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The effect of wage subsidies on job retention in a developing country

Evidence from South Africa

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Abstract: Wage subsidies served as a dominant labour market policy response around the world to mitigate job losses in response to the COVID-19 pandemic. However, no causal evidence of their effects exists for developing countries. We use unique panel labour force survey data and exploit a temporary institutional eligibility detail to estimate the causal effects of such a policy—the Temporary Employer/Employee Relief Scheme (TERS)—on job retention among formal private sector employees in South Africa. Using a difference-in-differences design, within the context of an economy with one of the highest unemployment rates in the world, we find that the policy increased the probability of remaining employed in the short term by 15.6 percentage points. Our findings imply that the policy saved 2.7 million jobs during April and May 2020 at a monthly cost of ZAR13,195 (US$1,851 PPP) per job saved. While this cost is large relative to the wage costs of jobs supported by the policy, it compares favourably to more developed country contexts. However, two thirds of the recipients were inframarginal and would have remained employed anyway in the policy's absence, arguably due to prioritization of rapid disbursement of relief over accurate targeting. We additionally examine heterogenous effects by subsidy intensity and find that effects are positive but marginally regressively distributed across the subsidy distribution.

Key words: COVID-19, wage subsidy, job retention, job loss, South Africa, labour market, TERS

JEL classification: J08, J38

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Data availability: The data underlying this article are available in the DataFirst repository, at:

- https://doi.org/10.25828/5sny-n338;
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- https://doi.org/10.25828/KG4N-KZ59;
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However, the data specifically related to wages underlying this article were provided by Statistics South Africa by permission. Data will be shared on request by the corresponding author with permission of Statistics South Africa.

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1 Introduction

Wage subsidies have served as one of the primary policies used by governments around the world to mitigate job losses in response to the COVID-19 pandemic (Bennedsen et al. 2020; ILO 2020; IMF 2021; Gentilini et al. 2022; Köhler and Hill 2022). While these active labour market policies vary in design, by subsidizing worker incomes and firm liquidity these policies seek to help employers retain workers and avoid the potentially costly process of hiring and training new workers as economic activity recovers (Giupponi and Landais 2020; Keenan and Lydon 2020; OECD 2020), as well as helping workers avoid adverse labour market scarring effects associated with periods of unemployment (OECD 2020). Globally, by January 2022, 60 per cent of countries had introduced some form of wage subsidy (Gentilini et al. 2022). Indeed, although existing estimates on the employment effects of wage subsidies in non-crisis periods tend to be modest at best, evidence suggests they may be particularly useful during periods of large, transient shocks (McKenzie 2017).

While several studies which estimate the causal job retention effects of wage subsidies during the pandemic exist for developed country contexts (see Chetty et al. 2020; Hubbard and Strain 2020; Bishop and Day 2020; Dalton 2021; Autor et al. 2022; Granja et al. 2022; Kuchakov and Skougarevskiy 2023; Smart et al. 2023), to our knowledge no such evidence exists for developing country contexts which tend to be characterized by markedly different labour markets. In particular, such policy was arguably one of the most important labour market interventions implemented during the pandemic in those developing countries characterized by high rates of unemployment where workers have low bargaining power, such as South Africa.

Since its inception in April 2020, South Africa's Temporary Employer/Employee Relief Scheme (TERS)—a wage subsidy which supported workers in firms who either fully or partially closed their operations due to the pandemic—benefited 5.7 million workers (61–70 per cent[1] of the formal, private employed population in 2020) at a cost of ZAR64 billion (approximately US$9 billion in purchasing power parity (PPP) terms) by its termination (Nxesi 2022). Despite some existing descriptive literature at the time of writing, no causal evidence exists on whether the TERS was successful in achieving its primary objective of mitigating job losses. To the authors' knowledge, the only study which has investigated this relationship is Köhler and Hill (2022) who find a significant positive association between receipt of the subsidy and job retention during the beginning of the pandemic. While their finding provides suggestive evidence towards the effectiveness of the policy, the correlational nature of their empirical approach prohibits them from identifying a causal effect and concluding whether the policy saved jobs.

In this paper, we provide the first estimates of the causal effects of wage subsidies on job retention in a developing country context during the COVID-19 pandemic. Using South Africa as a useful case study given its high-unemployment context, we make use of unique panel labour force survey data and exploit a temporary institutional eligibility criterion during the beginning of the TERS policy which allows for a unique opportunity to adopt a canonical difference-in-differences (DiD) design. Our approach compares the job retention probabilities of TERS-eligible and -ineligible employees in the formal private sector (who constitute the majority of workers in the country) from before to after the policy was introduced, covering the first half of 2020. Additionally, because TERS subsidies were a function of workers' wages (discussed in more detail in Section 2),

[1] Calculated using microdata from Statistics South Africa's Quarterly Labour Force Survey for all four quarters of 2020.

in addition to estimating average treatment effects, we test for the existence of effect heterogeneity across the imputed subsidy distribution in both absolute and relative terms; that is, with respect to variation in subsidy amounts and subsidization rates, respectively. Such a heterogeneity analysis is useful considering it sheds light on the effect of changes in subsidy rates as opposed to the effect of subsidy receipt alone regardless of receipt intensity or 'bite'. Furthermore, because the calculation of TERS subsidies took employer contributions towards workers' wages into account, and these contributions are not observed in the data, we test the sensitivity of our results to varying explicit employer contribution assumptions and provide a set of bounded estimates of the effect of the policy across the subsidy distribution.

The rest of this paper is structured as follows. In Section 2 we provide an overview of South Africa's TERS policy. Thereafter, we discuss the data in Section 3 and our identification strategy and model specifications in Section 4. We present our main results in Section 5 and robustness test results in Section 6. In Section 7 we conclude.

2 Background to South Africa's Temporary Employer/Employee Relief Scheme (TERS)

The South African government introduced the TERS policy on 25 March 2020 to provide wage support to employers and mitigate the extent of job loss expected to occur due to the pandemic and national lockdown. The initial lockdown was in place from 27 March 2020 until the end of April 2020 and was relatively stringent by international standards (Gustaffson 2020), entailing restricted workplace policy, school closures, a curfew, and international and domestic travel controls. From May 2020, the government adopted a five-level risk-adjusted strategy whereby the stringency of lockdown regulations varied according to the severity of contagion. In April 2022, after approximately 750 days of being in place, the country's lockdown policy was repealed. A description of the country's lockdown regulations is contained in Köhler et al. (2023).

Administered by the Unemployment Insurance Fund (UIF),[2] the TERS served as a wage subsidy targeting workers who remained employed but had suffered income loss as a result of a full or partial closure of their employer's operations due to the pandemic and associated regulations for a period of three months or less (Department of Employment and Labour 2020a, 2020b).[3] Initially, in order to minimize the volume of applications received, employers or the relevant bargaining council applied and distributed any benefits on behalf of the eligible worker, rather than the worker applying themselves. Employers were then liable to pay workers the relevant benefit within two days and submit proof of payment to the UIF within five days.[4] Nearly all subsidies (approximately 96 per cent) were paid within 30 days of application (Auditor-General South Africa 2020a, 2020b). Given the existing structures, databases, and legislation of the UIF, the government was able to implement the policy both timeously and effectively without the need for a special registration

[2] The UIF is a source of social insurance in South Africa which provides short-term income relief to the formally employed in the event of unemployment, maternity, adoption and parental leave, or illness.

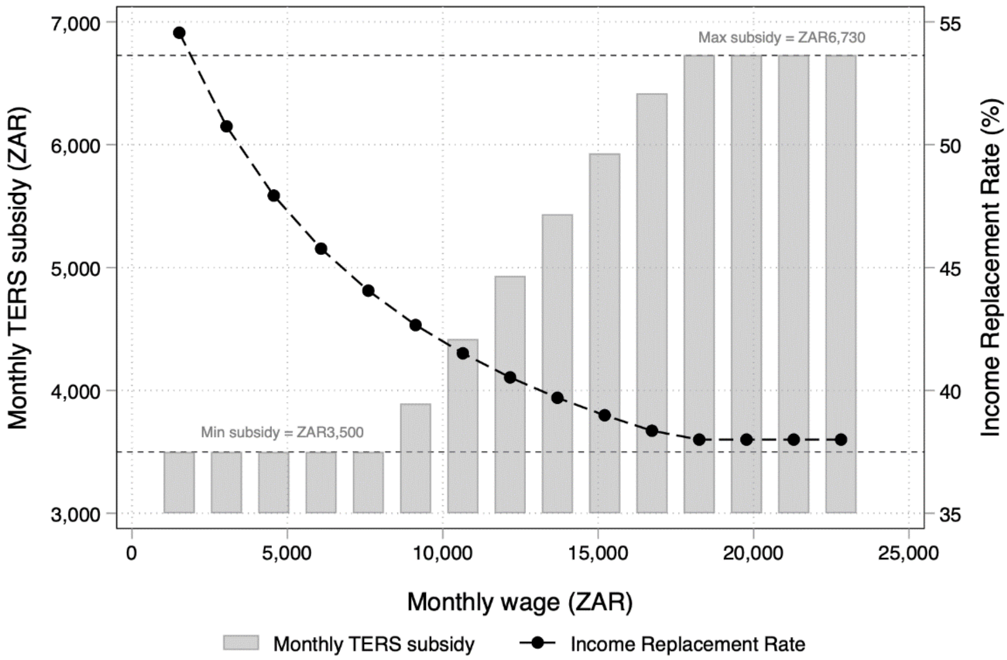
[3] Income loss may have been because workers were not able to work at all, worked fewer hours, or experienced temporary wage reductions related to the operational requirements of their employer due to the pandemic (Department of Employment and Labour 2020a, 2020b).

[4] An exception to this was when an employer employs fewer than ten workers, in which case the UIF paid these workers directly. This changed in May 2020 when direct payments into workers' bank accounts were implemented, although application for these benefits still needed to be received from the employer (Auditor-General South Africa 2020a).

drive for new beneficiaries (Gronbach et al. 2022). The TERS was subject to various extensions and amendments throughout the remainder of 2020 and 2021, ultimately terminating with a final claim period that ended on in July 2021. The reader is referred to Köhler and Hill (2022) for a detailed description of the evolution of the policy.

Preliminary TERS subsidy amounts are calculated according to the usual unemployment insurance benefit formula as laid out in Schedule 2 of South Africa's Unemployment Insurance Act (No. 63 of 2001) as amended by the Unemployment Insurance Amendment Act (No. 32 of 2003).[5] These preliminary subsidy amounts were adjusted according to an explicit upper-bound TERS benefit of ZAR6,730 (US$944 PPP) and an assurance that no individual had a *take-home pay* (after considering any contributions made towards wages by the employer) of less than the national minimum wage of ZAR3,500 (US$491 PPP) per month. Figure 1 presents a visual summary of this calculation in the simple case where there are no employer contributions.

Figure 1: Simulation of the calculation of TERS subsidy amounts



Note: for illustration purposes, calculations of TERS subsidy values presented here assume zero employer contributions.

Source: replicated with permission from Köhler and Hill (2022).

As mentioned above, preliminary calculated subsidy amounts are a function of a worker's wage; however, they were adjusted according to certain additional clauses specified in the consolidated directive. Firstly, as specified above, TERS benefit payments were bounded above by a value of ZAR6,730 (US$944 PPP). This is the equivalent of the subsidy value calculated on an upper-bound monthly wage threshold of ZAR17,712 (US$2,484 PPP). Thus, all workers who earned above this monthly wage threshold—or, equivalently, had a monthly TERS benefit calculated as being in

[5] The formula is as follows: $subsidy_i = IRR_i \times w_i = \left\{29.2 + \frac{7\,173.92}{232.92 + w_i}\right\} w_i$ where IRR_i is worker i's income replacement rate and w_i their daily wage.

excess of ZAR6,730 (US$944 PPP)—would have simply had their TERS benefit capped at this upper-bound value.

Secondly, the TERS aimed to ensure that all individuals had a take-home pay equivalent to the national monthly minimum wage of at least ZAR3,500 (US$491 PPP). Thus, the TERS subsidy would 'top-up' the wages of any workers where the sum of any employer contributions and calculated TERS benefits fell below the ZAR3,500 (US$491 PPP) threshold (Department of Employment and Labour 2020b).[6] For example, if an individual were earning ZAR3,040 (US$426 PPP) per month, their calculated monthly benefit would be ZAR1,544 (US$217 PPP). In a case of zero employer contributions, the TERS would top this wage up, and the actual benefit paid out by the TERS would be ZAR3,500 (US$491 PPP). However, if this worker's employer contributed ZAR1,000 (US$140 PPP) towards their monthly wage, then the TERS would only make up the difference to ensure the individual had a take-home pay of ZAR3,500 (US$426 PPP)—i.e. TERS would contribute ZAR2,500 (US$351 PPP) as a benefit.

Due to the progressive nature of the formula and lower and upper bounds, lower-wage workers received larger benefits in relative terms while higher-wage earners received larger benefits in absolute terms. This also meant that some low-wage workers received subsidies greater than their usual wage. Considering this aspect of the policy design, it is plausible that the TERS may have had heterogeneous effects across the subsidy distribution. We investigate this hypothesis in our analysis to follow.

Initially, the policy was restrictive in its coverage, which is central to our identification strategy. For its first two months, eligibility was restricted to UIF-contributing workers. This included most of the employed population (8.5 million, or 52 per cent of workers as of the first quarter of 2020)[7] given that most workers are legally obligated to be registered and contribute to the UIF.[8] Informal sector and UIF non-contributing formal sector workers were thus excluded. Following legal challenges, eligibility was expanded from the end of May 2020 to include any worker who could prove an existing employment relationship, whether they were a UIF-contributor or not.

3 Data

We make use of nationally representative, individual-level labour force survey data from Statistics South Africa's (StatsSA) Quarterly Labour Force Survey (QLFS) for the first two quarters of 2020 (henceforth 2020Q1 and 2020Q2). The survey contains detailed information on a wide array of demographic and labour market activities for individuals aged 15 years and older. More information on the survey's design is available via StatsSA (2008). Throughout our analysis, we employ the sampling weights available in the data and account for the complex survey design.

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[6] The TERS could only cover the cost of salaries and no other firm expense. Employers were permitted to supplement the TERS support, but not if this resulted in workers earning more than 100 per cent of their wage.

[7] Own calculations using weighted microdata from Statistics South Africa's Quarterly Labour Force Survey for 2020Q1.

[8] These exceptions include workers who are employed with an employer for less than 24 hours per month, those who work for national or provincial government, foreign workers on contract, and workers who only earn a commission. This also includes workers who do not need to be registered for income tax purposes, such as those who earn below the tax threshold, and those who are not voluntarily registered.

Prior to the pandemic the QLFS sample comprised nearly 70,000 individuals living in approximately 30,000 dwelling units surveyed through face-to-face interviews. Following the onset of the pandemic at the end of March 2020, StatsSA suspended face-to-face data collection and changed the survey mode to computer-assisted telephone interviewing (CATI). To facilitate this, and unlike in previous quarters, the sample that was surveyed in 2020Q1 and for which StatsSA had valid contact numbers was surveyed again in 2020Q2. The result was that the 2020Q2 data included 71 per cent of the 2020Q1 sample. To address sample selection, StatsSA adjusted the calibrated survey weights using a bias-adjustment procedure which relied on a range of observable characteristics. Thus, this sampling decision resulted in the survey temporarily becoming an unbalanced longitudinal survey – a unique scenario in its history. This aspect of the data is key to our identification strategy and ability to measure job retention, detailed below. The reader is referred to StatsSA (2020a) and Köhler et al. (2023) for a more detailed description on these pandemic-induced changes to the survey.

Because both treatment assignment in our identification strategy and our dependent variable of interest rely on observing individuals in both periods, we restrict our sample to the balanced panel of individuals, and more specifically, to those employed in the baseline period (2020Q1) but any labour market status in 2020Q2.[9] We further restrict the sample to those of working age (15–64 years) in 2020Q1. This procedure results in a balanced panel sample of 24,475 unique individuals observed twice, compared to the cross-sectional sample of nearly 42,000 working-age individuals in the baseline period. To determine whether our panel sample remains representative of the larger population, we estimate and test for differences in the weighted means of several observable covariates between the cross-sectional and panel sample at baseline. We present these estimates in Table 1. Our balanced panel sample appears to remain representative of the broader South African population. In columns (1) to (3) we observe that, relative to the baseline cross-sectional sample, individuals in our panel are slightly more likely to be older, female, self-reported African/Black, and have a tertiary-level education, and they are less likely to live in an urban area. Although these differences are all statistically significant at the 1 per cent level, their magnitudes are all relatively close to zero.

[9] To identify the balanced panel sample, we make use of household and person identifiers in the data as well as data on age, gender, and self-reported racial population group to ensure that we observe the same individual over time. We allow for a one-year difference in age between quarters in either direction to account for ageing or possible measurement error. We omit all observations that exhibit inconsistency in any of these characteristics. The anonymity of the data prohibits us from accessing other identifying variables of respondents such as names, surnames, and birth dates.

Table 1: Covariate balance table at baseline, by sample

	Whole samples			Employed samples		
	Cross-sectional (n = 41 827)	Balanced panel (n = 24 475)	Diff.	Cross-sectional (n = 17 044)	Balanced panel (n = 10 082)	Diff.
	(1)	(2)	(3)	(4)	(5)	(6)
Age (years)	35.040	35.328	-0.287***	39.465	40.206	-0.741***
	(0.070)	(0.086)	(0.054)	(0.095)	(0.119)	(0.081)
Female	0.505	0.515	-0.011***	0.442	0.459	-0.018***
	(0.002)	(0.003)	(0.002)	(0.004)	(0.005)	(0.003)
African/Black	0.808	0.829	-0.020***	0.752	0.768	-0.016***
	(0.005)	(0.006)	(0.004)	(0.006)	(0.008)	(0.005)
Urban	0.680	0.658	0.022***	0.763	0.752	0.011**
	(0.005)	(0.007)	(0.005)	(0.005)	(0.007)	(0.005)
Married	0.350	0.353	-0.004	0.506	0.522	-0.016***
	(0.004)	(0.004)	(0.003)	(0.005)	(0.007)	(0.004)
Education:						
Primary or less	0.134	0.127	0.007***	0.108	0.103	0.005**
	(0.002)	(0.003)	(0.002)	(0.003)	(0.004)	(0.002)
Incomplete secondary	0.433	0.433	0.000	0.336	0.324	0.012***
	(0.003)	(0.004)	(0.002)	(0.005)	(0.006)	(0.004)
Complete secondary	0.306	0.308	-0.001	0.338	0.344	-0.006
	(0.003)	(0.004)	(0.002)	(0.005)	(0.006)	(0.004)
Tertiary	0.127	0.132	-0.005***	0.218	0.229	-0.011***
	(0.003)	(0.003)	(0.002)	(0.005)	(0.006)	(0.004)

Note: this table presents estimates of mean values for observable covariates for the cross-sectional and balanced panel sample in the baseline period (2020Q1) accompanied by difference estimates. Samples restricted to the working-age population (15–64 years). All estimates are weighted using sampling weights. Standard errors presented in parentheses and take account of the complex survey design. The magnitude and statistical significance of a given difference are estimated using t-tests. *** p < 0.01, ** p < 0.05, * p < 0.10.

Source: authors' calculations based on QLFS 2020Q1 (StatsSA 2020b).

Given that job retention serves as our outcome variable of interest, it is useful to consider the representativity of employed individuals in our panel relative to the equivalent group in the cross-sectional sample at baseline. We present these estimates in columns (4) and (5). Compared to the means in columns (1) and (2), employed individuals in both the panel and cross-sectional samples are more likely to be older, live in urban areas, be married, have higher levels of formal education, and less likely to be self-reported African/Black or female. These are well-documented characteristics of the employed population in the South African labour market (Bhorat et al. 2015; Zizzamia 2020; Ranchhod and Daniels 2021). Comparing employed individuals across the panel and cross-sectional samples, as before we find that individuals in the former are more likely to be older, female, self-reported African/Black, and have a tertiary-level education, and less likely to live in an urban area—which is consistent with the observed differences in column (3)—and additionally those in the panel are more likely to be married. Again, however, although these differences are statistically significant their magnitudes are all close to zero. We are thus relatively confident that our balanced panel sample of individuals employed at baseline remains representative of the broader population.

As noted in Sections 1 and 2, we examine heterogeneous effects on job retention across the subsidy distribution. Because subsidy amounts are not observed in the data but are a function of wages, to do so we make use of raw, unimputed QLFS wage data not available in the public domain but provided to us by StatsSA. Our use of this data is important given the recent debate surrounding the quality of the public release QLFS wage data (which includes imputations) that has played out among labour market researchers in South Africa (Wittenberg 2017; Kerr and Wittenberg 2019a; Bhorat et al. 2021; Kerr 2021; Kerr and Wittenberg 2021).[10] Several studies have highlighted how the use of the public release wage data produces implausible results, however the underlying unimputed data are more reliable (Kerr and Wittenberg 2019a; Kerr 2021; Kerr and Wittenberg 2021). Our use of the unimputed data overcomes these data quality issues. We follow Wittenberg (2017) and Kerr and Wittenberg (2019b) to prepare the data for analysis by (i) identifying and omitting outliers and (ii) addressing missing data. For (i), we employ a studentized regression residual approach and recode the wage values of outliers as missing.[11] This process, which has the advantages of identifying outliers on both tails of the wage distribution, affected just five observations. For (ii), we use multiple imputation (MI) which is essentially an iterative form of stochastic imputation, but instead of imputing a single value through regression, MI uses the distribution of the empirical data to estimate multiple values which are then combined for inference using Rubin's (1987) rules to appropriately reflect the uncertainty associated with the imputation process around the true value.[12]

4 Identification strategy

The ideal approach to identify the average causal effect of the TERS would be a simple comparison of mean job retention probabilities between recipient and non-recipient workers in a context where all workers were randomly selected into receipt. However, such a comparison in reality would yield an estimate confounded by selection bias given that the policy was non-randomly distributed across workers—that is, only workers whose firms' operations fully or partially ceased in response to the pandemic and associated regulations were eligible to apply (Department of Employment and Labour 2020b). To address this source of bias, we exploit the fact that during the first few months of the policy only workers who were registered and contributing to UIF were eligible, as

[10] In brief, the survey collects data on wages before taxation and deductions from all workers. Workers are first asked to report their wages in monetary (Rands) terms, and those that do not are then asked to report the bracket or range that their wage falls into. In the public release of the QLFS wage data from 2010 onwards, StatsSA have included imputations for such cases of non-response. This procedure is not discussed at all in the public documentation; however, an internal document examined by Kerr and Wittenberg (2021) suggests that a hot deck imputation method was used in an inappropriate manner, resulting in a non-negligible share of inappropriate imputations. Unfortunately, the publicly released data does not make it possible to distinguish between imputed responses and actual responses.

[11] This is done by estimating a Mincerian-style wage regression of the logarithm of monthly wages on a vector of observable covariates including age (and its squared term), sex, racial population group, province, marital status, an urban indicator, highest level of education, main industry and occupation, trade union membership, and a public sector indicator. Observations with large residuals (in excess of five) are identified as outliers whose wage values are then recoded as missing.

[12] In our approach, we first imputed a bracket for those employed in the baseline period who did not have a bracket response or were classified as outliers by estimating an ordered logit model using the same observable covariates as in the outlier regression model, as well as the sampling design variables (weight, strata, and primary sampling unit). Thereafter, we imputed log wages based on the imputed bracket using predictive mean matching with five nearest neighbours. We repeat this process five times to obtain five sets of coefficients and standard errors. This approach was similarly followed by Kerr and Wittenberg (2019b) in their generation of the Post-Apartheid Labour Market Series (PALMS) dataset—a compilation of individual-level microdata from household surveys conducted between 1993 and 2019 in South Africa.

discussed in Section 2. This temporary institutional eligibility criterion provides a unique opportunity to isolate the short-term causal effect of the policy using a canonical difference-in-differences (DiD) approach. Importantly, a new theoretical and empirical literature in the DiD domain highlights how estimates obtained from many typical applications are often severely biased and do not correspond with interpretable causal parameters. However, this is only so in settings characterized by more than two time periods or heterogenous or 'staggered' treatment timing (de Chaisemartin and D'Haultfœuille 2020; Callaway and Sant'Anna 2021; Goodman-Bacon 2021; Sun and Abraham 2021; Athey and Imbens 2022; Borusyak et al. 2022; Roth et al. 2022; Imai et al. 2023). These concerns are not however relevant for our identification strategy given that we only have two periods, and every treated observation is treated in the same period. As such, we proceed with the canonical setup.

Using eligibility rather than actual receipt as a treatment assignment rule is common in the literature even if the full treatment group is not actually treated (Duflo 2003; Ranchhod 2006; Ardington et al. 2016; Ranchhod and Finn 2016; Abel 2019; Etinzock and Kollamparambil 2019; Bishop and Day 2020; Autor et al. 2022). Importantly, our use of eligibility rather than actual receipt is also motivated by the absence of data on the policy in our data.[13] Following Bishop and Day (2020) and Autor et al. (2022), we do however additionally scale up our estimate based on eligibility to one based on receipt and discuss this approach in detail in Section 5. Finally, given that the eligibility criteria changed from June 2020 to include all workers regardless of UIF-contribution status, as discussed in Section 2, it should be noted that our identification strategy only allows us to consider effects in the very short term, that is, the first few months of the policy. As such, job retention effects from the second half of 2020 are beyond the scope of this paper.

4.1 Main model specification

Formally, we estimate the following model for individual i in quarter t using ordinary least squares (OLS):

$$Job\ retention_{it} = \alpha + \beta TERS_i + \delta Post_t + \gamma(TERS_i \times Post_t) + \mu \boldsymbol{X}_{it} + \varphi_i + \varepsilon_{it} \tag{1}$$

which can be equivalently expressed as a two-way fixed effects (TWFE) model as:

$$Job\ retention_{it} = \varphi_i + \varphi_t + \beta^{TWFE} D_i + \mu \boldsymbol{X}_{it} + \varepsilon_{it} \tag{2}$$

where $Job\ retention_{it}$ is a binary employment variable equal to one if individual i was employed in both periods and zero if employed only in 2020Q1 but any other labour market status in the following quarter. Considering this temporary policy detail was only in place during the first two months of the policy, $Post_t$ equals one for all observations in April and May 2020 and zero for

[13] Although TERS receipt data does exist in another panel survey (the NIDS-CRAM), this dataset is insufficient for causal inference purposes because in the first wave of the survey, TERS receipt was only asked of workers in April 2020 – the first month of South Africa's national lockdown and the TERS policy. Should one employ a DiD approach here, not only does no relevant pre-treatment data exist, but data on TERS receipt is endogenous: TERS receipt can only be observed among those who remained employed in the treatment period. Notably, our use of the QLFS also has other advantages compared to the NIDS-CRAM, including a much larger sample which implies greater precision for our estimates, and a larger array of labour market variables. Regardless, we make use of the NIDS-CRAM to gauge whether TERS eligibility, through UIF contribution status, can be regarded as a reasonable proxy for TERS receipt during this period. In Table A1, we show that relative to non-recipients, recipients are statistically significantly more likely to work in semi-skilled occupations and have a written employment contract. These differences are consistent with those observed in Table 2 for our treatment groups. We are therefore confident that our use of eligibility as a proxy measure of receipt for this period is reasonable.

those in 2020Q1. Given we only have two time periods in our setup, $Post_t$ is equivalent to controlling for time fixed effects (FE) represented by φ_t in Equation (2). $TERS_i$ is our binary treatment variable indicating eligibility for the TERS during the period of analysis, equal to one for UIF-contributing workers and zero for non-contributing workers as of 2020Q1. Our treatment variable therefore is time-invariant. The relevant question from which this variable is derived is '*Does your employer pay UIF contributions for you?*'. Given that this question was only posed to employees,[14] we only include employees in our sample who represent the majority (84.2 per cent) of workers in the country.

$\boldsymbol{X}_{it}$ is a vector of observable demographic and labour market covariates which include age (and the squared term), sex, self-reported racial population group, province of residence, a binary urban residence indicator, marital status, highest education level, main occupation at the one-digit level, main industry at the one-digit level, firm size, contract duration (permanent, limited, or unspecified duration), weekly working hours, binary indicators for being a trade union member and having a written contract, and several measures of job quality including having paid leave and working for an employer who makes contributions to a pension fund or health insurance on the employee's behalf. All labour market covariates are with respect to their values in the baseline period. To control for labour market churn, we follow Ranchhod and Finn (2016) and generate a binary 'recent job' variable which identifies whether a worker began their job within the last six months preceding the survey. Furthermore, we exploit the panel to generate and control for a binary 'job-mover' variable equal to one for individuals who remained employed but changed occupations or industries (measured at the 1-digit level), and also control for individual FE – φ_i – which, when included, control for observed and unobserved time-invariant heterogeneity, such as worker productivity if assumed constant over time. When doing so, all time-invariant variables in $\boldsymbol{X}_{it}$ are automatically omitted from the model, as well as our time-invariant treatment indicator. However, we are still able to estimate our coefficient of interest, γ in Equation (1) or β^{TWFE} in Equation (2), due to prevailing variation in the DiD interaction term $TERS_i \times Post_t$ in Equation (1), equivalent to D_i in Equation (2). γ and β^{TWFE} then serve as the mean change in job retention probabilities among TERS-eligible workers relative to non-eligible workers from the pre- to the post-policy period. In the absence of any other confounders, this represents the average treatment effect of the policy in the short term. ε_{it} is the error term. All standard errors are clustered at the panel (individual) level to allow for correlation in the error for the same individual over time.

It should be explicitly noted that UIF contribution is highly correlated with employment formality in the South African labour market. In 2020Q1, under the legal definition of formal sector workers in the country, 71 per cent of formal sector workers were UIF contributors in contrast to just 23 per cent of informal sector workers.[15] Therefore, one concern is that our identification strategy here simply compares the employment trajectories of formal versus informal sector workers. To address this concern, we restrict our sample to employees in the formal sector (who represent 70 per cent of total employment as of 2020Q1). We further restrict our sample to employees in the private sector (who represent 67 per cent of total employment as of 2020Q1) considering that, as noted in Section 2, public sector workers were ineligible for the policy. Together, employees in the formal private sector represent 62 per cent of total employment as of 2020Q1. One might expect the formal sector to consist of only UIF-contributing workers. However, non-contributing formal

[14] Employees are defined as individuals who work for someone else for pay such as a wage, salary, commission, or in kind pay.

[15] Statistics South Africa defines formal sector workers as those who pay income tax and those who do not but work for establishments which employ at least five workers.

sector workers exist in the data because of tax and UIF registration exemptions in the relevant legislation.[16] We consider these exemptions in the balance test to follow.

Table 2 presents a balance table of mean estimates for several observable demographic and labour market covariates for our sample of TERS-eligible and -ineligible workers in the baseline and treatment period. Recall that period-specific covariate balance is not a requirement in DiD design. The identifying assumption implies that in the absence of the TERS, the trends in job retention probabilities of those eligible for the policy would have been similar to the equivalent trends for the ineligible on average. The validity of this approach then is threatened if the difference in mean covariate levels between TERS-eligible and -ineligible workers varies significantly from before to after treatment. To examine this, we include estimates of inter-period inter-group differences for each covariate. These estimates are equivalent to placebo outcome test estimates which, for a given covariate, are obtained by estimating the DiD model but using the covariate (which should in theory not be affected by TERS receipt) as the outcome.

Table 2: Covariate balance table, by treatment status and period

	Pre-treatment period			**Post-treatment period**			**Diff-in-diff**
	TERS-ineligible (n=822)	**TERS-eligible (n=3 993)**	**Diff.**	**TERS-ineligible (n=822)**	**TERS-eligible (n=3 993)**	**Diff.**	
	(1)	**(2)**	**(3) = (2) – (1)**	**(4)**	**(5)**	**(6) = (5) – (4)**	**(7) = (6) – (3)**
Demographic covariates							
Age (years)	36.333	38.756	2.423***	36.648	38.786	2.139***	-0.285
	(10.193)	(10.086)	(0.426)	-10.103	(10.235)	(0.503)	(0.296)
Female	0.425	0.413	-0.011	0.396	0.408	0.012	0.023*
	(0.494)	(0.492)	(0.021)	(0.489)	(0.491)	(0.024)	(0.013)
African/Black	0.842	0.689	-0.154***	0.842	0.659	-0.183***	-0.029*
	(0.365)	(0.463)	(0.017)	(0.365)	(0.474)	(0.020)	(0.019)
Urban	0.698	0.825	0.127***	0.709	0.808	0.100***	-0.027*
	(0.459)	(0.380)	(0.019)	(0.454)	(0.394)	(0.022)	(0.018)
Married or living with partner	0.437	0.528	0.091***	0.454	0.530	0.076***	-0.016
	(0.496)	(0.499)	(0.021)	(0.498)	(0.499)	(0.025)	(0.017)
Primary or less education	0.097	0.064	-0.033***	0.106	0.068	-0.038**	-0.004
	(0.296)	(0.245)	(0.012)	(0.307)	(0.252)	(0.015)	(0.014)
Secondary incomplete education	0.406	0.304	-0.102***	0.412	0.304	-0.108***	-0.007
	(0.491)	(0.460)	(0.020)	(0.492)	(0.460)	(0.024)	(0.020)
	0.339	0.423	0.083***	0.316	0.419	0.103***	0.020

[16] All workers in South Africa need to be registered for income tax, unless they earn below the income tax threshold (approximately ZAR80 000 (US$11 478 PPP) per year in 2020), and all workers registered and paying income tax also need to be registered and contributing to the UIF, with some exceptions. These include those (i) working for an employer for less than 24 hours per month, (ii) working for national or provincial government, (iii) being a foreign worker on contract, or (iv) only working for a commission. Workers satisfying condition (ii) are not evident in our data given public sector exclusion from our sample, and unfortunately the QLFS neither includes data on citizenship to consider condition (iii) nor type of remuneration to consider condition (iv). Nevertheless, our control group of UIF non-contributors in the formal private sector can then be said to consist of workers who earn below the income tax threshold or work for less than 24 hours per month, or both.

Secondary complete education	(0.474)	(0.494)	(0.020)	(0.465)	(0.493)	(0.023)	(0.018)
Post-secondary education	0.158	0.209	0.052***	0.166	0.209	0.043**	-0.009
	(0.364)	(0.407)	(0.016)	(0.372)	(0.407)	(0.020)	(0.013)
Labour market covariates							
Primary sector	0.112	0.110	-0.001	0.111	0.133	0.022	0.023*
	(0.315)	(0.313)	(0.013)	(0.315)	(0.340)	(0.018)	(0.014)
Secondary sector	0.274	0.252	-0.023	0.265	0.235	-0.029	-0.007
	(0.446)	(0.434)	(0.019)	(0.441)	(0.424)	(0.023)	(0.017)
Tertiary sector	0.614	0.638	0.024	0.624	0.632	0.008	-0.016
	(0.487)	(0.480)	(0.021)	(0.484)	(0.482)	(0.025)	(0.019)
High-skilled	0.076	0.139	0.063***	0.089	0.152	0.062***	-0.001
	(0.264)	(0.346)	(0.013)	(0.285)	(0.359)	(0.017)	(0.014)
Semi-skilled	0.650	0.660	0.010	0.605	0.640	0.035	0.025
	(0.477)	(0.474)	(0.020)	(0.489)	(0.480)	(0.025)	(0.022)
Less-skilled	0.274	0.201	-0.073***	0.306	0.208	-0.097***	-0.024
	(0.446)	(0.400)	(0.018)	(0.461)	(0.406)	(0.024)	(0.019)
Firm size: 1-9 workers	0.287	0.100	-0.187***	0.318	0.098	-0.220***	-0.033*
	(0.452)	(0.299)	(0.018)	(0.466)	(0.298)	(0.022)	(0.025)
Firm size: 10-49 workers	0.392	0.350	-0.042**	0.363	0.345	-0.018	0.023*
	(0.488)	(0.477)	(0.021)	(0.481)	(0.475)	(0.024)	(0.014)
Firm size: >50 workers	0.294	0.480	0.186***	0.286	0.485	0.198***	0.013
	(0.456)	(0.500)	(0.020)	(0.452)	(0.500)	(0.023)	(0.013)
Firm size: Unknown	0.027	0.071	0.043***	0.032	0.072	0.040***	-0.003
	(0.163)	(0.256)	(0.009)	(0.177)	(0.259)	(0.012)	(0.005)
Union member	0.081	0.305	0.224***	0.071	0.287	0.216***	-0.008
	(0.273)	(0.460)	(0.013)	(0.257)	(0.452)	(0.014)	(0.008)
Written contract	0.556	0.981	0.426***	0.546	0.982	0.436***	0.010
	(0.497)	(0.135)	(0.019)	(0.498)	(0.132)	(0.023)	(0.013)
Weekly working hours	44.000	44.310	0.310	43.603	44.191	0.588	0.278
	(14.717)	(9.533)	(0.576)	(14.751)	(9.744)	(0.649)	(0.348)
Pension fund	0.149	0.637	0.488***	0.126	0.622	0.496***	0.008
	(0.356)	(0.481)	(0.016)	(0.332)	(0.485)	(0.018)	(0.011)
Paid leave	0.308	0.851	0.542***	0.315	0.856	0.541***	-0.001
	(0.462)	(0.356)	(0.019)	(0.464)	(0.351)	(0.023)	(0.013)
Health insurance	0.105	0.295	0.190***	0.087	0.285	0.199***	0.009
	(0.306)	(0.456)	(0.015)	(0.281)	(0.452)	(0.016)	(0.010)
Income tax registered	0.259	0.734	0.474***	0.238	0.719	0.481***	0.006
	(0.438)	(0.442)	(0.019)	(0.426)	(0.450)	(0.022)	(0.013)
Pre-pandemic monthly wage	5 201.61	10 046.45	4844.84**	5 275.26	9 959.80	4 684.54**	-160.30
	(1 572.98)	(3 059.75)	(1 575.18)	(1 403.57)	(3 013.34)	(1 736.11)	(329.828)

Note: this table presents estimates of mean values for observable covariates by treatment group in the baseline period (2020Q1) and the treatment period (2020Q2) for the balanced panel sample, accompanied by inter-period inter-group differences. Sample restricted to the working-age population (15–64 years). Wage values as per 2020Q1 and are expressed in nominal terms. All estimates are weighted using sampling weights and take account of the complex survey design. Standard errors presented in parentheses and are clustered at the panel (individual) level. The magnitude and statistical significance of a given difference are estimated using t-tests. *** p < 0.01, ** p < 0.05, * p < 0.10.

Source: authors' calculations based on QLFS2020Q1 (StatsSA 2020b).

Columns (1) to (3) show that, relative to ineligible workers and in the baseline period, TERS-eligible workers were on average 2.4 years older and were more likely to live in urban areas, be married or be living together with a partner, have higher levels of formal education, work in a higher-skilled occupation, work in larger firms, be a trade union member, have a written contract, and have a higher quality job (as proxied by pension fund contribution, health insurance, and paid leave), while being less likely to be African/Black. Due to exemptions in tax legislation discussed above (workers earning below the income tax threshold do not need to be registered for income tax and do not need to contribute to the UIF if they work less than 24 hours per month), it is unsurprising that eligible workers were nearly three times more likely to be registered for income tax (73 per cent compared to 26 per cent) and had higher monthly pre-pandemic wages (approximately ZAR10,000 (US$1,403 PPP) compared to ZAR5,200 (US$729 PPP) on average). It is notable that mean working hours are statistically similar across the two groups, however eligible workers were less likely to work on a part-time basis (defined as less than 27 hours per week). As shown in columns (4) to (6), the signs, magnitudes, and levels of statistical significance of these differences are similar in the post-treatment period for most covariates. The difference estimates of just five covariates—gender, race, geographic area type, employment sector, firm size—changed in magnitude, as shown in column (7). However, for each of these covariates these changes are not economically significant (that is, the magnitudes of the coefficients are very small) and are only marginally statistically significant at the 10 per cent level. Overall, then, these trends are supportive of the validity of our approach.

4.2 Model extension: progressivity tests

Since TERS subsidies were a function of workers' wages, and specifically subsidies were larger for lower-wage workers in relative terms, we analyse whether the effects were heterogeneous and progressively distributed by making use of the raw, unimputed wage data provided by StatsSA discussed in Section 3 and exploiting the policy benefit formula. We do so through three approaches by estimating polynomial regression models which have a similar specification to Equation (2) but include an interaction term on the DiD term as follows:

$$Job\ retention_{it} = \varphi_i + \varphi_t + \beta^{TWFE} D_i \times w^p_{i2020Q1} + \mu \boldsymbol{X}_{it} + \varepsilon_{it} \tag{3}$$

where $w^p_{i2020Q1}$ represents a p-order polynomial term of the logarithm of worker i's (i) pre-pandemic monthly wage, (ii) imputed monthly TERS subsidy amount by making use of the policy benefit formula and pre-pandemic wage data, or (iii) imputed TERS subsidization rate, defined as worker i's imputed TERS subsidy amount as a share of their pre-pandemic wage. Whereas approaches (i) and (ii) allow us to consider effect variation by the absolute size of the subsidy, approach (iii) offers a relative perspective to examine the 'bite' of the subsidy. Because subsidies were a function of wages, we expect these three sets of estimates to reflect similar qualitative findings. Note that all three measures are time-invariant because all rely on wage data in the pre-pandemic or baseline period. However, as was the case with Equation (2), we are still able to control for φ_i and estimate the coefficient on the interaction terms in these models due to underlying variation in D_i. While these models remain linear in parameters, to avoid explicitly assuming one functional form, we estimate models where $p \in [1,3]$—that is, where the interaction term takes on a linear, quadratic, or cubic form. The resulting β^{TWFE} estimates are then used to predict the marginal effects of TERS receipt across the wage, subsidy, and subsidization rate distributions. Together, these three approaches allow us to gain a comprehensive assessment of whether the job retention effects of the subsidy varied according to its absolute and relative size. It should be noted that the estimates for (ii) and (iii) assume zero employer contributions because data on these contributions are not observed in the survey. However, as a robustness test we re-estimate the models under varying unilateral employer contribution assumptions ranging from 10 to 90 per cent of a given worker's wage. This approach then provides lower- and upper-bound estimates of the heterogenous effects of the policy across the absolute and relative subsidy distributions.

Additionally, we will show that a large share of workers received subsidies which exceeded their pre-pandemic wages. As discussed in Section 2, this was due to the policy's lower-bound take-home-pay amount (Department of Employment and Labour 2020b). It is plausible that job retention effects may again vary for this group of workers relative to those who only had their wages partially subsidized. We explore this binary source of heterogeneity by again estimating a similar interaction model as per Equation (3) but substitute $w^{p}_{i2020Q1}$ with a binary indicator for whether worker i's monthly TERS subsidy amount was larger than their pre-pandemic monthly wage, and zero otherwise.

5 Results

5.1 Main results

In Table 3, we report the main results from our model specified in Equation (1). We consider two sets of specifications which control for different observable and unobservable covariates. First, columns (1)–(3) present the estimates which only account for observable demographic and labour market covariates. Second, columns (4)–(6) additionally account for individual FE which absorb any unobserved time-invariant heterogeneity. Overall, we find evidence of a statistically significant and positive effect of the TERS policy on job retention in the short-term. This finding appears robust across all specifications and suggests that the TERS policy was successful in its primary aim of mitigating job losses, at least in the short-term. The estimated effect is relatively precise and ranges between 15.5 and 17.8 percentage points, with all estimates being statistically significant at the 1 per cent level. Interestingly, these estimates are not substantially different from that found by Köhler and Hill (2022).[17] However, they are much more precise.

Beginning with the first set of specifications, in column (1) the coefficient α (the constant term) is equal to one, which is expected given that we only include employed individuals in the pre-treatment period. For the same reason, the coefficient on $TERS_i$ indicates that eligible workers were not more likely than TERS-ineligible workers to be employed in the pre-treatment period, and expectedly the standard error could not be estimated. The binary $Post_t$ variable coefficient reflects the reduction in the probability of remaining employed for TERS-ineligible workers between 2020Q1 to 2020Q2, which is not unexpected considering the onset of the pandemic and lockdown regulations in South Africa at the end of 2020Q1. Our estimate of interest—the DiD term D_i—indicates that the TERS increased the probability of remaining employed by 17.5 percentage points, which however appears overestimated. When controlling for observable demographic and labour market covariates in column (3), this effect reduces to 15.5 percentage points, while the degree of statistical significance remains.

[17] The 95 per cent confidence intervals of the estimates here overlap with those estimated in Köhler and Hill (2022) (9.48–26.72 percentage points).

Table 3: Effect estimates of TERS eligibility on job retention

	(1)	(2)	(3)	(4)	(5)	(6)
$TERS_i$	0.000	-0.010***	-0.060***	.	.	.
	.	(0.003)	(0.010)	.	.	.
$Post_t$	-0.321***	-0.322***	-0.272***	-0.321***	-0.314***	-0.264***
	(0.021)	(0.021)	(0.021)	(0.021)	(0.022)	(0.022)
D_i	0.175***	0.178***	0.155***	0.175***	0.174***	0.156***
	(0.022)	(0.022)	(0.022)	(0.022)	(0.023)	(0.022)
Demographic controls	×	✓	✓	×	✓	✓
Labour market controls	×	×	✓	×	×	✓
Individual FE	×	×	×	✓	✓	✓
Constant	1.000***	0.720***	0.850***	1.000***	3.345***	3.831***
	(0.000)	(0.075)	(0.075)	(0.004)	(1.017)	(0.965)
Observations	8 520	8 450	8 303	8 520	8 450	8 303
R^2	0.114	0.129	0.165	0.560	0.561	0.558

Note: this table presents the effect estimates of TERS eligibility on job retention as per Equation (1). Sample restricted to the employed working-age population (15–64 years) in the formal private sector as of 2020Q1. FE = fixed effects. All estimates are weighted using sampling weights and take account of the complex survey design. Standard errors presented in parentheses and are clustered at the panel (individual) level. *** p < 0.01, ** p < 0.05, * p < 0.10.

Source: author's calculations based on QLFS2020Q1 and 2020Q2 (StatsSA 2020b, 2020c).

Considering the second set of specifications in columns (4)–(6), our effect estimates range between 15.6 and 17.5 percentage points, and thus appear relatively insensitive to the inclusion of individual FE. The precision of these estimates also remains effectively unchanged. The coefficient on $Post_t$ is also very stable across specifications. Expectedly, the binary $TERS_i$ variable is omitted from the model given that it is constant within workers over time, but we are still able to estimate an effect due to prevailing variation in the DiD interaction term, as discussed above. Our preferred effect estimate in column (6), which controls for observable demographic and labour market covariates as well as individual FEs, indicates that the TERS increased the probability of remaining employed by 15.6 percentage points.

The above estimate specifically refers to the effect of being TERS-eligible. However, the estimate understates the effect of TERS receipt because not all eligible workers received the TERS. To obtain an estimate of TERS receipt on job retention, we follow Bishop and Day (2020) and Autor et al. (2022) and scale our estimate to account for the estimated probability of receipt conditional on being eligible (in other words, the take-up rate). Formally, we scale our effect estimate as follows:

$$\beta^{receipt} = \frac{\beta^{TWFE}}{E(TERS\ receipt_i \mid Eligibility_i = 1\ )} \tag{4}$$

where β^{TWFE} is our effect estimate of TERS eligibility of 15.6 percentage points and $E(TERS\ receipt_i \mid Eligibility_i = 1\ )$ is the probability of receipt conditional on being eligible. Like Bishop and Day (2020), while we cannot estimate this probability using the QLFS data given the lack of data on TERS receipt; we infer it using a combination of the QLFS and administrative data on TERS recipient levels by month, obtained from an internal document from the South

African Department of Employment and Labour. Specifically, we divide the sum of the number of TERS recipients in April and May 2020 (approximately 4.4 million and 3.6 million, respectively, or 8 million collectively, as per the internal document) by the number of eligible workers in each month (approximately 8.5 million UIF-contributing workers per month, as estimated in the 2020Q1 QLFS) to yield a take-up rate of 47 per cent.[18] As such, $\beta^{receipt} = \frac{0.156}{0.4397} = 0.332$. This implies that 33.2 per cent of workers who received the TERS would not have remained employed had they not received the TERS during the period. Given the 8 million recipients during the period as indicated above, this proposes that the TERS saved approximately 2.7 million jobs during the two-month period. While this strongly suggests job loss at the pandemic's onset would have been significantly more severe in the absence of the policy, comparisons to the actual number of job losses reported at the onset of the pandemic would not be accurate.[19] Additionally, this procedure implies that a non-negligible share of workers (66.8 per cent) who received the TERS would have remained employed anyway. This share of inframarginal workers likely reflects the fact that the policy initially prioritized rapid disbursement of relief over accurately targeting those most affected or in need, which was similarly the case in other contexts (Bishop and Day 2020; Chetty et al. 2020; Dalton 2021; Autor et al. 2022; Smart et al. 2023). Importantly, while this implies that the policy did not help this subgroup of workers remain employed, it still provided them with income support.

Finally, we estimate the cost-effectiveness of the policy by combining this estimate of the number of jobs saved with data on the policy's monthly expenditure during the period obtained from the same former referenced internal document. Given that expenditure on the TERS amounted to ZAR35.1 billion (US$4.9 billion PPP) for April and May 2020, we estimate the average job saved cost ZAR13,195 (US$1,851 PPP) per month in nominal terms. This cost is large relative to the median monthly wage of eligible workers of ZAR5,315 (US$746 PPP) and nearly four times larger than the median monthly subsidy amount of ZAR3,500 (US$491 PPP). While this cost compares favourably to those in other contexts, such as in the United States where Autor et al. (2022) estimate a monthly cost per job saved equivalent to 3.4 times median monthly earnings, it does suggest that expenditure on the TERS exceeded the wage costs of jobs supported by it. This again likely reflects the policy's initial prioritization of rapid disbursement of relief over accurate targeting.

5.2 Progressivity test results

In Figure 2, we present our marginal effect estimates of TERS eligibility on job retention across the wage, subsidy amount, and subsidization rate distributions as per Equation (3). Regardless of the choice of the moderating variable or polynomial order, we find that the distribution of job retention effects is marginally regressive. While all point estimates are positive across each distribution—indicative of positive effects for all recipients regardless of the absolute or relative size of their wage or subsidy—effects appear to be an increasing function of subsidy amounts. Considering panel (a) which presents the marginal effect estimates across the pre-pandemic wage distribution, the linear polynomial specification exhibits a positive, albeit weak, gradient with

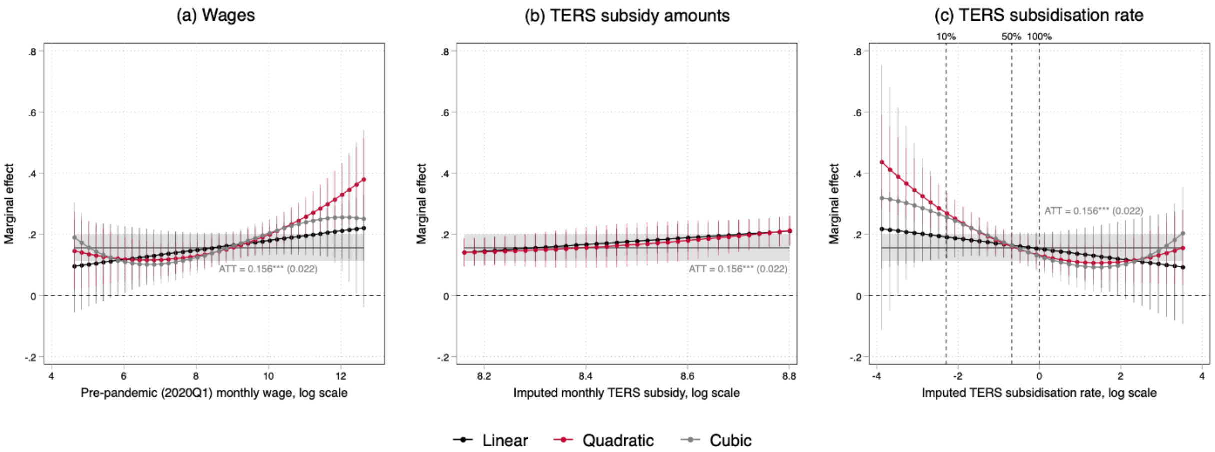
[18] Because the post-period for our effect estimate is April and May 2020, we restrict our data on receipt to be for the same period. However, we estimate eligibility using pre-pandemic (2020Q1) QLFS data given that this yields the number of eligible workers closest to the time the policy was introduced.

[19] This is because, first, the QLFS is cross-sectional in nature and hence the extent of job loss from 2020Q1 to 2020Q2 of 2.2 million individuals is in net terms. However, our estimate is in gross terms. Second, our estimate speaks to the number of jobs saved during the April and May 2020 period, whereas job loss in the QLFS from 2020Q1 to 2020Q2 is calculated using all three months in the latter quarter.

effects ranging from 10 to 22 percentage points for workers in our sample. A joint Wald test shows that this relationship is statistically significant (p=0.000). The estimates suggest that an increase of 0.2 log monthly wage points is associated with a 0.3 percentage point increase in job retention effects, equivalent to a 22 per cent or ZAR828 (US$116 PPP) increase in wages at the mean of the distribution.

Similarly, the quadratic polynomial estimates exhibit a positive but slightly stronger relationship with effect estimates ranging from 12 to 38 percentage points. Nearly all the point estimates from this specification do not statistically significantly differ from their equivalents in the linear polynomial specification. However, the now non-linear relationship suggests that, although effect estimates remain positive across the wage distribution and largely tend to increase with wages, effects appear to decrease to a minimum of 12 percentage points for workers earning up to approximately ZAR760 (US$107 PPP) per month. This however represents a small minority of workers at the distribution's lower tail and hence may be biased by noise in the data. The estimates from the cubic specification largely follow their equivalents in the quadratic specification. All estimates remain positive but have a narrower range of 10 to 26 percentage points, similar to the linear polynomial estimates.

Figure 2: Marginal effect estimates of TERS eligibility across varied distributions



Note: this figure presents marginal effect estimates of TERS eligibility on job retention probabilities across the wage, TERS subsidy, and TERS subsidization rate distributions obtained from linear predictions of the model estimates as per specification (4). Out-of-sample margins not included. Estimates make use of the multiply imputed wage data. Sample restricted to the employed working-age population (15–64 years) in the formal private sector as of 2020Q1. All estimates are weighted using sampling weights. Standard errors are clustered at the panel (individual) level. Spikes and shaded regions represent 95 per cent confidence intervals. ATT refers to the mean treatment effect as observed in Table 3.

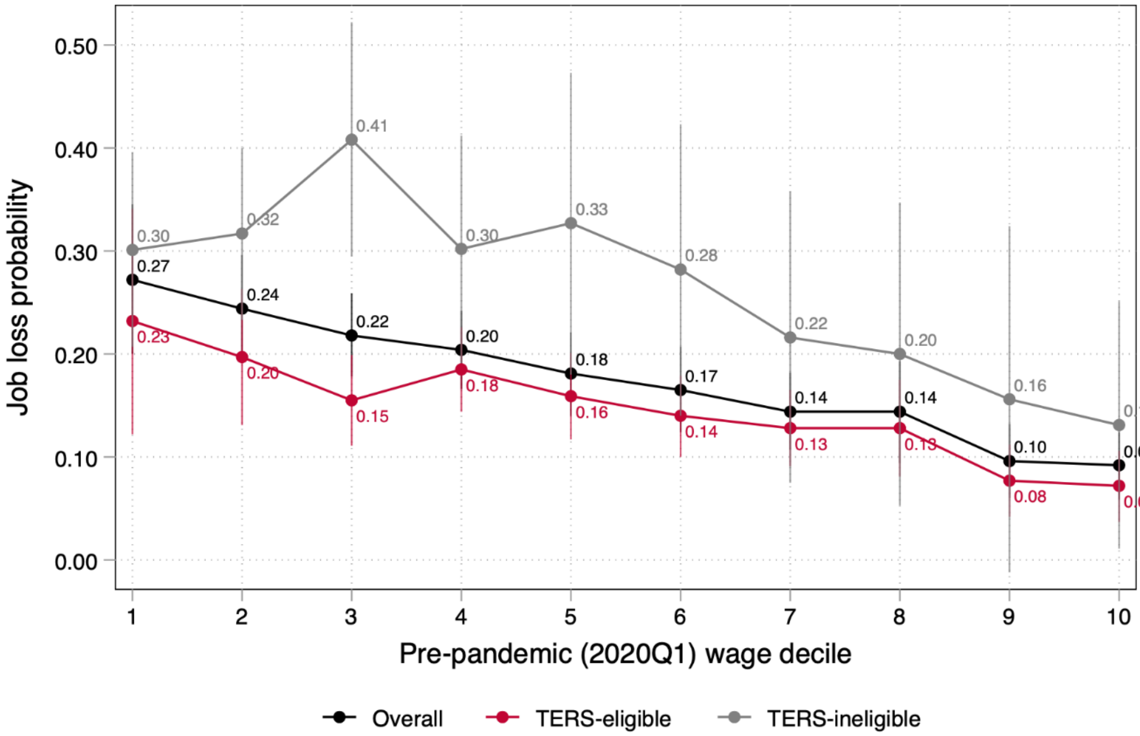
Source: author's calculations based on QLFS 2020Q1 and 2020Q2 (StatsSA 2020b, 2020c).

The estimates in panels (b) and (c), which consider effect heterogeneity by the imputed 'bite' of the subsidy in absolute and relative terms, also suggest that the short-term job retention effects of the policy were regressive. In the former, we again find a positive but weak relationship between effect estimates and imputed monthly subsidy amounts. Effects are positive across the whole subsidy distribution and range from 14 percentage points for workers who received the minimum subsidy amount of ZAR3,500 (US$491 PPP) per month to 21 percentage points for those who received the maximum of approximately ZAR6,730 (US$966 PPP) per month. The linear relationship is weaker to that of wages in panel (a), suggesting that an increase in a recipient's monthly subsidy of just 0.02 log points is associated with a 0.2 percentage point increase in job retention effects. This is equivalent to a 2 per cent or ZAR81 increase in subsidy amounts at the mean of the distribution. The quadratic and cubic polynomial estimates are all very similar to the linear estimates with respect to both magnitude and statistical significance.

In panel (c), we find an inverse relationship between job retention effects and the subsidization rate. Although the sign of this relationship contrasts with the previous two sets of estimates, this finding is again indicative of a regressive effect distribution given that, as discussed in Section 2, the policy design ensured that lower-wage workers received larger benefits in relative terms while higher-wage earners received larger benefits in absolute terms. The linear polynomial estimates suggest that effects are again positive across the whole subsidization rate distribution, ranging from just under 10 percentage points to 22 percentage points, with most estimates being statistically significant apart from those for very low-earning workers at the upper tail. Importantly, these estimates imply large effects even when only a small share of workers' wages were subsidized: a 10 per cent subsidization rate is associated with a job retention effect of approximately 20 percentage points, compared to 16.5 percentage points for a 50 per cent subsidization rate and 15 percentage points for a 100 per cent rate. This suggests that an increase in a recipient's subsidization rate of 0.2 log points is associated with a 0.3 percentage point decrease in job retention effects—similar in magnitude to the linear wage relationship in panel (a) but opposite in sign. The quadratic and cubic polynomial estimates again both imply a negative but stronger relationship for most of the subsidization rate distribution. These effect estimates range from approximately 10 percentage points to 32 percentage points for the cubic estimates and 43 percentage points for the quadratic estimates. However, most of these estimates do not statistically differ from the linear estimates. Finally, recall that some workers received subsidies which exceeded their pre-pandemic wages because of the policy's lower-bound subsidy amount. The linear estimates in panel (c) show that effects continue to decrease even for these workers whose wages were not only subsidized but effectively 'topped-up' and increased. However, they remained positive. We explore variation in effects for these workers later in this section.

One might argue that this regressive distribution of job retention effects may at least partially explain the regressive distribution of job loss observed in the South African labour market at the onset of the pandemic. To examine this, in Figure 3 we present estimates of job loss probabilities across the pre-pandemic wage distribution by TERS eligibility status, again restricting the sample to formal private sector workers in the baseline period. The estimates show that, regardless of a worker's position in the wage distribution (in other words, controlling for wages), TERS-eligible workers faced lower job loss probabilities relative to ineligible workers. Across most of the distribution, the latter group was nearly two times more likely to experience job loss relative to the former. This is in line with our estimates above of positive job retention effects of the policy across the entire wage distribution. However, controlling for TERS eligibility, we continue to observe a regressive job loss distribution. These simple bivariate descriptive statistics together with our causal estimates above suggest that although the TERS saved jobs across the wage distribution regressively, the regressive distribution of job loss appears to be explained by factors other than the policy itself. An analysis of these factors however lies beyond the scope of this paper.

Figure 3: Job loss probabilities across the wage distribution, by TERS eligibility status



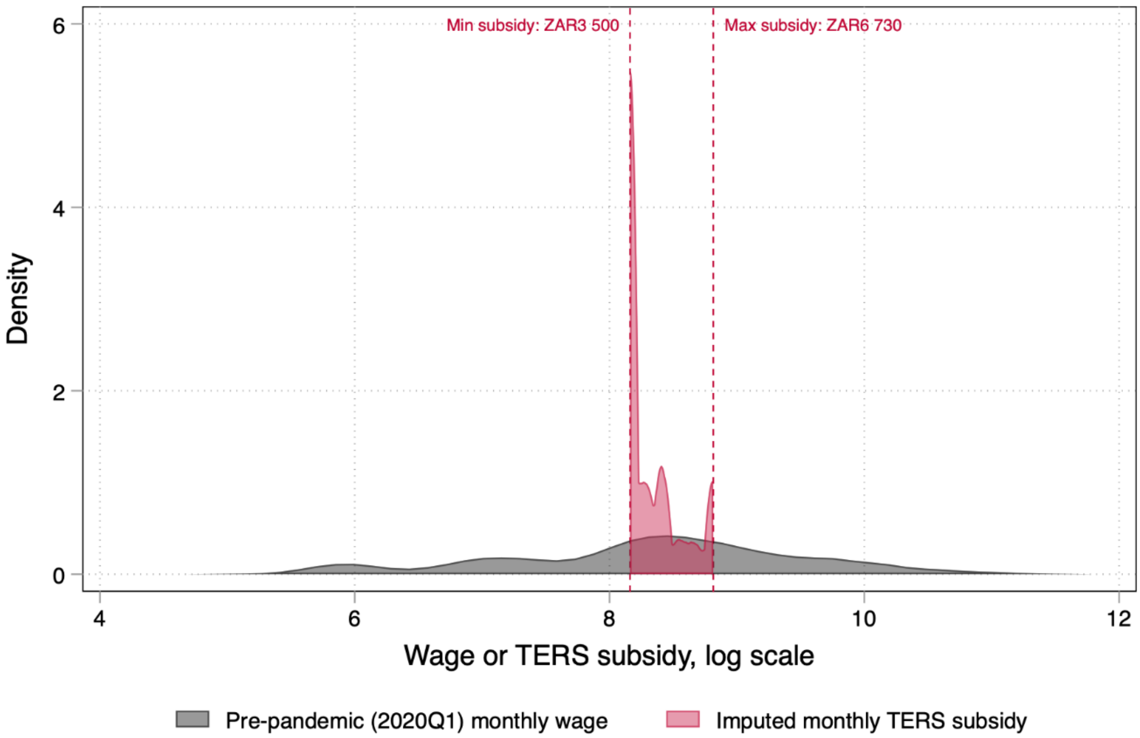
Note: this figure presents estimated job loss probabilities across the pre-pandemic (2020Q1) monthly wage distribution by treatment status (TERS eligibility). Job loss defined as being either unemployed (according to the narrow or broad definition) or economically inactive in 2020Q2 conditional on employment in 2020Q1. Estimates make use of the multiply imputed wage data. Sample restricted to the employed working-age population (15–64 years) in the formal private sector as of 2020Q1. All estimates are weighted using sampling weights. Standard errors are clustered at the panel (individual) level. Spikes represent 95 per cent confidence intervals.

Source: author's calculations based on QLFS 2020Q1 and 2020Q2 (StatsSA 2020b, 2020c).

As previously discussed, owing to the policy's lower-bound subsidy amount, many workers received subsidies which exceeded their pre-pandemic wages. These wages were effectively not only subsidized but 'topped-up' and increased. In Figure 4 we present the distributions of pre-pandemic monthly wages and imputed TERS subsidy amounts for eligible workers, again in the formal private sector. For brevity, these imputed subsidy amounts assume zero employer contributions. Expectedly, the subsidy distribution is both left- and right-censored given the policy's lower- and upper-bound amounts. While the average TERS-eligible worker earned a monthly wage of ZAR10,047 (US$1,409 PPP) (standard error (s.e.) = ZAR3,064), we estimate that they received an average monthly subsidy of ZAR4,180 (US$595 PPP) (s.e. = ZAR210), assuming zero employer contributions.[20] The left section of the figure makes it clear that many eligible workers—we estimate nearly a third (33.5 per cent; s.e. = 11.5 per cent)—received subsidies which exceeded their pre-pandemic wages. This large share is not surprising given (i) the pre-pandemic wages of all these workers were less than the policy's lower-bound subsidy amount of ZAR3,500 (US$491 PPP) under the zero employer contributions scenario and (ii) the well-documented extent of minimum wage non-compliance in the South African labour market (Bhorat et al. 2012a, 2012b, 2015, 2021).

[20] Given the unequal wage distribution, it is worth noting the estimated median monthly wage of ZAR5,315 (US$745 PPP) (s.e. = ZAR1,347) and median monthly TERS subsidy of ZAR3,500 (US$491) (s.e. = ZAR5).

Figure 4: Distributions of pre-pandemic wages and TERS subsidy amounts



Note: this figure presents kernel density estimates of the distributions of pre-pandemic (2020Q1) monthly wages and imputed TERS subsidy amounts among TERS-eligible workers. Estimates make use of the multiply imputed wage data. Sample restricted to the employed working-age population (15–64 years) in the formal private sector as of 2020Q1. All estimates are weighted using sampling weights. Subsidy amounts assume zero employer contributions.

Sourc: author's calculations based on QLFS 2020Q1 and 2020Q2 (StatsSA 2020b, 2020c).

As discussed in Section 4 it is plausible that job retention effects may vary for this group of workers who received subsidies which exceeded their usual wages relative to those who only had their wages partially subsidized. In Table 4 we present our model results as per Equation (3) with the coefficient of interest being the interaction of the DiD term (D_i) and the binary indicator for whether worker i's monthly TERS subsidy amount was larger than their pre-pandemic monthly wage ($Subsidy_i > wage_i$). Recall that the estimates on the $TERS_i$ and $Subsidy_i > wage_i$ variables are omitted from the FE models because they are coded based on pre-pandemic data and hence are time-invariant, however the interaction term of interest can still be estimated due to prevailing variation in D_i. While we continue to estimate an average treatment effect of approximately 15 percentage points, which is largely insensitive to the inclusion of observable controls and individual FE, we do not find any evidence that effects vary by whether a worker received a subsidy larger than their usual wage on average or not. This finding holds regardless of model specification, with all estimates being statistically insignificant and close to zero in magnitude, as well as under varying employer contribution rates ranging from 10 to 90 per cent.[21]

[21] These results are not presented here but are available from the authors upon request.

Table 4: Heterogeneous effect estimates of TERS eligibility, by subsidy in excess of pre-pandemic wage

	(1)	(2)	(3)	(4)	(5)	(6)
$TERS_i$	0.000	-0.007*	-0.054***	.	.	.
	(0.000)	(0.004)	(0.010)	.	.	.
$Post_t$	-0.282***	-0.287***	-0.243***	-0.282***	-0.279***	-0.238***
	(0.033)	(0.034)	(0.033)	(0.033)	(0.034)	(0.034)
D_i	0.150***	0.155***	0.141***	0.150***	0.153***	0.147***
	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)
$Subsidy_i > wage_i$	0.000	0.004	0.019	.	.	.
	(0.000)	(0.007)	(0.012)	.	.	.
$TERS_i \times (Subsidy_i > wage_i)$	-0.000	0.000	0.003	.	.	.
	(0.000)	(0.005)	(0.008)	.	.	.
$Post_t \times (Subsidy_i > wage_i)$	-0.076	-0.068	-0.058	-0.076	-0.067	-0.054
	(0.054)	(0.054)	(0.052)	(0.054)	(0.054)	(0.051)
$D_i \times (Subsidy_i > wage_i)$	0.022	0.018	0.001	0.022	0.015	-0.004
	(0.047)	(0.047)	(0.047)	(0.047)	(0.047)	(0.048)
Demographic controls	×	✓	✓	×	✓	✓
Labour market controls	×	×	✓	×	×	✓
Individual FE	×	×	×	✓	✓	✓
Constant	1.000***	0.737***	0.852***	1.000***	3.329***	5.912***
	(0.000)	(0.075)	(0.075)	(0.003)	(1.015)	(2.232)
Observations	8 520	8 450	8 299	8 520	8 450	8 299
R^2	0.119	0.133	0.168	0.200	0.202	0.184

Note: this table presents the model estimates as per specification (3) of the heterogenous effects of TERS receipt on job retention by whether a worker's imputed subsidy amount exceeded their pre-pandemic wage. Sample restricted to the employed working-age population (15–64 years) in the formal private sector as of 2020Q1. FE = fixed effects. All estimates are weighted using sampling weights and take account of the complex survey design and multiply imputed wage data. Standard errors presented in parentheses and are clustered at the panel (individual) level. Subsidy amount estimates assume zero employer contributions. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Source: author's calculations based on QLFS 2020Q1 and 2020Q2 (StatsSA 2020b, 2020c).

6 Robustness tests

In this section we present the results of a battery of robustness tests which seek to address the validity of our identification strategy. First, it should be noted that the estimates in column (7) of Table 2 are equivalent to unconditional placebo-outcome tests. That is, the estimates are individually obtained by estimating the DiD model but using each covariate as the outcome. These outcomes should theoretically be unaffected by the policy, such as exogenous worker demographic characteristics including age, race, sex, and highest level of education. Evidence of any significant estimate here would suggest that our control group of TERS-ineligible workers must be flawed. As previously discussed in Section 4, most estimates are not statistically significant, and for those four that are, they are only marginally significant at the 10 per cent level and the coefficient magnitudes are very small. These findings are therefore supportive of the validity of our approach.

Second, recall as per the discussion in Section 4 that, due to tax legislation exemptions, our control group of UIF non-contributors in the formal private sector includes workers who earn below the income tax threshold and hence are not income tax registered, or work for less than 24 hours per month, or both. This contrasts with our treatment group within which workers are much more likely to earn above the threshold and be tax registered (see Table 2). To examine whether these differences influence our main results, we re-estimate the model in Equation (1) but additionally control for binary indicators for income tax registration, working more than 24 hours per month, and earning above the income tax threshold. Additionally, as an alternative specification, we use a logit model to estimate propensity scores which are simply the predicted conditional probabilities of being UIF-registered as a sole function of these three covariates. We then control for these estimated scores as a composite exemption measure in our models. We present the results of these models in Table 5. We do not control for individual FE here given that each additional variable is coded based on pre-pandemic or baseline data and hence are time-invariant. We find that our main effect is very insensitive to the inclusion of each of the three additional controls or propensity scores, individually (columns 2, 3, 4, and 6) and collectively (columns 5 and 7). The precision of the estimate is unchanged and the coefficient itself has a very narrow range between 15.6 and 15.7 percentage points—only slightly higher than the comparable estimate of 15.5 percentage points in column (1)—and remains statistically significant at the 1 per cent level. Notably, most coefficients of the additional controls are close to zero in magnitude and are not statistically significant. As such, we are confident that our main results are not driven by these characteristic differences between TERS-eligible and -ineligible workers.

Table 5: Effect estimates of TERS eligibility, controlling for legislation exemption factors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
D_i	0.155***	0.156***	0.157***	0.156***	0.156***	0.156***	0.157***
	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)
$Income\ tax\ registered_i$		0.005			0.005		-0.112*
		(0.010)			(0.010)		(0.062)
$Monthly\ working\ hours > 24_i$			0.051*		0.051*		-0.009
			(0.031)		(0.031)		(0.037)
$Wage > tax\ threshold_i$				0.001	0.000		-0.011
				(0.009)	(0.009)		(0.012)
$Propensity\ score_i$						0.036	0.435*
						(0.036)	(0.229)
Demographic controls	✓	✓	✓	✓	✓	✓	✓
Labour market controls	✓	✓	✓	✓	✓	✓	✓
Constant	0.850***	0.900***	0.849***	0.899***	0.849***	0.877***	0.631***
	-0.075	(0.073)	(0.078)	(0.073)	(0.078)	(0.078)	(0.146)
Observations	8 303	8 303	8 303	8 299	8 299	8 299	8 299
R^2	0.165	0.164	0.164	0.164	0.164	0.164	0.165

Note: this table presents the model estimates of D_i as per specification (2) with the inclusion of three additional controls of interest relating to legislation exemption from UIF registration (income tax registration, working more than 24 hours per month, and earning above the income tax threshold) as well as an estimated propensity score. Individual fixed effects (FE) not included. Sample restricted to the employed working-age population (15–64 years) in the formal private sector as of 2020Q1. All estimates are weighted using sampling weights and take account of the complex survey design. Standard errors presented in parentheses and are clustered at the panel (individual) level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Source: author's calculations based on QLFS 2020Q1 and 2020Q2 (StatsSA 2020b, 2020c).

Next, we consider the expansion of another South African labour market policy—the Employment Tax Incentive (ETI)—in a similar period as the TERS as another potential source of bias. Initially introduced in 2014, the ETI provides employers with payroll tax credits for up to two years for newly hired workers aged 18–29 years who earn no more than ZAR6,500 (US$933 PPP) per month but above the relevant minimum wage (SARS 2022). For more detail on how the ETI is structured and the effects of this policy on South African labour market outcomes, the interested reader is referred to Ranchhod and Finn (2016), Bhorat et al. (2020), and Budlender and Ebrahim (2021). On 1 April 2020, approximately the same time as the introduction of the TERS, the ETI was announced to be expanding in two ways. First, the monthly credit amount was increased for workers who were eligible under the original stipulations by an additional ZAR500 (US$70 PPP). Second, a monthly credit of ZAR500 was provided for workers aged 18–29 years for whom the subsidy period had ended previously, or for older workers earning below ZAR6,500 (US$912 PPP) per month who were ineligible for the ETI because of their age (National Treasury 2020). This expansion was only available for employers registered with the South African Revenue Service (SARS) on 1 March 2020 and scheduled to run for four months ending in July 2020.[22] Notably, relative to the magnitude of TERS subsidies, the expansion of the ETI was small but not insignificant in the South African labour market.

Because both the TERS and ETI provided income support for overlapping groups of workers during the same time although by different means, our estimated effect of the TERS may be confounded. To address this, we control for worker eligibility under the expanded ETI policy in our models by, given the policy's description above, simply using a binary indicator of whether worker i's pre-pandemic monthly wage was below ZAR6,500 (US$912 PPP) but above the national minimum wage of ZAR3,500 (US$491 PPP).[23] We again do not control for individual FE here given the ETI variable is based on pre-pandemic wage data and hence is time-invariant. We present the results of these models in Table 6. The most parsimonious model in column (1) shows that our estimated effect is insensitive to simply controlling for expanded ETI eligibility. The coefficient on the ETI indicator is statistically insignificant and close to zero. Our effect estimate reduces marginally but not meaningfully after additionally including demographic controls; however, including labour market controls reduces to estimated effect to 11.4 percentage points—four percentage points (or 26 per cent) lower than the equivalent estimate in Table 3, a statistically significant difference. We interact the ETI eligibility term with D_i as an alternative specification as shown in columns (5) and (6) but find similar results to the preceding estimates. Although the ETI coefficient remains close to zero in all specifications, overall, this result suggests that our main estimate may be slightly upwards-biased, however it is still relatively large in magnitude and remains statistically significant at the 1 per cent level. This then suggests that our primary result of the positive effect of the TERS on job retention is not driven by the coincidental introduction of the expanded ETI policy.

[22] The ETI was expanded once again in 2021, but discussion of this expansion is beyond the scope of this paper.

[23] Like the TERS, due to the absence of any ETI-related item in the QLFS instrument, we are unable to identify actual recipients of the policy in the data.

Table 6: Effect estimates of TERS eligibility, controlling for expanded ETI eligibility

	(1)	(2)	(3)	(4)	(5)	(6)
D_i	0.178***	0.171***	0.114***	0.114***	0.184***	0.115***
	(0.022)	(0.022)	(0.023)	(0.023)	(0.023)	(0.023)
$Expanded\ ETI\ eligibility_i$	-0.015	-0.001	0.004	0.005	-0.006	0.005
	(0.010)	(0.010)	(0.009)	(0.010)	(0.007)	(0.009)
$D_i \times Expanded\ ETI\ eligibility_i$					-0.019	-0.001
					(0.021)	(0.019)
Demographic controls	×	✓	✓	✓	×	✓
Labour market controls	×	×	✓	✓	×	✓
Legislation exemption controls	×	×	×	✓	×	✓
Constant	1.004***	0.713***	0.831***	0.825***	1.002***	0.825***
	(0.003)	(0.076)	(0.076)	(0.080)	(0.002)	(0.079)
Observations	8 520	8 450	8 299	8 299	8 520	8 299
R^2	0.116	0.130	0.164	0.164	0.116	0.164

Note. this table presents the model estimates of D_i as per specification (2) with the inclusion of an additional control variable measuring worker-level eligibility for South Africa's Employment Tax incentive (ETI). Sample restricted to the employed working-age population (15–64 years) in the formal private sector as of 2020Q1. Individual fixed effects (FE) not included. Legislation exemption controls include those listed in Table 5 notes. All estimates are weighted using sampling weights and take account of the complex survey design. Standard errors presented in parentheses and are clustered at the panel (individual) level. *** p < 0.01, ** p < 0.05, * p < 0.10.

Source: author's calculations based on QLFS 2020Q1 and 2020Q2 (StatsSA 2020b, 2020c).

We next conduct a placebo-period test by re-running our model specified in Equation (1) using data from the rotational panel component of the QLFS for the same period in 2018 and 2019. Because this period was prior to the policy being in place, we expect to find no evidence of any effect. We exploit the pre-pandemic QLFS's rotational panel component to identify the balanced panel samples from 2018Q1 2018Q2 and 2019Q1–2019Q2, respectively, using the household and respondent identifiers available in the data. We present the results of these models in Table 7, where the estimates in columns (1) to (3) are obtained using the pooled 2018 and 2019 samples together and those in columns (4) to (7) use the year-specific stratified samples to allow us to control for individual FE. In all specifications, we estimate a positive and statistically significant estimate of D_i ranging between approximately 6 and 7 percentage points. It is notable that the magnitude of this estimate is much smaller (nearly three times) relative to our main results in Table 3 which range between 16 and 18 percentage points. These estimates are also statistically significantly different from one another. Arguably, this stark difference in the significance and magnitudes of the coefficients between the pre-treatment and treatment years speaks to the effect of the TERS policy but implies that our estimated effect may be slightly overestimated—in line with our results in Table 6.

Table 7: Placebo-period test effect estimates of TERS eligibility

Sample:	**2018Q1+Q2; 2019Q1+Q2**			**2018Q1+Q2**		**2019Q1+Q2**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
D_i	0.074***	0.074***	0.058***	0.059***	0.058***	0.057***	0.057***
	(0.008)	(0.008)	(0.007)	(0.009)	(0.009)	(0.010)	(0.010)
2019_t		-0.001	-0.000				
		(0.002)	(0.002)				
Demographic controls	×	×	✓	✓	✓	✓	✓
Labour market controls	×	×	✓	✓	✓	✓	✓
Individual FE	×	×	×	×	✓	×	✓
Constant	1.000***	1.001***	0.922***	0.899***	1.721***	0.945***	1.245***
	(0.000)	(0.001)	(0.018)	(0.026)	(0.479)	(0.024)	(0.401)
Observations	22 226	22 226	21 920	11 434	11 434	10 486	10 486
R^2	0.043	0.043	0.062	0.064	0.522	0.067	0.525

Note: this table presents the placebo period test estimates of D_i as per specification (2) for the balanced rotational panel samples in 2018Q1–2018Q2 and 2019Q1–2019Q2. Sample restricted to the employed working-age population (15–64 years) in the formal private sector in the baseline period. FE = fixed effects. All estimates are weighted using sampling weights and take account of the complex survey design. Standard errors presented in parentheses and are clustered at the panel (individual) level. *** p < 0.01, ** p < 0.05, * p < 0.10.

Source: author's calculations based on QLFS 2018Q1, 2018Q2, 2019Q1, and 2019Q2 (StatsSA 2018a, 2018b, 2019a, 2019b).

Our next robustness test relates to our use of multiple imputation (MI) to address the non-randomly distributed missing wage data and then use these data for our heterogeneity analysis. As discussed in Section 3 in our MI process we estimate five imputations which, taken together, reflect the uncertainty around the true value. As shown in the literature (van Buuren et al. 1999; Royston 2004; Graham et al. 2007), the number of imputations is one aspect of the MI process that can affect the reliability of imputed draws and statistical inference. To examine the stability of our estimates, we re-estimate Equation (3) for each polynomial order and two interaction terms of interest—monthly wages (an absolute measure of the 'bite' of the policy) and the subsidization rate (a relative measure)—using a varied number of imputations ranging from two to 50. We use two imputations as the baseline given Daniels' (2022) finding that stability of estimates of MI data can be achieved with as little as two imputations. We present the estimates of the relevant interaction term in Table 8, omitting the coefficients of D_i alone for brevity.[24] As shown in the table, compared to our main results shown in column (1), the magnitudes and signs of the estimates are very insensitive to the number of imputations. This holds regardless of the choice of interaction term or its polynomial order. In few cases, estimates using just two imputations are not significant, however those with more imputations are. This is not unexpected given that a greater number of imputations tends to increase precision, and in all cases changes in estimates or standard errors are marginal. As such, given the stability of the estimates, we are confident our results are not sensitive to our choice of the number of wage imputations.

[24] These estimates are available from the authors upon request.

Table 8: Interaction term estimates by interaction term, polynomial order, and number of wage imputations

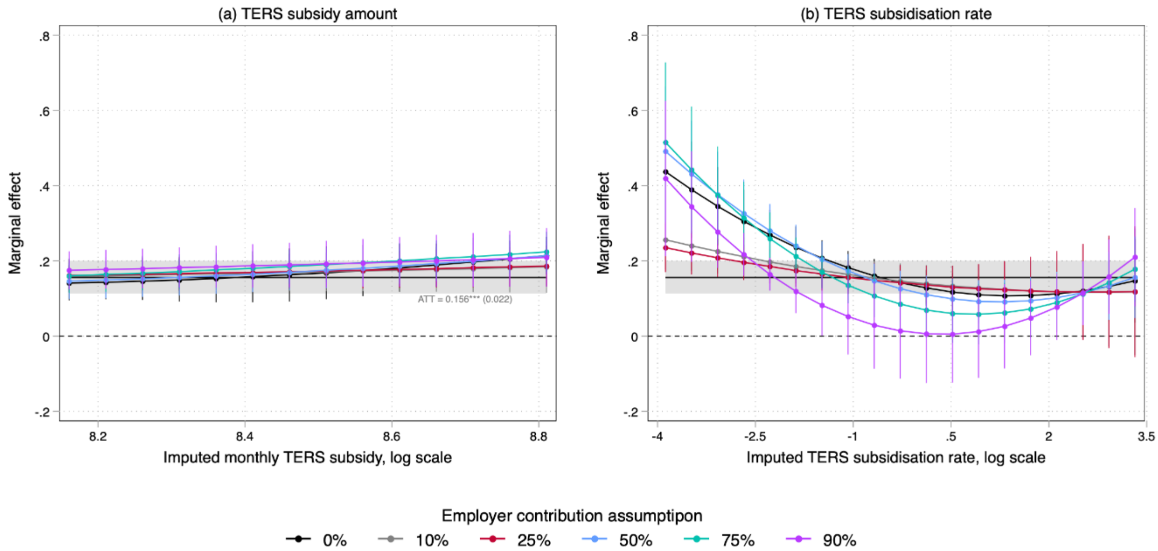
	Main results	Imputations			
	m=5	m=2	m=10	m=20	m=50
	(1)	(2)	(3)	(4)	(5)
Panel (a): Linear interaction					
$D_i \times log(wage)_i$	0.018	0.025	0.021	0.021	0.019
	(0.015)	(0.027)	(0.015)	(0.013)	(0.013)
$D_i \times log(subsidisation\ rate)_i$	-0.020	-0.030	-0.025	-0.024	-0.021
	(0.021)	(0.037)	(0.021)	(0.018)	(0.018)
Panel (b): Quadratic interaction					
$D_i \times log(wage)_i$	-0.104	-0.078	-0.090	-0.089	-0.105*
	(0.069)	(0.100)	(0.082)	(0.068)	(0.062)
$D_i \times log(wage)_i^2$	0.008*	0.006	0.007	0.007*	0.008**
	(0.004)	(0.005)	(0.005)	(0.004)	(0.004)
$D_i \times log(subsidisation\ rate)_i$	-0.038***	-0.041***	-0.039***	-0.037***	-0.038***
	(0.008)	(0.011)	(0.010)	(0.009)	(0.009)
$D_i \times log(subsidisation\ rate)_i^2$	0.013**	0.011	0.012**	0.012**	0.013***
	(0.005)	(0.006)	(0.006)	(0.005)	(0.005)
Panel (c): Cubic interaction					
$D_i \times log(wage)_i$	-0.664*	-0.669	-0.699	-0.662*	-0.687*
	(0.387)	(0.556)	(0.426)	(0.387)	(0.387)
$D_i \times log(wage)_i^2$	0.079	0.079	0.084	0.079	0.082*
	(0.049)	(0.071)	(0.053)	(0.048)	(0.049)
$D_i \times log(wage)_i^3$	-0.003	-0.003	-0.003	-0.003	-0.003
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)
$D_i \times log(subsidisation\ rate)_i$	-0.055**	-0.059	-0.058**	-0.055***	-0.056***
	(0.022)	(0.037)	(0.023)	(0.019)	(0.020)
$D_i \times log(subsidisation\ rate)_i^2$	0.011**	0.011	0.010*	0.009**	0.010**
	(0.005)	(0.006)	(0.005)	(0.005)	(0.004)
$D_i \times log(subsidisation\ rate)_i^3$	0.003	0.004	0.004	0.004	0.004
	(0.004)	(0.006)	(0.004)	(0.003)	(0.003)

Note: this table presents the estimates of the interaction terms from the models specified in Equation (3) of the heterogenous effects of TERS eligibility on job retention by the interaction term used, the polynomial order of the interaction term, and the number of wage imputations (m) used in the multiple imputation procedure. Coefficients on D_i alone are omitted for brevity but are available from the authors upon request. Sample restricted to the employed working-age population (15–64 years) in the formal private sector as of 2020Q1. All models control for individual fixed effects (FE) and the vectors of observable demographic and labour market covariates $\boldsymbol{X}_{it}$. All estimates are weighted using sampling weights and take account of the complex survey design and multiply imputed wage data. Standard errors presented in parentheses and are clustered at the panel (individual) level. *** p < 0.01, ** p < 0.05, * p < 0.10.

Source: author's calculations based on QLFS 2020Q1 and 2020Q2 (StatsSA 2020b, 2020c).

Our final robustness test aims to investigate the impact of the TERS in the presence of varying employer contributions. To this point, all estimates and robustness tests have assumed that employers have not contributed to employees' wages, and that the TERS benefit was solely responsible for covering lost wages during the pandemic. Unfortunately, data on these employer contributions is not available, and so the size (and subsequent effect) of the TERS benefit may have been slightly overestimated due to the assumption of zero employer contributions. Although the incentive-compatibility structure of the programme may have acted to discourage employer contributions to workers' wages, it is clear that some firms did make substantial contributions to employees' wages. Thus, we simulate the impact of the TERS subsidy in the presence of varying employer contributions to wages. As part of this exercise, we assume a unilateral percentage employer contribution across all workers in our sample and re-estimate the effect of the TERS subsidy when employers contributed each of 10, 25, 50, 75, and 90 per cent of workers' wages. This exercise acts as a type of bounding mechanism on the effects of the TERS subsidy, and we present the results of our estimation in Figure 5.

Figure 5: Marginal effect of TERS subsidy across the TERS benefit and subsidy rate distribution, by employer contribution assumption



Source: author's calculations based on QLFS 2020Q1 and 2020Q2 (StatsSA 2020b, 2020c).

After accounting for varying employer contributions, we find that our results across the distribution of TERS benefit amounts are highly consistent: In all cases, those individuals receiving larger TERS subsidies see a larger job-saving effect relative to those individuals with lower TERS subsidy amounts. This is consistent with a marginally regressive effect, where the TERS was more effective in saving jobs amongst higher-wage workers; however, we note that the impact of the TERS remains positive, and close to the average estimated effect in all cases, indicating that the TERS did still have positive job-saving effects for all, no matter the level of employer contribution.

On the other hand, panel (b) depicts the marginal effect of the TERS subsidy on employment retention across the subsidization rate scale. Here, we note that once again, our results are slightly regressive, which is consistent with our previous findings. However, here there is slightly more divergence in our estimated effects by employer contribution level. Specifically, we see that as the subsidization rate increases to 0 on the log scale (equivalent to 100 per cent subsidization), the effect of the TERS seems to attenuate towards zero as employer contributions increase. This seems sensible as low subsidization rates correspond to high-wage earners (since the TERS would have subsidized a lower proportion of their overall wages), and as the subsidization rate rises, wages begin to fall. Specifically, in the middle of the subsidization rate distribution, as employer contributions begin to increase, the sum of employer contributions and TERS benefits start to exceed the workers' monthly wages, and as a result, the actualized TERS benefit for these individuals becomes smaller in absolute magnitude. Thus, expectedly, as the absolute value of the TERS benefit decreases, the effect of the TERS is attenuated. However, for subsidization rates above 100 per cent, we start to find positive impacts of the TERS once again across all employer contribution assumptions, since at this point individuals may begin to earn below the ZAR3,500 (US$491 PPP) monthly take-home-pay threshold. As a result, the TERS begins to act as a top-up for wages, and the absolute size of the subsidy once again increases, having larger impacts. It is worth noting, however, that the size of the impacts for low-wage earners (i.e. those on the far-right of the subsidization distribution) are lower than those for high-wage earners (i.e. those on the far-left of the subsidization distribution), once again highlighting the slightly regressive results of the TERS subsidy.

7 Conclusion

Wage subsidies were one of the primary policies used by governments around the world to mitigate job losses in response to the COVID-19 pandemic. Several studies exist which estimate the causal effects of these policies on job retention in developed countries, however to our knowledge no evidence exists for developing country contexts. In this paper, we provide the first set of causal effect estimates of wage subsidies on job retention in a developing country during the pandemic, using South Africa as an interesting case study given its high unemployment context. To do so, we use unique panel labour force survey data and exploit a temporary institutional eligibility criterion of the country's Temporary Employer/Employee Relief Scheme (TERS) policy to adopt a difference-in-differences design.

Our analysis yields three sets of findings. First, our preferred specification shows that the policy increased the probability of remaining employed in the short term by just under 16 percentage points, which is robust to a battery of robustness tests. Second, we show that this effect is not only positive but economically meaningful. Adjusting our estimate by a scaling factor indicates that the policy saved 2.7 million jobs during the two-month period, suggesting that job loss in South Africa would have been much more severe at the pandemic's onset in the absence of the policy. Our findings also imply that two thirds of TERS recipients were inframarginal and would have remained employed anyway in the policy's absence. This likely reflects the initial prioritization of rapid disbursement of relief over accurately targeting those most in need, which is not unique to South Africa's context. Combined with expenditure data, this implies an average monthly cost of ZAR13,195 (US$1,850 PPP) per job saved. This compares favourably to more developed contexts, however it still indicates that expenditure on the TERS exceeded the wage costs of jobs supported by it. Third, while effects remain positive and large even if only a small share of wages were subsidized, we show that the policy's effects were positive but marginally regressively distributed across the subsidy distribution. This result holds when subjected to varying employer contribution assumptions, indicating that even in a scenario where employers acted to 'co-insure' workers' wages during the pandemic, the policy still served as an important job-saving tool for the South African labour market.

Our analysis makes several contributions. While it adds to the now large literature on the labour market effects of the COVID-19 pandemic, it specifically contributes to the literature on the labour market effects of government interventions during a crisis (Mian and Sufi 2012; Agarwal et al. 2017; Zwick and Mahon 2017), the employment effects of wage subsidies in general (Card and Hyslop 2005; Kangasharju 2007; Betcherman et al. 2010; Groh et al. 2016) and during a crisis in particular (Bruhn 2020). However, its key input lies in its provision, to our knowledge, of the first set of estimates of the job retention effects of wage subsidies during the COVID-19 pandemic in a developing country context. In doing so, we show that effects are in line with the developed country consensus that wage subsidies can be particularly useful during periods of large, transient shocks, despite tending to have only modest effects in non-crisis periods (McKenzie 2017; Bruhn 2020).

Our analysis is not without its limitations; namely, we are only able to estimate short-term effects, and our identification relies on eligibility. Future work ought to consider gaining access to administrative data, such as the Unemployment Insurance Fund's database, to examine longer-term effect dynamics as the policy was extended and eligibility was restricted to vulnerable sectors, and how these changes affected estimates of cost-effectiveness. Finally, our study focuses exclusively on employment effects, but a more complete evaluation would include a broader set of outcomes.

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Appendix

Table A1: Covariate balance table at baseline using an alternative dataset, by TERS receipt status in April 2020

	(1) TERS non-recipients	(2) TERS recipients	(3) Difference
Age (years)	38.147	37.442	-0.705
	(10.662)	(9.517)	(1.029)
Female	0.438	0.393	-0.046
	(0.496)	(0.488)	(0.049)
African/Black	0.760	0.746	-0.014
	(0.427)	(0.435)	(0.047)
Urban	0.856	0.823	-0.033
	(0.351)	(0.382)	(0.041)
Primary education or less	0.086	0.075	-0.011
	(0.280)	(0.263)	(0.022)
Incomplete secondary education	0.319	0.291	-0.028
	(0.466)	(0.454)	(0.045)
Complete secondary education	0.229	0.288	0.059
	(0.420)	(0.453)	(0.046)
Tertiary education	0.366	0.347	-0.019
	(0.482)	(0.476)	(0.049)
High-skilled	0.261	0.161	-0.099**
	(0.439)	(0.368)	(0.043)
Semi-skilled	0.531	0.687	0.155***
	(0.499)	(0.464)	(0.049)
Less-skilled	0.208	0.152	-0.056
	(0.406)	(0.359)	(0.036)
Written contract	0.660	0.816	0.155***
	(0.474)	(0.388)	(0.042)
Weekly working hours	40.268	41.469	1.201
	(15.740)	(11.617)	(1.237)

Note: this table presents estimates of mean values for observable covariates by TERS receipt status in April 2020 accompanied by inter-group differences in means. Sample restricted to the working-age population (15–64 years). All estimates are weighted using sampling weights. Standard errors presented in parentheses and are clustered at the panel (individual) level. The magnitude and statistical significance of a given difference are estimated using t-tests. *** p < 0.01, ** p < 0.05, * p < 0.10.

Source: author’s calculations based on NIDS-CRAM (2020).