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The effect of wage subsidies on job retention

Evidence from South Africa during the COVID-19 pandemic

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Abstract: Wage subsidies have served as a primary labour market policy used around the world to mitigate job losses in response to the COVID-19 pandemic. In South Africa, where unemployment is among the highest globally, the Temporary Employer–Employee Relief Scheme supported millions of workers in a far-reaching and progressive manner. We make use of unique labour force panel data to estimate the causal effect of the policy on short-term job retention among formal private sector workers, who represent the majority of workers in the country, by exploiting a temporary institutional eligibility detail and estimating a difference-in-differences model. We find that the policy increased the probability of remaining employed by 16 percentage points in the short-term. This finding holds when subjected to several robustness tests. We further estimate heterogeneous and progressive effects across the wage distribution with larger effects observed for lower-wage workers, against a backdrop of regressively distributed job loss in the country. Our analysis provides evidence on the role of wage subsidies in the mitigation of job loss during crises in developing countries.

Key words: COVID-19, South Africa, labour market, wage subsidy, job loss, Temporary Employer–Employee Relief Scheme

JEL classification: J08, J38

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1   Introduction

Wage subsidies have served as one of the primary labour market measures used by governments around the world to mitigate job losses in response to the COVID-19 pandemic. 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). By January 2022, 60 per cent of countries globally had introduced the most popular form of job retention policy: a wage subsidy that is typically used to subsidize hours worked or top-up wages (Gentilini et al. 2022). Indeed, although existing estimates on the employment effects of wage subsidies generally are modest at best, evidence suggests they may be particularly useful during conditions of large, temporary shocks (McKenzie 2017).

In South Africa, where unemployment is among the highest in the world (35.3 per cent as of the end of 2021), such policy has arguably served as the government's most important labour market intervention during the pandemic. Since its inception in April 2020, the Temporary Employer–Employee Relief Scheme (TERS)—a wage subsidy that supported employers who fully or partially closed their operations—benefited 5.7 million workers (61–70 per cent[1] of the formal, private employed population in 2020) at a cost of 64 billion South African Rand (ZAR) (approximately US$4 billion at the time of writing) as of April 2022 (Nxesi 2022). Although minimal, existing research on the policy has highlighted its progressive coverage. Köhler and Hill (2022) show that benefits were higher in relative terms for lower-wage workers and that, over time, receipt increased among several groups of vulnerable workers. The authors also show that TERS receipt is strongly and positively correlated with job retention during the beginning of the pandemic (Köhler and Hill 2022). Van der Berg et al. (2022) find a negative relationship between TERS receipt and household food insecurity during the pandemic. Using a tax–benefit microsimulation model, Barnes et al. (2021) simulate that mean disposable income in South Africa decreased by 11 per cent between March and June 2020, but that in the absence of the government's support policies (inclusive of the TERS), this rate of reduction may have been more than double (25 per cent).

Despite the existing 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 in the South African labour market during the pandemic. To the best of our knowledge, the only study that has investigated this relationship is Köhler and Hill (2022); however, their findings are only descriptive in nature. In this paper, we estimate the short-term causal effect of the TERS on job retention—that is, the probability of remaining employed—by making use of unique panel labour force data and exploiting a temporary institutional eligibility detail at the beginning of the policy that allows for a unique opportunity to use a quasi-experimental econometric technique—a difference-in-differences (DiD) model—to infer causality. Specifically, our approach compares the job retention probabilities of comparable TERS-eligible and TERS-ineligible workers from before to after the policy was introduced, covering the first half of 2020. Additionally, we hypothesize and test for the existence of effect heterogeneity across the wage distribution; specifically, that lower-wage workers experienced larger effects given the progressive design of the policy's benefit formula. We check the robustness of these results by conducting several placebo tests, making use of alternative indicators of progressivity that are correlated with wages but do not exhibit the same underlying

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[1] Calculated using microdata from Statistics South Africa's (StatsSA) Quarterly Labour Force Survey (QLFS) for all four quarters of 2020 (i.e. 2020Q1–2020Q4).

data quality issues, and controlling for eligibility for another labour market policy introduced in the same period.

We find evidence of a statistically significant and positive effect of the TERS policy on job retention in the short-term, a finding that is robust to model specification. Our preferred estimate suggests the policy increased the probability of remaining employed by just under 16 percentage points, significant at the 1 per cent level. This finding holds when subjected to the robustness tests mentioned above and may be interpreted as an upper-bound estimate. Furthermore, our results support our hypothesis that the design of the policy resulted in progressive outcomes. We find larger job retention effects of up to 26.6 percentage points for lower-wage workers and find no evidence of any effect among the top 40 per cent of workers. This finding holds when alternative indicators of progressivity are used, with larger estimated effects for workers who work in less-skilled occupations, have lower levels of formal education, and work in the primary and secondary sectors. This evidence of progressivity is noteworthy considering the regressivity of pandemic-induced job loss in South Africa. Overall, these findings provide evidence on the role of wage subsidies in the mitigation of job loss in the context of crises in developing countries to ultimately aid labour market recovery, and that if designed appropriately, show how they could provide protection to workers who are most at risk of job loss.

The rest of this paper is structured as follows. In Section 2 we provide an overview of the TERS policy and a related discussion on causal inference. In Section 3 we discuss the data, our identification strategy, and model specifications. We present our results in Section 4 and in Section 5 we conclude.

2   Background to the TERS in South Africa

2.1   Policy overview

On 27 March 2020, South Africa entered a national lockdown to curtail spread of COVID-19. This initial lockdown lasted until the end of April 2020 and was relatively stringent by international standards (Gustaffson 2020), entailing school closures, a curfew, and international and domestic travel controls to name a few. Only workers in occupations deemed essential for economic function and pandemic response were permitted to work in their usual workplace. 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 national state of disaster was terminated resulting in the repeal of this strategy.

The South African government introduced the TERS policy at the end of March 2020 to provide wage support to employers and mitigate the extent of job loss likely to occur due to the pandemic and national lockdown. The initial directive was issued on 25 March 2020—two days after the national lockdown was announced, and two days before it commenced. Administered by the Unemployment Insurance Fund (UIF),[2] the TERS served as a wage subsidy for 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 lockdown. For the first two months of

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

the policy, however, only UIF-contributing workers were eligible to claim benefits. Most workers[3] in South Africa are obligated by law to be registered and contribute to the UIF, so this pool of eligible workers included most workers (8.5 million, or 52 per cent of employment as of the first quarter of 2020);[4] however, it excluded the informal sector and UIF non-contributing formal sector workers. 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 drive for new beneficiaries (Gronbach et al. 2022).

Initially, 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.[5] Employers who applied on behalf of their workers were then liable to pay workers the relevant benefit within two days and submit proof of payment to the UIF within five days. To encourage transparency of this process, the UIF published a list of employers who had received benefits in the public domain so that workers could follow up on any delayed payments directly. In May 2020, direct payments into workers' bank accounts were implemented, although application for these benefits still needed to be received from the employer (AGSA 2020a). Despite initial pay-out delays owing to large application backlogs and technical infrastructure breakdowns (Jain et al. 2020), most benefits (approximately 96 per cent) were paid within 30 days of application (AGSA 2020a, 2020b).

As mentioned, initially the TERS was restrictive in its coverage, allowing only UIF-contributing workers to claim benefits. Following legal challenges, this limited eligibility criterion, which is important to our identification strategy, was later relaxed and 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. After May 2020, the TERS was subject to various additional extensions and amendments ultimately terminating with a final claim period that ended on 25 July 2021. Gronbach et al. (2022) show that these extensions exceeded the mean duration of COVID-19 response policies in sub-Saharan Africa, in part because of South Africa's long-standing political support for social protection. At the time of writing, although the TERS claim period is over, payments of TERS benefits are still being actioned, provided that applications received are valid for the claim period. Figure 1 presents a summarized timeline of the TERS policy lifespan from its introduction in April 2020 through to its end in July 2021.[6]

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[3] 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.

[4] Authors' calculations using weighted microdata from StatsSA's QLFS for 2020Q1 (se StatsSA 2020b).

[5] An exception to this is when an employer employs fewer than 10 workers. In this case, the UIF paid these workers directly during this period.

[6] Readers interested in a fuller description of the TERS timeline are referred to Section 2 of Köhler and Hill (2022).

Figure 1: Timeline of the TERS in the context of South Africa's national lockdown

23 Mar 2020 President announces national lockdown	**27 Mar 2020** Level 5 lockdown commences	**26 May 2020** Amended TERS Directive published (non-UIF workers eligible)	**21 Jul 2020** TERS extended for 1 Jul − 15 Aug claim period	**7 Sep 2020** TERS extended for 16 Aug − 15 Sep claim period	**11 Nov 2020** TERS extended for 16 Sep − 15 Oct claim period	**11 Feb 2021** TERS extended for 16 Oct '20 − 15 Mar '21 claim period for certain workers	**3 Mar 2021** TERS Extension Directive published	**31 May 2021** Adjusted level 2 lockdown commences	**28 June 2021** Adjusted level 4 lockdown commences	

25 Mar 2020 Initial TERS Directive issued; various amendments thereafter	**1 May 2020** Level 4 lockdown commences	**1 Jun 2020** Level 3 lockdown commences	**17 Aug 2020** Level 2 lockdown commences	**21 Sep 2020** Level 1 lockdown commences	**28 Dec 2020** Adjusted level 3 lockdown commences	**1 Mar 2021** Adjusted level 1 lockdown commences	**20 Apr 2021** Substitute TERS Directive published (clarifying the eligible)	**15 June 2021** Adjusted level 3 lockdown commences	**15 July 2021** TERS extended for 16 Mar − 25 July '21 claim period for certain workers	

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

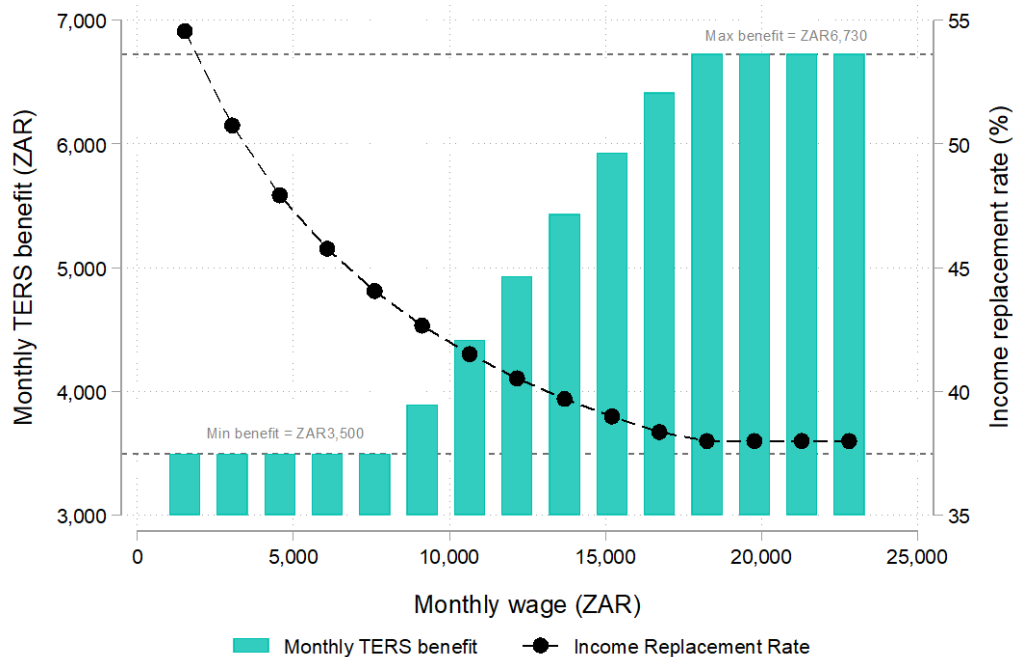TERS benefits are calculated according to the usual unemployment insurance benefit formula as laid out in Schedule 2 of South Africa's Unemployment Insurance Act (No. 63 of 2001) as amended by the Unemployment Insurance Amendment Act (No. 32 of 2003). The formula for calculating an individual's benefit amount is as follows:

$$Benefit_i = IRR_i \times w_i = \left\{ 29.2 + \frac{7,173.92}{232.92 + w_i} \right\} w_i \qquad (1)$$

Equation 1 shows that the TERS benefit for individual i is a function of an income replacement rate (IRR_i), which is fixed to vary between 38 and 60 per cent as a function of the individual daily wage (w_i), with a maximum wage threshold of ZAR17,712 (US$2,541 in purchasing power parity (PPP) terms) per month. Benefits are also lower-bounded by the monthly equivalent of the national minimum wage, implying a minimum benefit of ZAR3,500 (US$502 PPP) per month (regardless of whether their calculated benefit falls below this amount). This means that benefits range between ZAR3,500 per month and ZAR6,730.56 (US$966 PPP) per month.[7] Figure 2 presents a visual summary of how benefits are calculated for workers. Due to the progressive structure of the income replacement rate and the bounds on benefit values, it is unsurprising that lower-wage workers receive larger benefits in relative terms whereas higher-wage earners receive larger benefits in absolute terms. Considering this progressive aspect of the policy design, it is plausible that the TERS may have resulted in a heterogeneous impact on job retention across the wage distribution, with larger effects being exhibited for lower-wage workers. We investigate this hypothesis in our analysis to follow.

Figure 2: Simulation of the calculation of TERS benefits

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

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[7] The TERS may only cover the cost of salaries and no other firm expense. Employers are permitted to supplement the TERS support, but not if this results in workers earning more than 100 per cent of their wage. This is, of course, subject to the caveat that workers whose calculated benefits fall below the ZAR3,500 threshold would be paid ZARR3,500 as a minimum benefit (which could result in them earning over 100 per cent of their wage).

2.2 Understanding the causal effect of the TERS policy

Given the primary aim of the TERS, determining whether and to what extent the policy protected workers from job loss is a key research objective. To the best of our knowledge, the only notable study investigating the relationship between TERS receipt and job retention was conducted by Köhler and Hill (2022). The authors used a propensity score matching technique on panel data collected during the lockdown period in South Africa (all five waves of the National Income Dynamics Study–Coronavirus Rapid Mobile Survey) to find that TERS recipients in April 2020 were 18.1 percentage points more likely to have remained employed in June 2020 relative to non-recipients, all else equal. However, the authors found no evidence of any significant relationship for the period June 2020 to May 2021, corresponding with the period of the gradual reopening of the South African economy. Although these findings are useful in providing suggestive evidence towards the success of the policy, they are insufficient to determine whether the TERS itself saved jobs. In part, this lack of causality was to the result of concerns surrounding firm- and worker-level selection into TERS receipt. For instance, Köhler and Hill (2022) suggest that only the most organized or efficient firms may have been able to apply for TERS on behalf of their workers, at least initially. Similarly, it could be that larger firms were more likely to apply as they were likely better resourced to compile and submit applications. In these cases, it is possible that firm-level characteristics—on which the NIDS-CRAM does not have data—may have influenced the relationship between TERS receipt and job retention. Consequently, controlling for firm-level characteristics in some form may be key to correctly estimating the causal effect of the policy.

Regarding worker-level selection into TERS receipt, Köhler and Hill (2022) hypothesize that firms may have prioritized applications for workers who they felt were most indispensable. Given firm-level optimization decisions, it is possible then that there is a correlation between higher job quality and increased TERS receipt. We opt to control for job quality using several indicators informed by research conducted by Monnakgotla and Oosthuizen (2021) on job quality in South Africa.

The present paper extends the work of Köhler and Hill (2022) by providing causal, rather than just correlational, evidence of the effect of the TERS policy on job retention, at the mean but also across the wage distribution. In the section that follows, we discuss our choice of data as well as our identification strategy and model specifications.

3   Data and methodology

3.1   Quarterly Labour Force Survey

We make use of nationally representative, individual-level labour force data from Statistics South Africa's (StatsSA) Quarterly Labour Force Survey (QLFS) for the first two quarters of 2020 (henceforth 2020Q1 and 2020Q2). Conducted since 2008, the QLFS is a cross-sectional household survey that contains detailed information on a wide array of demographic and socioeconomic characteristics and labour market activities for individuals aged 15 years and older and serves as the official source of labour market statistics in South Africa. More information on the survey's design is available via StatsSA (2008). Throughout our analysis, we use the sampling weights available in the data that account for original selection probabilities and non-response and are benchmarked to known population estimates of the entire civilian population of South Africa.

Prior to the COVID-19 pandemic, the QLFS sample of nearly 70,000 individuals living in approximately 30,000 dwelling units were surveyed through face-to-face interviews. However, 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. To facilitate this, and unlike in previous quarters, the sample that was surveyed in 2020Q1 and for which StatsSA had contact numbers was surveyed again in 2020Q2. The result was that the 2020Q2 data included 71 per cent of the 2020Q1 sample, as not all dwelling units could be contacted. This sampling decision resulted in the survey changing from a cross-sectional survey with a rotational panel element to an (unbalanced) longitudinal survey—a unique scenario in the survey's history. This aspect of the data is key to our identification strategy and ability to measure job retention, as detailed later.

Despite the availability of panel data, there are some quality concerns to note. Given the extent of attrition between 2020Q1 and 2020Q2, there is a concern that population estimates using the latter data may suffer from selection bias given characteristic differences between households that could and could not be contacted. StatsSA sought to address this by adjusting the calibrated survey weights using a bias-adjustment procedure that relied on observable characteristics such as age, gender, and race (StatsSA 2020a). However, respondents may still be unobservably different from non-respondents. At the time of writing, an explicit external review of this process has yet to be conducted and would require more information than is available in the public documentation. Additionally, following the onset of the pandemic, the survey's response rate and consequently sample size reduced considerably from approximately 88 per cent (n=67,000) to 57 per cent (n=47,000) (Bhorat et al. 2022). Despite these changes, the weighted population estimates for 2020Q2 appear plausible (Köhler et al. 2021).

3.2 Balanced panel sample representivity

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, those who were employed in 2020Q1 (regardless of their labour market status in 2020Q2). We further restrict the sample to those of working age (15–64 years) in 2020Q1. To identify the balanced panel sample, we make use of household and person identifiers in the data as well as restricting on age, gender, and self-reported racial population group to ensure that we observe the same individual over time.[8] 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. This procedure results in a balanced panel sample of 24,475 individuals, compared with the cross-sectional sample of nearly 42,000 working-age individuals in 2020Q1.

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 in 2020Q1, and we do this for both the cross-sectional and panel sample. We present these estimates in Table 1. 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 and, therefore, we do not expect this to meaningfully impact on any of the panel data results. As such, it appears that our balanced panel sample remains representative of the broader South African population.

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[8] The anonymity of the data prohibits us from accessing other identifying variables of respondents such as names and surnames.

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 are 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).

Job retention serves as the dependent variable in our analysis; therefore, it is useful to consider the representivity 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) in Table 1. Compared with the means in columns (1) and (2), employed individuals in both the panel and the 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; Ranchhod and Daniels 2021; Zizzamia 2020). 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) in Table 1—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 can be relatively confident that our balanced panel sample of individuals employed in the baseline period remains representative of the broader population.

3.3 Wage data adjustments

Our use of the raw, unimputed QLFS wage data provided to us by StatsSA in our heterogeneity analysis, discussed in the section on 'Heterogeneous effects', is important to examine in brief 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 (Bhorat et al. 2021; Kerr 2021; Kerr and Wittenberg 2019a, 2021; Wittenberg 2017). First, the survey collects data on wages before taxation and deductions from all employees, employers, and own-account workers. These workers are asked to report their wages in monetary (South African Rands) terms, and those who do not are then asked to report the bracket or range that their wage falls into. A substantive issue exists in this regard: in the public release of the QLFS wage data from 2010 onwards, StatsSA have included imputations for the wages of workers who did not report them in monetary terms. Unfortunately, the public release documents do not include an explanation on how these imputations were conducted; in fact, wage imputations are never even mentioned. However, an internal document examined by Kerr and Wittenberg (2021) suggests StatsSA used a hot deck imputation method, which the authors argue has been used inappropriately. Unfortunately, the publicly released data does not make it possible to distinguish between the imputed responses and the actual responses.

Several studies have highlighted how the use of this public release wage data produces implausible results. Kerr and Wittenberg (2019a) and Kerr (2021) show that these imputations result in unreliable trends in several measures of wage inequality, including the Gini coefficient, the variance of log wages, and at five different percentiles. Notably, Kerr and Wittenberg (2021) compare estimates from unimputed wage data (obtained privately from Stats SA for 2011 and 2012) with public release data for the same periods. The authors find that the imputed wage data produce unreliable results, but that when the unimputed data are used the results appear to be much more reliable. This suggests that although the quality of the imputations done by Stats SA is questionable, the underlying wage data are not. At present, the unimputed data have not been made available in the public domain.

Considering the questionable quality of the wage data in the public release QLFS, our use of the raw, unimputed wage data from the QLFS for the first quarter of 2020 resolves the data quality issues pertaining to their imputations, discussed earlier. To prepare the unimputed data for analysis, we follow Wittenberg (2017) and Kerr and Wittenberg (2019b) and adjust the data to (i) identify and omit outliers and (ii) address missing values. For (i), we use a studentized regression residual approach by estimating a Mincerian 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 (in our case, this affected just five observations). Two advantages of this approach are the identification of outliers on both tails of the wage distribution, as opposed to only observations with extreme implausibly high values, and the procedure's ability to correct for points of high leverage tending to be associated with smaller residuals (Wittenberg 2017).

Regarding missing values, Wittenberg (2017) discusses how there are two broad approaches for dealing with them: reweighting non-missing values to account for missing ones, or imputing for the missing data, where several methods are available for the latter. We choose to use multiple imputation that is essentially an iterative form of stochastic imputation but has a particular advantage over such methods. Instead of imputing a single value through regression, multiple imputation uses the distribution of the empirical data to estimate multiple values that taken together reflect the uncertainty around the true value. 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. In the relevant wage section of our analysis, we then obtain five sets of coefficients and standard errors that are then combined for inference and appropriately reflect the uncertainty associated with the imputation process. This approach was similarly followed by Kerr and Wittenberg (2019b) in their generation of the Post-Apartheid Labour Market Series dataset—a compilation of individual-level microdata from household surveys conducted between 1993 and 2019 in South Africa.

3.4 Identification strategy

Our main aim in this paper is to estimate the causal effect of TERS receipt on job retention; that is, the probability of remaining employed for those who have their earnings temporarily subsidized by the programme. In the ideal setting, to establish causality we would have randomized assignment of treatment (in this case, TERS receipt). In reality, TERS benefits were not randomly distributed across workers, only workers whose firms' operations fully or partially ceased in response to the pandemic were eligible to apply. A simple comparison of outcomes by TERS receipt status is therefore an inappropriate method for measuring causal impact, as the relationship will likely be confounded by selection bias. Indeed, Köhler and Hill (2022) do show that TERS recipients differ from non-recipients across several key covariates: recipients are more likely to be men, work in semi-skilled occupations, work in the secondary sector, and have a written employment contract, among other factors.

As noted in Section 2, initially only workers who were registered and contributing to UIF were eligible to claim TERS benefits. This temporary institutional eligibility detail presents a unique opportunity to use a quasi-experimental econometric approach (i.e. a DiD model) to estimate the short-term causal effect of TERS receipt on job retention. Many studies make use of similar eligibility criteria as a treatment assignment rule, even if the full treatment group is not actually treated. For example, Ranchhod and Finn (2016) exploit an age eligibility threshold for treatment to estimate the effect of the Employment Tax Incentive in South Africa—a wage subsidy intended to address youth unemployment. This approach is also common to estimate the effects of non-contributory pension receipt in South Africa (Abel 2019; Ardington et al. 2016; Duflo 2003; Etinzock and Kollamparambil 2019; Ranchhod 2006). Our approach using TERS eligibility rather than actual receipt is further motivated by the lack of data on the TERS in the QLFS.[9] Lastly, it should be noted that our identification strategy only allows us to consider effects over the very short-term; that is, our results are focused on the first few months in the life of the policy. As such

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[9] Data on TERS receipt does exist in another panel survey (the NIDS-CRAM); however, this dataset is insufficient for causal inference because in the first wave of the survey, TERS receipt was only asked of those individuals who were employed in April 2020—the first month of South Africa's national lockdown and the TERS policy. If one uses a difference-in-differences (DiD) approach then, not only does no relevant pre-treatment data exist, but data on TERS receipt is endogenous: we can only observe TERS receipt status among those who remained employed in the treatment period. Our use of the QLFS also has other advantages compared with 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 Appendix Table A1, we use the NIDS-CRAM Wave 1 data to 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 TERS eligibility as a proxy measure of TERS receipt for this period is reasonable.

the effects of changes in the TERS eligibility criteria over time (such as allowing non-UIF applicants and then limiting applications by sector) in the latter half of 2020 and in 2021 are beyond the scope of this paper.

Main model

Formally, we estimate the following canonical[10] DiD model specification for individual i in quarter t using ordinary least squares (OLS):

$$Job\ retention_{it} = \alpha + \beta TERS_i + \delta Post_t + \gamma(TERS_i \times Post_t) + \mu X_{it} + \theta P_i + \varphi_i + \varepsilon_{it} \tag{2}$$

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 those in 2020Q1. $TERS_i$ is our treatment variable indicating eligibility for 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,[11] we only include employees in our sample who represent the majority (84.2 per cent) of working-aged workers in the country. Furthermore, it should be noted that UIF contribution is highly correlated with employment formality in the South African labour market. In 2020Q1, under StatsSA's definition that specifies formal sector workers as those who pay income tax and those who do not but work for establishments that employ at least five workers, 71 per cent of formal sector workers were UIF contributors in contrast to just 23 per cent of informal sector workers. 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, so why do we observe non-contributing workers in the formal sector in the data? The reason largely has to do with tax and UIF registration exemptions in the relevant legislation. Specifically, 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 working for an employer for less than 24 hours per month, working for national or provincial government, being a foreign worker on contract, or only working for a

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[10] A new theoretical and empirical literature on the econometrics of DiD has developed in recent years, with most contributions highlighting how in practice many typical applications of this approach do not meet all requirements of the canonical DiD setup (two treatment groups and two periods), due to the presence of more than two periods and 'staggered' treatment timing (for a recent review, see Roth et al. 2022). We recognize the concerns raised in this literature, but they are not relevant for our identification strategy, given that we only have two periods and every treated observation is treated at the same period.

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

commission. As such, we believe our control group of UIF non-contributors in the formal private sector likely consists of workers who earn below the income tax threshold and work for less than 24 hours per month. We consider these characteristics in the balance test to follow.

In Equation 2, $\boldsymbol{X}_{it}$ is a vector of observable demographic and labour market covariates that we control for to account for possible confounding variables, reduce the residual variance, and improve the precision of the estimates. These covariates 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 1-digit level, main industry at the 1-digit level, firm size, contract duration (permanent, limited, or unspecified duration), weekly working hours, binary indicators for being a trade union member, being registered for income tax, 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 of health insurance (medical aid) 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 that identifies whether a worker began their job within the last 6 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 fixed effects (FEs)—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, due to prevailing variation in the DiD interaction term. γ then serves 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 TERS policy in the short-term. ε_{it} is the regression 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.

Although similar mean levels of covariates between the treatment and control group at baseline is not a requirement in DiD designs, the validity of our identification strategy may be threatened if these underlying characteristics differ significantly. If so, differences in job retention probabilities from before to after the onset of the policy may be explained by these characteristic differences rather than the policy itself. Table 2 presents mean estimates across a range of covariates by treatment status in the baseline period. Relative to ineligible workers, eligible workers are on average 2 years older and are more likely to be living in urban areas, be married or be living together with a partner, have higher levels of formal education, work in a higher-skilled occupation, be a trade union member, have a written contract, have a higher quality job (as presented by pension fund contribution, health insurance, and paid leave), and work in larger firms, and are less likely to be African/Black. Due to exemptions in tax legislation discussed earlier (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 TERS-eligible workers are nearly three times more likely to be registered for income tax (73 per cent compared with 26 per cent) and have higher pre-pandemic wages (approximately ZAR9,800 (US$1,406 PPP) compared with ZAR4,700 (US$674 PPP) per month on average). It is notable that mean working hours are statistically similar across the two groups; however, eligible workers are less likely to work on a part-time basis (defined as less than 27 hours per week).

Table 2: Covariate balance table at baseline, by treatment and adjustment status

	Unadjusted			Propensity score covariate adjusted		
	(1)	(2)	(3)	(4)	(5)	(6)
	TERS-ineligible	TERS-eligible	Difference	TERS-ineligible	TERS-eligible	Difference
Demographic covariates						
Age (years)	36.333	38.756	2.423***	37.834	38.408	0.574
	(10.193)	(10.086)	(0.426)	(0.486)	(0.182)	(0.550)
Female	0.425	0.413	−0.011	0.413	0.415	0.002
	(0.494)	(0.492)	(0.021)	(0.024)	(0.009)	(0.028)
African/Black	0.842	0.689	−0.154***	0.716	0.713	−0.002
	(0.365)	(0.463)	(0.017)	(0.020)	(0.008)	(0.022)
Urban	0.698	0.825	0.127***	0.810	0.804	−0.006
	(0.459)	(0.380)	(0.019)	(0.019)	(0.007)	(0.022)
Married or living together with a partner	0.437	0.528	0.091***	0.515	0.511	−0.004
	(0.496)	(0.499)	(0.021)	(0.024)	(0.009)	(0.027)
Primary or less	0.097	0.064	−0.033***	0.061	0.071	0.009
	(0.296)	(0.245)	(0.012)	(0.012)	(0.005)	(0.014)
Secondary incomplete	0.406	0.304	−0.102***	0.363	0.313	−0.050*
	(0.491)	(0.460)	(0.020)	(0.023)	(0.008)	(0.026)
Secondary complete	0.339	0.423	0.083***	0.388	0.413	0.025
	(0.474)	(0.494)	(0.020)	(0.023)	(0.009)	(0.026)
Post-secondary	0.158	0.209	0.052***	0.188	0.203	0.015
	(0.364)	(0.407)	(0.016)	(0.022)	(0.008)	(0.025)
Labour market covariates						
Primary sector	0.112	0.110	−0.001	0.107	0.111	0.004
	(0.315)	(0.313)	(0.013)	(0.014)	(0.005)	(0.015)
Secondary sector	0.274	0.252	−0.023	0.288	0.249	−0.040*
	(0.446)	(0.434)	(0.019)	(0.021)	(0.008)	(0.024)
Tertiary sector	0.614	0.638	0.024	0.605	0.640	0.035
	(0.487)	(0.480)	(0.021)	(0.023)	(0.009)	(0.026)
High-skilled	0.076	0.139	0.063***	0.126	0.129	0.003
	(0.264)	(0.346)	(0.013)	(0.018)	(0.007)	(0.020)
Semi-skilled	0.650	0.660	0.010	0.622	0.666	0.045*
	(0.477)	(0.474)	(0.020)	(0.024)	(0.009)	(0.027)
Less-skilled	0.274	0.201	−0.073***	0.252	0.205	−0.047**
	(0.446)	(0.400)	(0.018)	(0.020)	(0.007)	(0.022)
Union member	0.081	0.305	0.224***	0.178	0.288	0.110***
	(0.273)	(0.460)	(0.013)	(0.017)	(0.008)	(0.020)
Written contract	0.556	0.981	0.426***	0.921	0.911	−0.010
	(0.497)	(0.135)	(0.019)	(0.010)	(0.003)	(0.011)
Weekly working hours	44.000	44.310	0.310	44.300	44.247	−0.054
	(14.717)	(9.533)	(0.576)	(0.578)	(0.187)	(0.648)
Part-time workers	0.086	0.026	−0.060***	0.042	0.034	−0.008
	(0.281)	(0.158)	(0.011)	(0.012)	(0.004)	(0.013)
Pension fund	0.149	0.637	0.488***	0.473	0.575	0.102***
	(0.356)	(0.481)	(0.016)	(0.021)	(0.008)	(0.024)
Paid leave	0.308	0.851	0.542***	0.699	0.776	0.077***
	(0.462)	(0.356)	(0.019)	(0.020)	(0.006)	(0.024)
Health insurance	0.105	0.295	0.190***	0.161	0.286	0.125***
	(0.306)	(0.456)	(0.015)	(0.020)	(0.008)	(0.023)
Firm size: <10 workers	0.287	0.100	−0.187***	0.145	0.127	−0.019

	(1)	(2)	(3)	(4)	(5)	(6)
	(0.452)	(0.299)	(0.018)	(0.019)	(0.006)	(0.021)
Firm size: 10–49 workers	0.392	0.350	−0.042**	0.362	0.357	−0.005
	(0.488)	(0.477)	(0.021)	(0.024)	(0.009)	(0.027)
Firm size: ≥50 workers	0.294	0.480	0.186***	0.451	0.451	0.000
	(0.456)	(0.500)	(0.020)	(0.023)	(0.009)	(0.026)
Firm size: Don't know	0.027	0.071	0.043***	0.042	0.065	0.023*
	(0.163)	(0.256)	(0.009)	(0.011)	(0.005)	(0.012)
Registered for income tax	0.259	0.734	0.474***	0.588	0.670	0.082***
	−0.438	−0.442	(0.019)	(0.021)	(0.008)	(0.025)
Pre-pandemic monthly wage (2020Q1, nominal South African Rands)	4,695.58	9,775.43	5,079.85***	6,738.08	9,396.75	2,658.67***
	(1,380.15)	(2,441.14)	(1,160.68)	(2,466.89)	(2,248.46)	(642.44)

Note: this table presents estimates of mean values for observable covariates by treatment group in the baseline period (2020Q1) accompanied by intergroup differences for the balanced panel sample. Columns (1)(3) present the estimates, while Columns (4)–(6) present the propensity score adjusted estimates, adjusted by estimating a linear regression using OLS of the covariate on the UIF-contributor indicator controlling for the estimated propensity scores and then predicting the fitted values. Sample restricted to the working-age population (15–64 years). All estimates are weighted using sampling weights. Standard errors are 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. ***$p<0.01$, **$p<0.05$, *$p<0.10$.
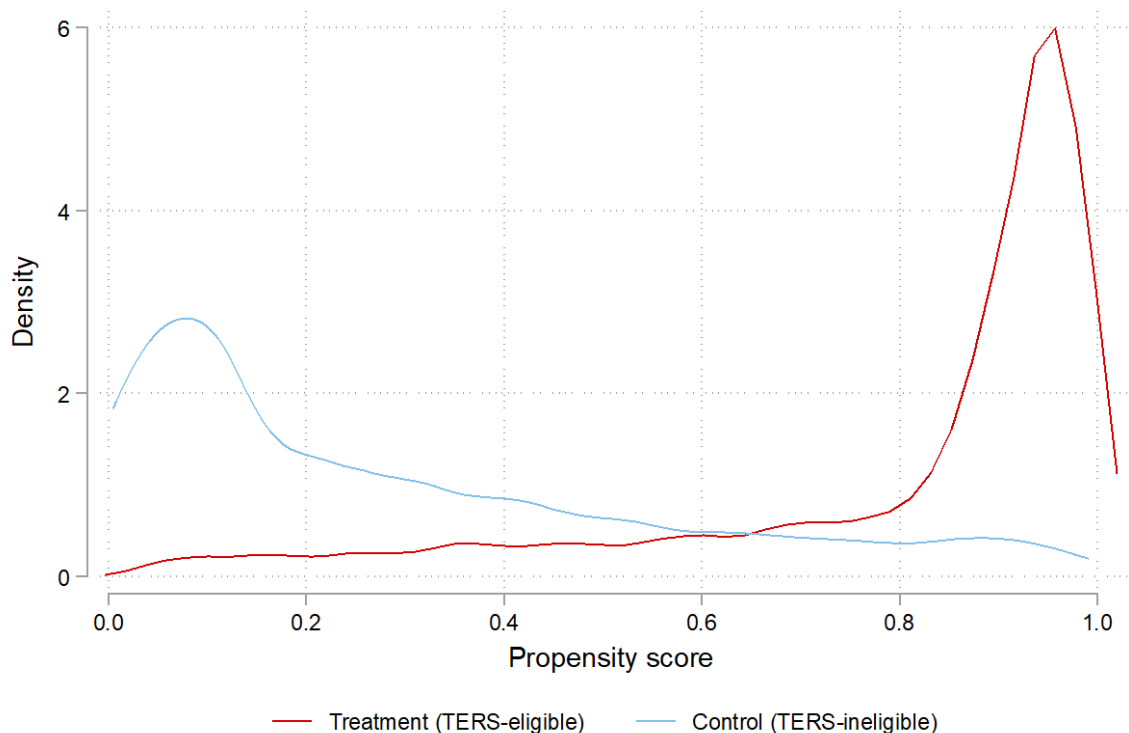
Source: authors' calculations based on QLFS 2020Q1 (StatsSA 2020b).

Considering these significant differences, we undertake a propensity score matching (PSM) approach that allows us to control for the conditional probability of TERS eligibility in our DiD models. We use a logit model to predict TERS eligibility probabilities (propensity scores) conditional on the aforementioned vector of observable covariates $\boldsymbol{X}_{it}$ in the baseline period as follows:

$$Pr(P_i) \stackrel{\text{def}}{=} Pr(TERS_{it} = 1 | \boldsymbol{X}_{it}) \tag{3}$$

Thereafter, we use covariate adjustment by simply controlling for these estimated scores in our DiD model described earlier, as represented by P_i in Equation 2. Although PSM can be used in several ways in a regression context, such as inverse probability weighting or stratifying observations on their propensity scores and estimating effects within strata, Elze et al. (2017) show that covariate adjustment can perform just as well as, if not better, than conventional PSM methods. We make use of the same vector of covariates, $\boldsymbol{X}_{it}$, in our propensity score model as we do in our main DiD model to control for covariate differences that may still exist between TERS-eligible and TERS-ineligible workers who exhibit equivalent estimated propensity scores. In Figure 3 we observe sufficient overlap in the distribution of propensity scores by treatment status following our estimation approach, suggesting that the PSM 'common support' technical requirement holds. The concentrations of high scores among the TERS-eligible and low scores among the TERS-ineligible workers are indicative of a reasonably specified propensity score model. In Columns (4)–(6) in Table 2, we again compare the characteristics of the TERS-eligible and TERS-ineligible workers after controlling for these propensity scores. Now, eligible and ineligible workers appear much more observably comparable. We find far fewer statistically significant between-group differences, and the magnitudes of any remaining significant differences are much smaller.

Figure 3: Distribution of estimated propensity scores, by treatment status

Note: this figure presents kernel density estimates of the distribution of propensity scores by treatment status. Here, a propensity score is the probability of being a UIF contributor conditional on a vector of observable covariates in the pre-treatment period (2020Q1).

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

Heterogeneous effects

As discussed in Section 2.1, due to the progressive design of the TERS benefit formula, we hypothesize that the policy may have had heterogeneous effects on job retention across the wage distribution, with larger effects being exhibited among lower-wage workers. In addition to our main model, we investigate this hypothesis by making use of the raw, unimputed wage data from the QLFS, described in detail in Section 3.3, by re-estimating our model in specification (2) but for five distinct sub-samples of workers based on their pre-pandemic (2020Q1) monthly wage quintile. As before, in these models we control for the vector of observable demographic and labour market covariates, X_{it}, the estimated propensity scores, P_i, and individual FEs φ_i.

4 Results

4.1 Main results

In Table 3, we report the main results from our model specified in Equation 2. We consider three sets of specifications that control for different observable and unobservable covariates. First, Columns (1)–(3) present the estimates that only account for observable demographic and labour market covariates. Second, Columns (4)–(6) additionally account for the estimated propensity scores. Finally, Columns (7)–(9) further account for individual FEs, which seek to absorb any unobserved time-invariant heterogeneity.

Table 3: Estimates of the effect of TERS receipt on short-term job retention

	DiD			DiD + PS			DiD + PS + FE		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
TERS	0.000***	−0.010***	−0.061***	−0.048***	−0.050***	−0.061***			
	(0.000)	(0.003)	(0.010)	(0.010)	(0.010)	(0.010)			
Post	−0.321***	−0.322***	−0.275***	−0.322***	−0.324***	−0.275***	−0.323***	−0.317***	−0.266***
	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.022)	(0.022)
TERS×Post	0.175***	0.178***	0.155***	0.176***	0.179***	0.158***	0.178***	0.177***	0.159***
	(0.022)	(0.022)	(0.022)	(0.022)	(0.021)	(0.021)	(0.022)	(0.023)	(0.022)
Controls									
Demographic	N	Y	Y	N	Y	Y	N	Y	Y
Labour market	N	N	Y	N	N	Y	N	N	Y
PS	N	N	N	Y	Y	Y	Y	Y	Y
Individual FE	N	N	N	N	N	N	Y	Y	Y
Constant	1.000***	0.720***	0.849***	0.948***	0.691***	0.849***	1.000***	3.327***	3.801***
	(0.000)	(0.075)	(0.075)	(0.011)	(0.075)	(0.075)	(0.003)	(1.021)	(0.968)
Observations	8,520	8,450	8,303	8,447	8,430	8,284	8,447	8,430	8,284
R^2	0.114	0.129	0.167	0.120	0.134	0.167	0.560	0.562	0.558

Note: TERS, Temporary Employer–Employee Relief Scheme; DiD, difference-in-differences; FEs, fixed effects; PS, propensity score (the conditional probability of being a UIF contributor); UIF, Unemployment Insurance Fund; N, no; Y, yes. This table presents estimates of specification (2). Sample restricted to the employed working-age population (15–64 years) as of 2020Q1. All estimates are weighted using sampling weights. Standard errors are presented in parentheses and are clustered at the panel (individual) level. ***$p<0.01$, **$p<0.05$, *$p<0.10$.

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

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 approximately 16 and 18 percentage points, with all estimates statistically significant at the 1 per cent level. Interestingly, although more precise, this estimate is not substantially different from that found by Köhler and Hill (2022).[12]

Beginning with the first set of specifications, in Column (1) of Table 3 coefficient α (the constant term) is equal to one, which is expected given that we only include employed individuals in the pre-treatment period. The coefficient on $TERS_i$ indicates that TERS-eligible workers were not more likely than TERS-ineligible workers to be employed in the pre-treatment period—this is also expected, and for the same reason. The binary $Post_t$ variable coefficient suggests a 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 on the DiD term, γ, 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, this effect reduces to 15.5 percentage points, whereas the degree of statistical significance remains.

Considering the second set of specifications in Columns (4)–(6) of Table 3, our estimate of interest γ ranges between 15.8 and 17.6 percentage points, and thus appears relatively insensitive to controlling for the estimated propensity scores. The precision of these estimates remains effectively unchanged. The coefficient on the binary $TERS_i$ variable has grown slightly in magnitude but remains relatively close to zero, whereas the coefficient on $Post_t$ is also effectively unchanged. Our preferred estimate in Column (9), which controls for observable demographic and labour market covariates, the estimated propensity scores, as well as individual FEs, indicates that the TERS increased the probability of remaining employed by 15.9 percentage points. 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 earlier.

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[12] 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).

4.2   Heterogeneity results

Although we observe a significant and positive effect on average, it is not clear—and, in fact, it is unlikely—that this effect would be constant for all workers. As discussed previously, the TERS policy was designed progressively to provide proportionally greater wage support to lower-wage workers. We present the results of our heterogeneity analysis in this section, where we estimate effects across the pre-pandemic wage distribution.

In Figure 4, we produce a coefficient plot showing the results of the split-sample estimation procedure outlined in the section on 'Heterogeneous effects'. Overall, these results support the hypothesis that the TERS had larger job retention effects for lower-wage workers. We estimate effects ranging between 12.5 and 26.6 percentage points[13] for the poorest 60 per cent of workers. Although the largest effect is observed for quintile 2 workers, the estimated effects for each quintile group within the poorest 60 per cent of workers do not statistically significantly differ from each other. Moreover, in our preferred models where we control for observable covariates, estimated propensity scores, and individual FEs, we do not find any statistically significant evidence that the policy had any effect for workers in the top 40 per cent. These results suggest that the average short-term job retention effect we observe is driven by effects for lower-wage workers. This finding is noteworthy considering that pandemic-induced job loss in South Africa disproportionately affected lower-wage workers (Köhler and Bhorat 2020; Ranchhod and Daniels 2021). Overall, this implies that the policy was not only successful in the short term but it also provided much-needed support to the workers who required it most.
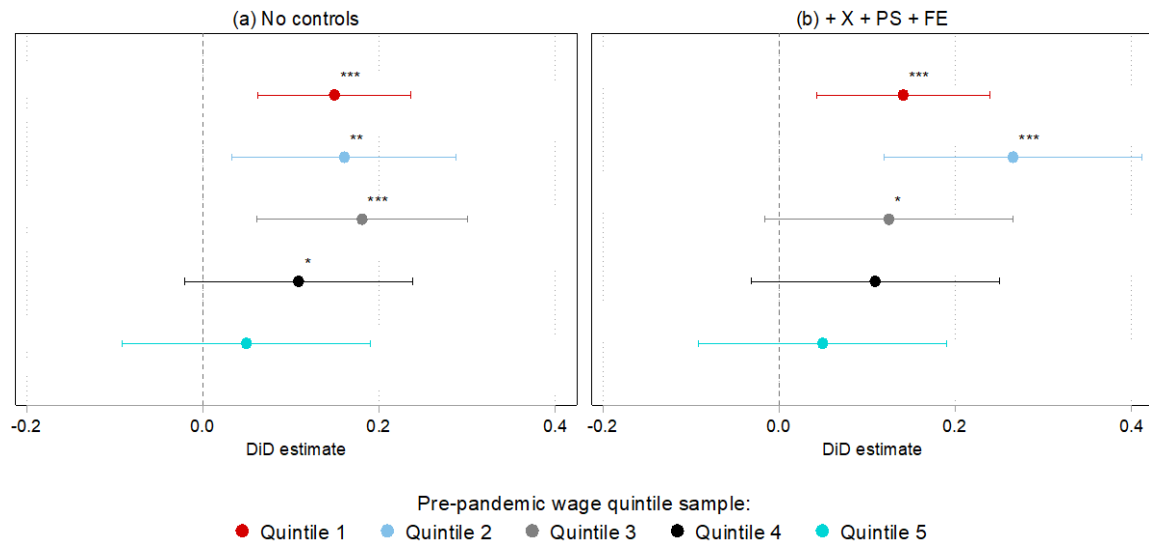
To test the sensitivity of our result, we rerun our split-sample model for several alternative pre-pandemic indicators of progressivity, which are themselves correlated with wages. We make use of the following indicators: employment sector,[14] skill level,[15] and highest education level completed. We present the results of our alternative split-sample models in Table 4. Once again, our results suggest that the TERS was progressive in its effects on job retention. Specifically, for those individuals in the most vulnerable of each of our chosen indicators, the estimated effect is generally large and statistically significant, ranging from 13 to 27 percentage points—larger than the magnitude of the estimated average effect. We further observe that as vulnerability decreases, generally so does the magnitude of the effect, except for skill level where the largest effect is estimated for semi-skilled workers (however, this effect is not statistically significantly different from that for less-skilled workers). For instance, with respect to sector we estimate an effect of 26.7 percentage points for primary sector workers, 14.5 percentage points for secondary sector workers, and 13.7 percentage points for tertiary sector workers. Similarly, we observe larger effects for workers who have lower levels of formal education.

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[13] Point estimates of the results for these models are presented in Appendix Table A2.

[14] Classified according to 1-digit Standard Industrial Classification codes, with agriculture, hunting, forestry, fishing, mining, and quarrying classified as primary; manufacturing, utilities, and construction classified as secondary; and the remainder classified as tertiary.

[15] Classified according to 1-digit occupation codes, with elementary occupations and domestic workers being classified as less-skilled; legislators, senior officials, managers, and professionals being classified as high-skilled; and the remainder classified as semi-skilled.

Figure 4: Coefficient plot of estimates of heterogeneous effects of TERS receipt on short-term job retention, by pre-pandemic hourly wage quintile

Note: this figure presents estimates of the main DiD model for split-samples by pre-pandemic (2020Q1) monthly nominal wage using the multiply imputed wage data. Sample restricted to the employed working-age population (15–64 years) as of 2020Q1. All estimates are weighted using sampling weights. Standard errors are clustered at the panel (individual) level. Capped spikes represent 95 per cent confidence intervals. X refers to X_{it}, PS refers to P_i, and FE refers to φ_i. The model estimates are presented in Appendix Table A2. ***$p<0.01$, **$p<0.05$, *$p<0.10$.

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

Table 4: Estimates of heterogeneous effects of TERS receipt on short-term job retention, by sector, skill level, and highest level of education

	Sector			Skill level			Education		
	Primary	Secondary	Tertiary	Less skilled	Semi-skilled	Highly skilled	Incomplete secondary	Complete secondary	Tertiary
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
TERS×Post	0.267***	0.145***	0.137***	0.129***	0.165***	0.046	0.206***	0.099***	0.053
	(0.073)	(0.041)	(0.028)	(0.046)	(0.027)	(0.059)	(0.035)	(0.034)	(0.045)
Controls									
Demographic	Y	Y	Y	Y	Y	Y	Y	Y	Y
Labour market	Y	Y	Y	Y	Y	Y	Y	Y	Y
PS	Y	Y	Y	Y	Y	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Constant	−2.960	4.725**	4.638***	2.236	4.112***	2.266	5.362***	3.267**	2.692
	(2.472)	(2.193)	(1.139)	(2.213)	(1.165)	(2.423)	(1.757)	(1.463)	(1.770)
Observations	955	2,025	5,304	1,839	5,533	912	3,392	3,352	1,556
R^2	0.597	0.591	0.558	0.606	0.559	0.549	0.588	0.553	0.534

Note: TERS, Temporary Employer–Employee Relief Scheme; PS, propensity score (the conditional probability of being a UIF contributor); FEs, fixed effects; UIF, Unemployment Insurance Fund; Y, yes. This table presents estimates of specification (2) for distinct sub-samples of workers based on pre-pandemic (2020Q1) covariates. Sample restricted to the employed working-age population (15–64 years) as of 2020Q1. All estimates are weighted using sampling weights. Standard errors are presented in parentheses and are clustered at the panel (individual) level. All models control for vectors of demographic controls, labour market controls, and individual FE. ***$p<0.01$, **$p<0.05$, *$p< 0.10$.

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

5   Robustness tests

To ensure that the results presented in Section 4 can be attributable to the TERS policy and are not simply spurious correlations, we undertake several robustness tests. First, as a formal approach to providing support for the parallel trends assumption, we conduct a placebo-period test by rerunning our preferred model specification using data from the QLFS for the same period in 2019. Because this period was prior to the policy being in place, we expect to find no evidence of any effect. We additionally conduct a placebo-outcome test by rerunning our preferred model specification but for outcomes we do not expect should be affected by the policy, such as exogenous worker demographic characteristics including age, race, sex, and highest level of education.[16] When we do so, we omit the relevant variable from our vector of observable demographic control variables. Evidence of an effect here would then suggest that our control group of TERS-ineligible workers must be flawed.

Second, we explicitly consider the expansion of South Africa's Employment Tax Incentive (ETI) programme during the pandemic period and to what extent this programme itself may have affected job retention and hence possibly confound our results. For background, the ETI is a wage subsidy operational in South Africa which aims to reduce youth unemployment. Originally introduced in January 2014, the policy provides employers with payroll tax credits for newly hired workers between the ages of 18 and 29 years who earn no more than ZAR6,500 (US$933 PPP) per month but above the minimum wage (SARS 2022). The ETI can be claimed for a maximum of 2 years following the hire of an eligible worker, and the value of the subsidy decreases after the first 12 months of claim.[17]

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, workers who were eligible for the ETI under the original stipulations would receive an additional ZAR500 (US$72 PPP) per month subsidy on their wages. Second, the programme would provide a ZAR500 subsidy for workers aged 18–29 years for whom the subsidy period had ended previously, or those workers aged 30–65 years earning below ZAR6,500 (US$933 PPP) who were not eligible for the ETI because of their age (National Treasury 2020). This expansion was only available for those employers registered with the South African Revenue Service on 1 March 2020 and scheduled to run for 4 months, ending at the end of July 2020.[18]

Relative to the magnitude of wage subsidies of the TERS, this expansion of the ETI is small but not insignificant in the labour market. While the effect of the ETI on employment outcomes has been the subject of much research, with varying results as to its efficacy, our concern here is that the expansion of this programme coincided with the introduction of the TERS. Because both programmes acted as wage subsidies for overlapping groups of workers at the start of the pandemic, our estimate of the TERS effect may be confounded. To account for the expanded ETI

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[16] We cannot control for individual FEs in these placebo outcome models given that all but one (age) of the outcomes we use are time-invariant.

[17] 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 Bhorat et al. (2020), Budlender and Ebrahim (2021), and Ranchhod and Finn (2016).

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

programme, we control for worker eligibility under the expanded ETI subsidy in our models.[19] This simplifies to including a binary indicator of whether or not a worker earned below ZAR6,500 per month (but above the national minimum wage of ZAR3,500). As we found the effect of the TERS on job retention to be higher among lower-wage workers, it is therefore possible that this result in part may have been the result of the expanded ETI.

The results of these tests are presented in Table 5. As shown in Columns (2)–(7), we find no evidence of an effect on any of the placebo outcomes. With all estimates being statistically insignificant close to zero in magnitude, this result provides support for the parallel trends assumption. Considering the results of the placebo-period test in Column (1), the estimate of interest is positive and statistically significant. This finding is not necessarily unexpected given that our treatment variable is defined by unemployment insurance contribution. However, it is notable that the magnitude of the coefficient is much smaller (approximately three times) relative to our main results in Table 3 which range between 16 and 18 percentage points, and the estimates are statistically significantly different from one another at the 1 per cent level. Arguably, this stark difference in the significance and magnitudes of the coefficients between both years speaks to the effect of the TERS policy but implies that our estimated effect may be slightly overestimated.

Finally, in Column (8) of Table 5 we present our model results while controlling for eligibility of the expanded ETI policy. In terms of magnitude, statistical significance, and precision, the estimated effect of 15.9 percentage points here is notably similar to that observed in our main results. Moreover, the coefficient on the ETI indicator is statistically insignificant and close to zero. This might suggest that the expanded ETI had no effect on job retention, which may be unsurprising given the magnitude of the subsidy was much smaller than the TERS. However, more research is required to make such conclusions confidently, but this is beyond the scope of this paper. Overall, this finding suggests that the expanded ETI policy does not have a confounding effect on our estimated TERS effect.

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[19] For the same reason that we cannot control for individual FEs in the placebo outcome models (see footnote 18), we cannot do so when we control for expanded ETI eligibility given that this variable is time-invariant.

Table 5: Robustness test results: placebo-period, placebo-outcome, and controlling for expanded ETI eligibility

Test:	Placebo-period	Placebo-outcome						Expanded ETI eligibility
Outcome	Pr(Employed)	Pr(Black)	Pr(Female)	Age	Pr(Urban)	Pr(Married)	Pr(Secondary education)	Pr(Employed)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TERS×Post	0.057***	−0.023	0.017	0.184	−0.020	−0.009	0.011	0.159***
	(0.010)	(0.018)	(0.014)	(0.280)	(0.015)	(0.017)	(0.019)	(0.022)
Expanded ETI eligibility								0.004
								(0.009)
Controls								
Demographic	Y	Y	Y	Y	Y	Y	Y	Y
Labour market	Y	Y	Y	Y	Y	Y	Y	Y
PS	Y	Y	Y	Y	Y	Y	Y	Y
Individual FE	Y	N	N	N	N	N	N	N
Constant	1.243***	−0.493***	0.160	42.344***	0.280***	−0.869***	0.432***	0.850***
	(0.401)	(0.100)	(0.133)	(1.613)	(0.100)	(0.118)	(0.123)	(0.075)
Observations	10,462	8,284	8,284	8,284	8,284	8,284	8,284	8,259
R^2	0.525	0.525	0.465	0.218	0.239	0.394	0.237	0.167

Note: TERS, Temporary Employer–Employee Relief Scheme; PS, propensity score (the conditional probability of being a UIF contributor); FEs, fixed effects; UIF, Unemployment Insurance Fund; Y, yes; N, no; ETI, Employment Tax Incentive. This table presents estimates of specification (2) as follows: (1) a pre-treatment placebo period (2019); (2)–(7): several placebo outcomes: age (years), and binary indicators for being African/Black, female, living in an urban area, married, and having a complete secondary education; and (8): controlling for eligibility for the expanded ETI using the multiply imputed wage data. Sample restricted to the employed working-age population (15–64 years) in the baseline period. All estimates are weighted using sampling weights. Standard errors are presented in parentheses and are clustered at the panel (individual) level. R^2 value for model (8) equivalent to the mean R^2 value across imputation datasets. ***$p<0.01$, **$p<0.05$, *$p<0.10$.

Source: authors' calculations based on QLFS 2019Q1, 2019Q2, 2020Q1, and 2020Q2 (StatsSA 2019a, 2019b, 2020b, 2020c).

6   Conclusion

Wage subsidies have served as one of the primary measures used by governments around the world to mitigate job losses in response to the COVID-19 pandemic. In South Africa, where unemployment is among the highest in the world, such a policy (the TERS) has arguably served as the government's most important labour market intervention during the pandemic to date. Despite the existing literature on the policy, no causal evidence exists testing whether the policy was successful in achieving its primary objective of mitigating job losses. In this paper, we use unique panel labour force data, as well as wage data provided to us by South Africa's National Statistics Office, to estimate the causal effect of the TERS on job retention by exploiting a temporary eligibility detail at the beginning of the policy. Furthermore, given the progressive design of the policy's benefit formula, we hypothesize and test for evidence of heterogeneous and progressive effects across the wage distribution.

We find evidence of a statistically significant and positive effect of the TERS policy on job retention for private formal sector workers, who represent the majority of employment in South Africa. Our preferred estimate suggests that the policy increased the probability of remaining employed by just under 16 percentage points, significant at the 1 per cent level. This finding holds when subjected to several placebo tests and controlling for eligibility for another labour market policy introduced in the same period. We find larger job retention effects of up to 26.6 percentage points for lower-wage workers and find no evidence of any effects among the top 40 per cent of workers. This supports our hypothesis that the policy impact was progressive. This finding holds when we use alternative indicators of progressivity, with larger effects generally observed for workers in less-skilled occupations, with lower levels of formal education, and who work in the primary and secondary sectors. The progressivity of these results is noteworthy considering the regressivity of pandemic-induced job loss in the country, where job loss disproportionately affected lower-wage workers (Köhler and Bhorat 2020; Ranchhod and Daniels 2021).

Overall, our analysis provides, to the best of our knowledge, the first causal evidence that not only was South Africa's TERS policy successful in its primary objective of mitigating job losses but also its progressive design combated the regressive employment effects of the pandemic, thus protecting workers who needed it most. This suggests that although the extent of job loss in the South African labour market was significant, with net employment contracting by 14 per cent in the first quarter of the pandemic, it would have been markedly higher in the absence of the TERS policy. It is important to note, however, that our analysis only tests for effects in the short-term (within the first 2 months). Overall, our findings provide evidence on the effective role that wage subsidies can play in a developing country as a strategy to mitigate job loss during a labour market crisis.

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Appendix

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

	(1)	(2)	(3)
	TERS non-recipients	TERS recipients	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)
Education			
Primary or less	0.086	0.075	−0.011
	(0.280)	(0.263)	(0.022)
Incomplete secondary	0.319	0.291	−0.028
	(0.466)	(0.454)	(0.045)
Complete secondary	0.229	0.288	0.059
	(0.420)	(0.453)	(0.046)
Tertiary	0.366	0.347	−0.019
	(0.482)	(0.476)	(0.049)
Skill level			
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: TERS, Temporary Employer–Employee Relief Scheme. This table presents estimates of mean values for observable covariates by TERS receipt status in April 2020 accompanied by intergroup differences in means. Sample restricted to the working-age population (15–64 years). All estimates are weighted using sampling weights. Standard errors are 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 NIDS-CRAM (2020).

Table A2: Estimates of heterogeneous effects of TERS receipt on short-term job retention, by pre-pandemic wage quintile

Subsample	Quintile 1		Quintile 2		Quintile 3		Quintile 4		Quintile 5	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
TERS×Post	0.149***	0.141***	0.161**	0.266***	0.181***	0.125*	0.109*	0.109	0.050	0.048
	(0.044)	(0.049)	(0.061)	(0.074)	(0.060)	(0.071)	(0.066)	(0.072)	(0.068)	(0.075)
Controls										
Demographic	N	Y	N	Y	N	Y	N	Y	N	Y
Labour market	N	Y	N	Y	N	Y	N	Y	N	Y
PS	N	Y	N	Y	N	Y	N	Y	N	Y
Individual FE	N	Y	N	Y	N	Y	N	Y	N	Y
Constant	1.000	1.002***	1.000	1.006***	1.000	1.004***	1.000	1.000***	1.000	1.000***
	(0.000)	(0.008)	(0.000)	(0.013)	(0.000)	(0.011)	(0.000)	(0.007)	(0.000)	(0.005)
Observations	1,583	1,503	1,498	1,470	1,539	1,509	1,590	1,578	1,651	1,613
R^2	0.130	0.219	0.151	0.298	0.112	0.210	0.083	0.153	0.047	0.087

Note: TERS, Temporary Employer–Employee Relief Scheme; N, no; Y, yes; PS, propensity score; FEs, fixed effects; DiD, difference-in-differences. This table presents estimates of the main DiD model for split samples by pre-pandemic (2020Q1) monthly nominal wage using the multiply imputed wage data, corresponding to Figure 4. Sample restricted to the employed working-age population (15–64 years) as of 2020Q1. All estimates are weighted using sampling weights. Standard errors are clustered at the panel (individual) level. R^2 values for a given model equivalent to the mean R^2 value across imputation datasets. ***$p<0.01$, **$p<0.05$, *$p<0.10$.

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