that the disruptions that these challenges cause to firms is a serious concern in general, and this would have discouraged firms to fire older workers to recruit younger ones. A final point is the absence of large positive effects which further casts doubt that displacement effects might have occurred. If there were such displacement effects, employment outcomes of the ineligible workers would have fallen compared to a situation where no policy was implemented. The difference-in-difference estimator would then capture both jobs obtained by eligible workers, and jobs that were displaced, overstating the effect on eligible workers. The fact that I can rule out even small positive effects provides evidence against such concerns. It thus seems in the end unlikely that there would have been such displacement effects.

A second concern is substitution effects. When wage subsidies are targeted, firms may hire eligible workers at the expense of other workers, which is labeled the substitution effect. This is different from displacement effects because firms do not fire workers. If there were substitution of old workers by young workers, the difference-in-difference estimates would capture these effects, since it is estimated out of differences in outcomes for young and older workers. The job creation effects would then be lower than the effect estimated here. But the fact that the estimates are low implies that substitution effects are not a concern here. All young workers are eligible, and as such, substitution between young workers is unlikely.

The third concern is deadweight loss: firms could take up the wage subsidy and hire workers that they would have hired anyway, even in the absence of the wage subsidy. This concern was in the mind of policymakers when devising the policy, as reflected in the newly hired requirement to be able to claim the subsidy, along with the penalty for displacement. However, none of these requirements prevent a firm that would have hired young workers anyway from claiming the subsidy. The combination of high claims by firms and no effects on employment suggest that the claims of the subsidy are entirely driven by this behavior. This is consistent with estimates in Ebrahim et al. (2017) using firms' administrative data, which indicate that that firms that claimed the ETI increased hiring of young workers, but also hiring of older workers. Another interpretation of their results is that firms claimed the subsidy as they expanded and hired both young and old workers, independently of the ETI.

5.2 Robustness checks

I test the robustness of the results to some concerns that may confound these results. The first concern is that firms can claim the subsidy for periods of different lengths depending of the age of the worker. Firms can claim the subsidy for workers for at most two years, or until these workers become 30 years old. This implies that firms can claim the subsidy for two years only for workers that are hired at 28 or below, which translates to workers born in 1986 or afterwards. I use individuals born between 1986 and 1988 as my eligible group, and compare them to individuals born between 1981-1983.[42] From the perspective of the value of the subsidy for firms (i.e. ignoring productivity concerns), it is more worthy for firms to hire workers for whom they can claim the subsidy for a longer period. This would imply larger effects for these workers. Overall, the the effects on employment for younger workers that are eligible for two years (Table 7, column 2) are still not significant. Even when considering the point estimates and the largest effects that they imply, there is no clear pattern regarding differential effects on these younger workers. The effects estimated on private formal employment and private employment are consistent with larger effects on these young workers, whereas the opposite is true for overall employment. Overall, these results are not consistent with larger effects on workers for whom firms could claim the subsidy longer.

Second, I address the concern that there might be differential returns to education for the eligible and the ineligible cohorts. The system of education has changed over time in South Africa, along changes in the political landscape with the end of the apartheid regime, al-

[42]The latter group is chosen both to have a 3 year cohorts band for ineligible workers, just as for the workers that are in the eligible group. The cohorts in the middle, those born 1984 or 1985 are workers that are eligible for less than two years.

though the changes in education sometimes predated the political end of apartheid.[43] The sample in my analysis spans several cohorts that potentially were differentially affected by these changes. These individuals were as young as 6 and as old 15 in 1994, the year marking the end of the apartheid regime. To the extent that there have been changes in the quality of education, and thus changes in the returns to education, this may confound the estimates of the effects of the policy, as these are differential changes for young and old cohorts just as the change induced by the ETI. I address this concern by allowing the employment returns to education to differ by time (Table 7, column 3) or by cohorts (eligible, and ineligible; Table 7, column 4) in Equation 1. The results from these specifications are similar to the main results, which implies that different returns to education is not a concern for my estimates of the effects of the ETI.

Finally, I address a concern related to the effects of experience on employment outcomes. My main specification assumes the same experience profile for eligible and ineligible workers. It may be the case that worker eligible for the subsidy, who are on average younger, are on a different experience profile. I address this concern by allowing the experience proxy to enter Equation 1 differently for eligible and ineligible individuals. The results over the three employment outcomes (see table 7, column 5) are similar to the main results, suggesting that my results are not sensitive to differential experience profiles.

5.3 Heterogeneity of the effects

It is possible that a null effect on the average young worker masks a combination of positive and negative effects on different subgroups of youth. Therefore, I explore the heterogeneity of the effects of the policy along dimensions that are regularly considered in studies of South Africa labor market (Banerjee et al., 2008): gender (Table 8), race (Table 9), and education (Table 10). Women, Blacks (referred to simply as Africans) and individuals with low education have worse employment outcomes. Of the three, arguably only the difference in employment outcomes by level of education is potentially related to differences in the workers' productivity. [44]

[43] One characteristic of the system of education under the apartheid system was racial segregation of schools. School desegregation started as earlier as 1990 in the province of Gauteng, years before the end of apartheid regime.

[44] The theoretical predictions on the heterogeneous effects are not clear; i.e. for education for instance, whether individuals with low education or those with high education might gain more from a wage subsidy. To the extent that the difference in employment by education reflects only differences in productivity, the wage subsidy could induce firms to hire now those who otherwise were not profitable for these firms, i.e. individuals with low education. This would imply a higher effect on individuals with low level of education. Similarly, firms could hire more workers with high level of education as the wage subsidy potentially help them expand their business, especially since some of these workers with high education were previously

Across those three dimensions, the most puzzling result is along the gender dimension. Table 8 presents the results of Equation 1, estimated separately for men and women. The overall effect is a combination of negative effects on women's employment and positive effects on men's employment. The results suggest that private formal employment decreased by 3 percentage points for women; the most optimistic effect implied by these estimates is a reduction by .9 percentage points of the probability of being employed in a private formal firm, because of the policy. These results seem to be a combination of decreased employment for young female workers and increased employment for older female workers (see Figure 6). However, for most outcomes, the increase for older women seems to have started before the policy. Moreover, the trends in the outcomes seem rather erratic to support evidence of parallel trends, even though tests similar to the ones presented in the identification strategy section do not rule out that the two cohorts of women had similar trends before the policy. Given these concerns, the result that the wage subsidy decreased employment for young women should be read with caution.

The trends in outcomes look more similar for young and older male workers (see Figure 7); the estimates for the effects of the policy, though positive, are not statistically significant. Given the precision of the estimates, positive effects as small as 3 percentage points can be ruled out as the increase of overall employment for young men; but similarly, these estimates cannot rule out decreases in overall employment of 2 percentage points.

Overall, the analyses by the worker's level of education and race do not reveal any heterogeneity. Table 10 presents estimates of Equation 1 separately for individuals without and with a matric, the exit exam at the end of secondary school. These results are really similar and imply no differential effects with respect to the level of education. Table 9 presents the results by race, separately for White and Black South Africans, the two major race groups in South Africa. The point estimates for Whites are larger. In particular, the estimates imply an increase by 1.6 percentage points of the probability of private formal employment for eligible Whites, because of the policy. These results are however really imprecise and do not rule out null effects, nor the effects estimated for Black South Africans. In conclusion, there is no heterogeneity of the effects with respect to race.

Finally, I perform the analysis of the heterogeneity by main industry. As the data that I use for the analysis is a household survey, there is little information that would allow heterogeneity analysis with respect to firms characteristics. Respondents however give the

unemployed.

industry in which they are employed, and the information is coded to have the 1-digit main industry, per the Standard Industrial Classification of industries. I investigate whether employment has changed in some industries in ways that would suggest an impact of the policies for firms in those industries. I use the main industries as possible outcomes, and estimate for each of the industries, equations similar to 1. Table 11 shows the results of the estimation. Employment across all the industry has not differently changed for eligible workers compared to ineligible ones. This implies no differential effects of the policy by industry.

6 Conclusion

In this paper, I evaluate the impacts of the Employment Tax Incentive, a wage subsidy implemented by South Africa's government. The policy was designed to give incentives to firms for hiring young workers by lowering their labor costs. The wage subsidy was valuable to firms as it could amount to half the wage paid to a young worker. It was originally intended to last only two years, but it has been extended twice, and is currently scheduled to last until 2029. The basis of this renewal has been the higher than expected take-up of the subsidy by firms.

Using the ETI's age-eligibility restrictions and start time, I evaluate its impact on young workers by comparing individuals below and above the birth cohort cutoff for eligibility, before and after the start of implementation. My results show that young workers' employment did not increase as a consequence of the subsidy; I rule out effects as small as a 1 percentage point increase in overall employment.

Conceptually, the wage subsidy would have narrowed the productivity gap of young workers (due in part by the lack or mismatch of young worker's skills), and compensated employers for any initial training that these workers would have required. However, firms would have hired young workers only if the amount of the subsidy was large enough to offset this productivity gap as well as the training costs for employers. Firing regulations combined with the temporary nature of the subsidy (the subsidy could be claimed for up to two years for the same worker) would have further reduced the chances that firms indeed hired these young workers. Because the wage subsidy did not lead to improvements in employment of young workers, these other constraints (skills and labor regulations) should be considered for further policy actions, possibly in conjunction with the wage subsidy. Indeed, there have been some promising experiments on certifying skills (Carranza et al., 2019) and improving firms' information on labor regulations (Bertrand and Crepon, 2019). Even though these

experimental studies find improvements in labor market outcomes, my analysis suggests that we should exercise caution when generalizing the findings of these experiments. Indeed, the present analysis shows how the results of the wage subsidy implemented at national scale are different from those of the pilot study by Levinsohn et al. (2014).

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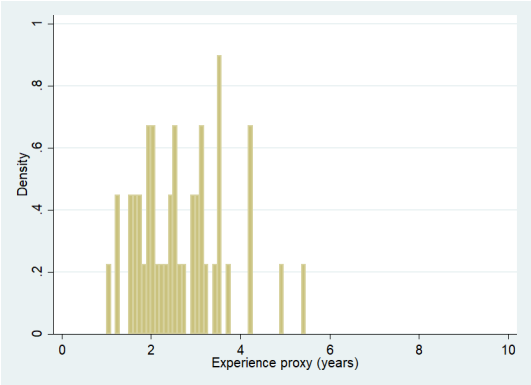
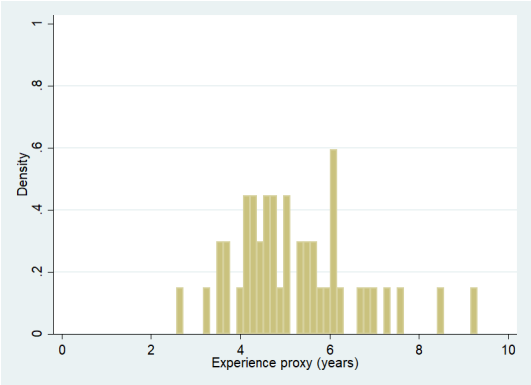
Figure 3: Evolution of outcomes over time

Figure 4: Distribution of the proxy before the policy

Proxy for experience in 2013Q3, just before the policy

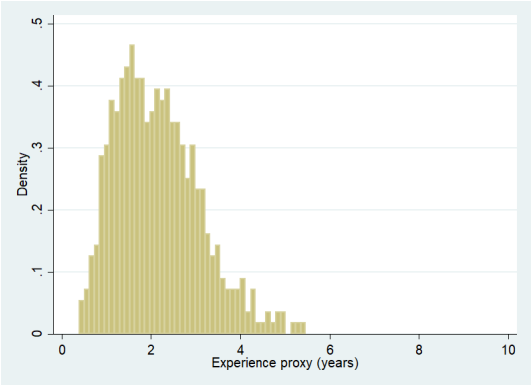


Eligible cohorts

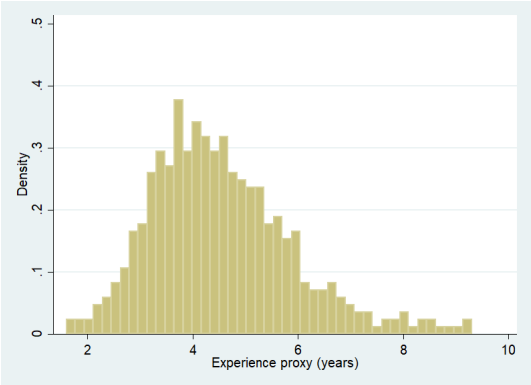


Ineligible cohorts

Proxy for experience in 2011-2013Q3, just before the policy

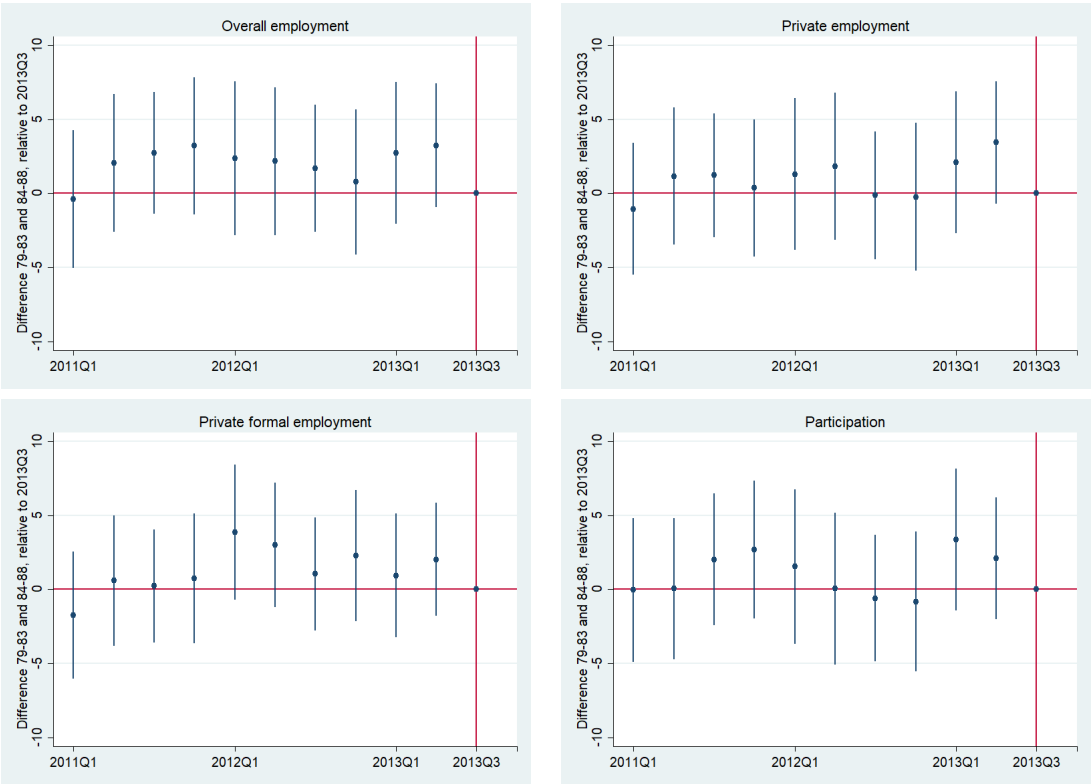


Eligible cohorts



Ineligible cohorts

Figure 5: Testing parallel trends prior to the policy



Notes: The graphs above present the result of estimating Equation 2. The coefficients plotted here are the coefficients of interaction between year-quarter dummies and the dummy for young cohorts. They represent the difference between the old and the young cohort, relative to the difference in $2013Q3$, the last quarter before the implementation of the ETI. The bars are the 95% confidence intervals.

Figure 6: Evolution of outcomes over time for women

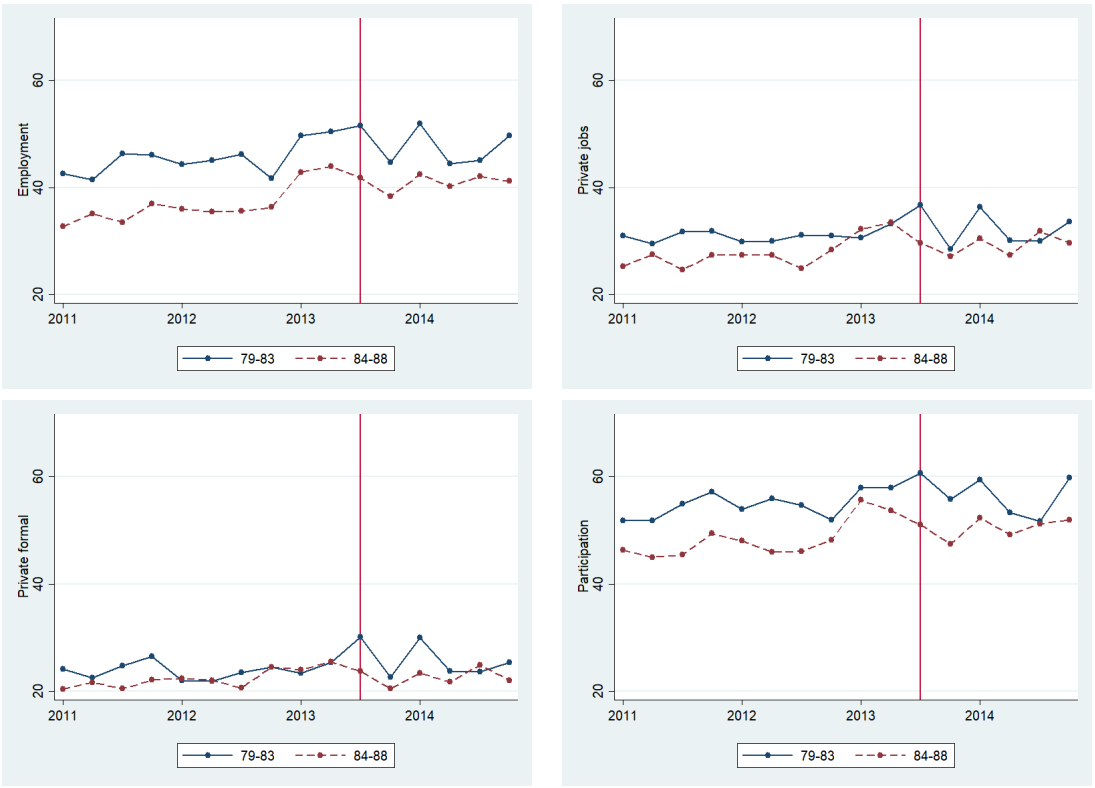


Figure 7: Evolution of outcomes over time for men

Table 1: Cohorts and eligibility

	Young cohort					Old cohort				
Age in 2013	25	26	27	28	29	30	31	32	33	34
Year of birth	88	87	86	85	84	83	82	81	80	79
Year										
2013Q1-2013Q3										
2013Q3-2014	X	X	X	X	S					

Notes: S denotes that some of the individuals are eligible. X denotes that all the individuals are eligible.

Table 2: Detail summary statistics of outcomes of interest and control variables

Periods	Before policy			After policy			
Cohort	Ineligible	Eligible	Diff (t-stat)	Ineligible	Eligible	Diff (t-stat)	DID (t-stat)
Private formal employment	32.22	27.20	-5.02 (-7.94)	33.10	29.40	-3.21 (-3.48)	0.67 (0.67)
Private employment	43.32	35.79	-7.53 (-12.21)	43.94	38.94	-4.62 (-4.50)	1.16 (1.11)
Employment	55.19	43.90	-11.29 (-18.17)	57.59	48.95	-7.76 (-8.22)	1.84 (1.91)
Participation	63.34	54.50	-8.85 (-14.78)	65.15	58.14	-6.39 (-7.02)	0.70 (0.74)
Gender is female	48.86	49.97	1.11 (1.96)	48.76	48.84	0.14 (0.14)	-1.14 (-1.26)
African	82.27	82.55	0.28 (0.52)	82.15	82.67	0.31 (0.41)	-0.36 (-0.43)
No schooling	1.57	0.88	-0.69 (-4.82)	1.63	1.26	-0.34 (-1.86)	0.39 (1.87)
Primary or less	9.57	7.87	-1.70 (-4.81)	9.04	7.13	-1.90 (-4.03)	-0.22 (-0.40)
Secondary	42.10	42.56	0.46 (0.68)	41.20	41.27	-0.19 (-0.21)	-0.48 (-0.45)
Matric	31.36	35.57	4.21 (6.86)	31.49	35.10	3.81 (4.11)	-0.33 (-0.32)
Post-secondary	14.46	12.42	-2.04 (-2.99)	15.79	14.39	-1.41 (-1.73)	0.36 (0.45)
Gauteng	28.62	26.38	-2.24 (-2.92)	29.07	26.55	-2.06 (-2.04)	-0.36 (-0.37)
Single	57.37	74.67	17.30 (26.64)	53.92	70.25	15.29 (14.20)	-0.91 (-0.90)
Living with a partner	14.44	11.87	-2.57 (-5.26)	14.93	12.60	-2.48 (-3.23)	-0.51 (-0.63)
Married	26.61	12.93	-13.68 (-21.39)	29.37	16.49	-11.77 (-13.46)	1.49 (1.74)
HH has an employed member	54.04	59.51	5.47 (9.27)	53.33	58.65	4.92 (5.26)	-0.40 (-0.38)
HH has a pensioner	19.92	21.48	1.57 (3.21)	20.10	20.60	0.26 (0.37)	-0.69 (-0.84)

Table 3: Placebo effect using 2013 first three quarters

	Overall employment		Private employment		Private formal employment	
	(1)	(2)	(3)	(4)	(5)	(6)
δ_1^P, placebo in 2013	1.174	0.193	2.283	1.323	0.519	-0.268
	(1.357)	(1.289)	(1.433)	(1.336)	(1.330)	(1.224)
Experience proxy	12.245	8.360	9.696	6.081	10.125	4.824
	(1.197)***	(0.861)***	(1.073)***	(0.821)***	(0.985)***	(0.747)***
Experience proxy square	-0.685	-0.551	-0.487	-0.422	-0.487	-0.339
	(0.113)***	(0.090)***	(0.104)***	(0.082)***	(0.098)***	(0.078)***
Controls	N	Y	N	Y	N	Y
R^2	0.04	0.16	0.02	0.13	0.03	0.14
N	39,407	39,407	39,407	39,407	39,407	39,407

Notes: Data are from the QLFS 2011-2013Q3. Sample: individuals from the incoming rotation group, born in 1979-1988. δ_1^P is the coefficient for the placebo test before the policy (see Equation 3). Standard errors in brackets are clustered at the stratum level. Regressions are weighted using the sample weights from the QLFS and estimated using a linear probability model. Controls are race (Blacks, Colored, Asians/Indians and Whites), gender, education (no education, some primary, primary completed, some secondary, secondary completed and post-secondary), marital status, the presence of an individual older than 65 and the presence of another individual who is working, all included in the regression equation as dummy variables.

Table 4: Impact of the ETI on youth employment: main results

	(1)	(2)	(3)	(4)
A: Overall employment				
δ_1	1.842	1.353	-0.206	-0.735
	(0.965)*	(0.891)	(0.984)	(0.893)
Experience proxy			12.120	8.549
			(1.118)***	(0.741)***
Experience proxy square			-0.716	-0.622
			(0.090)***	(0.071)***
Average for young cohort prior to the policy:		43.9%		
R^2	0.01	0.15	0.03	0.15
N	56,453	56,453	56,453	56,453
Controls	N	Y	N	Y
B: Private employment				
δ_1	1.157	0.752	-0.137	-0.765
	(1.046)	(0.968)	(1.067)	(0.979)
Experience proxy			9.524	5.854
			(1.002)***	(0.688)***
Experience proxy square			-0.507	-0.441
			(0.085)***	(0.064)***
Average for young cohort prior to the policy:		35.8%		
Controls	N	Y	N	Y
R^2	0.01	0.13	0.02	0.13
N	56,453	56,453	56,453	56,453
C: Private formal employment				
δ_1	0.672	0.446	-0.485	-0.782
	(1.005)	(0.905)	(1.005)	(0.927)
Experience proxy			9.860	4.390
			(0.954)***	(0.638)***
Experience proxy square			-0.492	-0.347
			(0.084)***	(0.060)***
Average for young cohort prior to the policy:		27.2%		
R^2	0.00	0.14	0.02	0.14
N	56,453	56,453	56,453	56,453
Controls	N	Y	N	Y

Notes: Data are from the QLFS 2011-2014. Sample: individuals from the incoming rotation group, born in 1979-1988. δ_1 is the difference-in-difference estimator (see Equation 1). Standard errors in brackets are clustered at the stratum level. Regressions are weighted using the sample from the QLFS and estimated using a linear probability model. Controls are race (Blacks, Colored, Asians/Indians and Whites), gender, education (no education, some primary, primary completed, some secondary, secondary completed and post-secondary), marital status, the presence of an individual older than 65 and the presence of another individual who is working, all included in the regression equation as dummy variables.

Table 5: Impact of the ETI on youth employment: aggregate level regressions

	Overall employment		Private employment		Private formal employment	
	(1)	(2)	(3)	(4)	(5)	(6)
δ_1	-0.206	-0.858	-0.137	-0.934	-0.485	-0.857
	(1.061)	(0.822)	(1.051)	(0.894)	(1.212)	(0.913)
Experience proxy	12.120	8.316	9.524	5.627	9.860	4.505
	(1.033)***	(0.804)***	(1.053)***	(0.672)***	(1.392)***	(0.629)***
Experience proxy square	-0.716	-0.609	-0.507	-0.435	-0.492	-0.355
	(0.095)***	(0.068)***	(0.100)***	(0.061)***	(0.134)***	(0.058)***
Controls	N	Y	N	Y	N	Y
R^2	0.46	0.61	0.32	0.54	0.31	0.63
N	1,440	1,440	1,440	1,440	1,440	1,440

Notes: Data are from the QLFS 2011-2014. Sample: individuals from the incoming rotation group, born in 1979-1988. Data are aggregated at the birth cohort-province level. δ_1 is the difference-in-difference estimator (see Equation 1). Standard errors in brackets are clustered at the stratum level. Regressions are weighted using the sample weights from the QLFS and estimated using a linear probability model. Controls include race (Blacks, Colored, Indians, Whites), gender, education (no education, some primary, primary completed, some secondary, secondary completed, post-secondary), marital status, the presence of an individual older than 65 and the presence of another individual who is working, all included in the regression equation as dummy variables.

Table 6: Effects of the ETI on labor force participation

	(1)	(2)	(3)	(4)
δ_1	0.701	0.322	-0.502	-0.952
	(0.947)	(0.904)	(0.995)	(0.926)
Experience proxy			5.601	6.330
			(1.004)***	(0.772)***
Experience proxy square			-0.377	-0.412
			(0.084)***	(0.073)***
Controls	N	Y	N	Y
R^2	0.01	0.09	0.01	0.09
N	56,453	56,453	56,453	56,453

Notes: Data are from the QLFS 2011-2014. Sample: individuals born in 1979-1988 from the incoming rotation group. δ_1 is the difference-in-difference estimator (see equation 1). Standard errors in brackets are clustered at the stratum level. Regressions are weighted using the sample weights from the QLFS and estimated using a linear probability model. Controls are race (Blacks, Colored, Asian/Indians and Whites), gender, education (no education, some primary, primary completed, some secondary, secondary completed, post-secondary), marital status, the presence of an individual older than 65 and the presence of another individual who is working, all included in the regression equation as dummy variables.

Table 7: Robustness checks

	Main results	Young workers eligible for 2 years	Interacting education with		Different experience functions
			time	cohort	
	(1)	(2)	(3)	(4)	(5)
A: Employment					
δ_1	-0.735	-1.248	-0.835	-0.735	-0.694
	(0.893)	(1.067)	(0.896)	(0.893)	(0.908)
R^2	0.15	0.15	0.15	0.15	0.15
B: Private employment					
δ_1	-0.765	-0.172	-0.868	-0.777	-0.633
	(0.979)	(1.224)	(0.978)	(0.978)	(0.981)
R^2	0.13	0.12	0.13	0.13	0.13
C: Private formal employment					
δ_1	-0.782	-0.501	-0.862	-0.785	-0.530
	(0.927)	(1.119)	(0.928)	(0.926)	(0.956)
R^2	0.14	0.14	0.14	0.14	0.14
N	56,453	40,596	56,453	56,453	56,453

Notes: Data are from the QLFS 2011-2014. Sample: individuals born in 1979-1988, from the incoming rotation group. Column 1 presents the main results, as obtained in table 4, column 4. Column 2 restricts the analysis on workers born between 1986-1988 and between 1981-1983, allowing to test the effects on young workers eligible for two years of subsidies. Column 3 presents results of specifications that include interaction terms between education and year dummies, and column 4 includes interaction terms between education and a dummy for eligible cohorts. Column 5 allows for different effects of experience for eligible and ineligible cohorts. δ_1 is the difference-in-difference estimator (see Equation 1). Standard errors in brackets are clustered at the stratum level. Regressions are weighted using the sample weights from the QLFS and estimated using a linear probability model. Controls include race (Blacks, Colored, Indians, Whites), gender, marital status, the presence of an individual older than 65 and the presence of another individual who is working, all included in the regression equation as dummy variables.

Table 8: Heterogeneous effects by gender

	Overall employment		Private employment		Private formal employment	
	(1)	(2)	(3)	(4)	(5)	(6)
A: Men						
δ_1	0.838	0.425	1.382	1.025	1.795	1.729
	(1.403)	(1.292)	(1.378)	(1.300)	(1.352)	(1.311)
Experience proxy	11.615	7.852	8.872	4.944	9.664	3.548
	(1.341)***	(1.015)***	(1.332)***	(1.070)***	(1.280)***	(1.067)***
Experience proxy square	-0.684	-0.595	-0.451	-0.363	-0.456	-0.265
	(0.113)***	(0.094)***	(0.116)***	(0.096)***	(0.119)***	(0.100)***
Controls	N	Y	N	Y	N	Y
R^2	0.04	0.17	0.02	0.13	0.02	0.13
N	26,836	26,836	26,836	26,836	26,836	26,836
B: Women						
δ_1	-1.509	-1.857	-1.960	-2.516	-3.031	-3.379
	(1.455)	(1.326)	(1.509)	(1.385)*	(1.429)**	(1.267)***
Experience proxy	12.398	9.015	9.886	6.565	9.855	5.119
	(1.232)***	(0.979)***	(1.062)***	(0.855)***	(1.036)***	(0.769)***
Experience proxy square	-0.724	-0.659	-0.532	-0.529	-0.508	-0.437
	(0.105)***	(0.097)***	(0.095)***	(0.083)***	(0.094)***	(0.074)***
Controls	N	Y	N	Y	N	Y
R^2	0.03	0.14	0.02	0.10	0.02	0.14
N	29,617	29,617	29,617	29,617	29,617	29,617

Notes: Data are from the QLFS 2011-2014. Sample: individuals in the incoming rotation group, born in 1979-1988. δ_1 is the difference-in-difference estimator (see Equation 1). Standard errors in brackets are clustered at the stratum level. Regressions are weighted using the sample weights attached to the QLFS and estimated using a linear probability model. Controls include race, education, marital status, the presence of an individual older than 65 and the presence of another individual who is working, all included in the regression equation as dummy variables.

Table 9: Heterogeneous effects by race

	Overall employment		Private employment		Private formal employment	
	(1)	(2)	(3)	(4)	(5)	(6)
A: Blacks						
δ_1	-0.490	-0.925	-0.483	-1.004	-0.891	-1.133
	(1.099)	(1.023)	(1.193)	(1.123)	(1.117)	(1.046)
Experience proxy	10.093	7.966	7.288	5.241	7.614	3.798
	(1.212)***	(0.892)***	(1.028)***	(0.778)***	(0.887)***	(0.681)***
Experience proxy square	-0.580	-0.535	-0.384	-0.374	-0.374	-0.297
	(0.110)***	(0.097)***	(0.096)***	(0.083)***	(0.086)***	(0.073)***
Controls	N	Y	N	Y	N	Y
R^2	0.03	0.13	0.02	0.10	0.02	0.10
N	45,957	45,957	45,957	45,957	45,957	45,957
B: Whites						
δ_1	0.393	-0.200	-0.061	-0.672	2.293	1.565
	(4.180)	(3.811)	(4.062)	(4.101)	(4.083)	(4.006)
Experience proxy	9.848	9.835	9.134	9.409	6.598	6.739
	(2.210)***	(2.372)***	(2.531)***	(2.666)***	(2.528)**	(2.771)**
Experience proxy square	-0.765	-0.760	-0.654	-0.682	-0.426	-0.436
	(0.205)***	(0.203)***	(0.220)***	(0.220)***	(0.221)*	(0.231)*
Controls	N	Y	N	Y	N	Y
R^2	0.02	0.12	0.01	0.08	0.01	0.07
N	3,221	3,221	3,221	3,221	3,221	3,221

Notes: Data from the QLFS 2011-2014. Sample: individuals born in 1979-1988, from the incoming rotation group. δ_1 is the difference-in-difference estimator (see equation 1). Standard errors in brackets are clustered at the stratum level. Regressions are weighted using the sample from the QLFS and estimated using a linear probability model. Controls include gender, education (no education, some primary, primary completed, some secondary, secondary completed, post-secondary), marital status, the presence of an individual older than 65 and the presence of another individual who is working, all included in the regression equation as dummy variables.

Table 10: Heterogeneous effects by level of education

	Overall employment		Private employment		Private formal employment	
	(1)	(2)	(3)	(4)	(5)	(6)
A: Individuals with less than a matric						
δ_1	-0.209	-0.769	-0.018	-0.812	-0.515	-0.954
	(1.454)	(1.359)	(1.425)	(1.300)	(1.194)	(1.118)
Experience proxy	7.957	6.409	5.987	4.203	6.076	2.421
	(1.260)***	(0.988)***	(1.125)***	(0.893)***	(0.827)***	(0.736)***
Experience proxy square	-0.403	-0.420	-0.242	-0.282	-0.236	-0.180
	(0.109)***	(0.096)***	(0.101)**	(0.086)***	(0.083)***	(0.075)**
Controls	N	Y	N	Y	N	Y
R^2	0.02	0.11	0.01	0.13	0.02	0.10
N	30,020	30,020	30,020	30,020	30,020	30,020
B: Individuals with a matric or more						
δ_1	-0.454	-0.852	-0.451	-0.905	-0.531	-0.850
	(1.450)	(1.321)	(1.597)	(1.490)	(1.563)	(1.456)
Experience proxy	14.879	10.790	12.002	7.478	12.098	6.397
	(1.242)***	(0.984)***	(1.291)***	(1.003)***	(1.325)***	(1.028)***
Experience proxy square	-1.000	-0.839	-0.726	-0.582	-0.697	-0.512
	(0.108)***	(0.091)***	(0.117)***	(0.096)***	(0.121)***	(0.098)***
Controls	N	Y	N	Y	N	Y
R^2	0.04	0.13	0.03	0.11	0.02	0.11
N	25,997	25,997	25,997	25,997	25,997	25,997

Notes: Data from the QLFS 2011-2014. Sample: individuals born in 1979-1988, from the incoming rotation group. δ_1 is the difference-in-difference estimator (see equation 1). Standard errors in brackets are clustered at the stratum level. Regressions are weighted using the sample from the QLFS and estimated using a linear probability model. Controls include race (Blacks, Colored, Asians/Indians, Whites), gender, marital status, the presence of an individual older than 65 and the presence of another individual who is working, all included in the regression equation as dummy variables.

Table 11: Heterogeneous effects by industry

Main industry	Trade	Community and social	Financial	Manufacturing	Construction
δ_1	-0.593	0.705	0.337	-0.619	-0.688
	(0.668)	(0.628)	(0.531)	(0.489)	(0.447)
R^2	0.02	0.08	0.06	0.03	0.04
N	56,453	56,453	56,453	56,453	56,453
Eligible cohorts, before policy	11.5	7.4	6.5	5.5	3.8
	Private households	Transport	Agriculture	Mining	
δ_1	-0.021	0.095	0.307	-0.031	
	(0.352)	(0.370)	(0.307)	(0.249)	
R^2	0.03	0.02	0.04	0.05	
N	56,453	56,453	56,453	56,453	
Eligible cohorts, before policy	2.3	2.5	2.0	1.0	

Notes: Data are from the QLFS 2011-2014. Sample: individuals from the incoming rotation group, born in 1979-1988. δ_1 is the difference-in-difference estimator (see Equation 1). For each column, the dependent variable is a dummy variable which is 1 if when an individual is employed in the mentioned industry, and zero otherwise. Regressions are estimated using a linear probability model weighted using the sample from the QLFS. Standard errors in brackets are clustered at the stratum level. Controls are race (Blacks, Colored, Asians/Indians and Whites), gender, education (no education, some primary, primary completed, some secondary, secondary completed and post-secondary), marital status, the presence of an individual older than 65 and the presence of another individual who is working, all included in the regression equation as dummy variables.