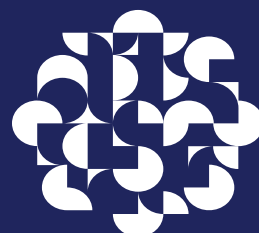# COVID-19 and the labour market: Estimating the employment effects of South Africa's national lockdown

By Timothy Köhler, Haroon Bhorat, Robert Hill and Benjamin Stanwix

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COVID-19 and the labour market:

Estimating the employment effects of South Africa's national lockdown

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Abstract

Like most around the world, the South African government quickly implemented a relatively stringent national lockdown in response to the COVID-19 pandemic. Although much research documenting the pandemic's economic effects exists, these studies are largely descriptive in nature and are therefore unable to distinguish changes in employment attributable to lockdown policy versus other pandemic-related factors such as foreign policy and consumption-related behavioural responses. In this paper, we seek to specifically isolate and provide causal evidence on the effect of South Africa's lockdown policy in particular. To do so, we adopt a quasi-experimental econometric technique to exploit variation in legislated industry-level permission to work and the coincidental timing of the lockdown and data collection dates of nationally representative labour force data. We find that the national lockdown decreased the probability of employment for those not permitted to work by 8 percentage points relative to the control group. This significant, negative effect holds across several robustness tests. Using this estimate we can approximate that, of the 2.2 million fewer people employed, South Africa's lockdown policy directly accounted for just under 600 000 (or 26% of total jobs lost), suggesting the majority of job loss can be attributed to other pandemic-related factors. We further show that the lockdown particularly jeopardized the livelihoods of those in the informal sector, with an estimated effect nearly 3 times larger than the overall effect. The vulnerability of this group to the economic consequences of the pandemic is of concern, given that their informality presents a challenge for government to provide targeted relief. To prevent further widening labour market inequalities, our analysis emphasises the importance of effective policy to support the livelihoods of those in the informal economy.

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1. Introduction

South Africa has been one of the countries affected most adversely by the COVID-19 pandemic in Africa. By the end of April 2021, South Africa accounted for the highest number of confirmed cases per capita on the continent with over 1.5 million cumulative cases, representing over a third (35%) of total confirmed African cases. In response, like most governments around the world, South Africa implemented a national lockdown to prepare the necessary health infrastructure as well as to delay and minimise the spread of the virus. This initial lockdown, which began on 26 March 2020 and lasted for five weeks, was relatively stringent by international standards (Bhorat et al., 2020; Gustafsson, 2020), making no allowance for any non-essential activities outside the home. Following this, a phased easing of restrictions was introduced in five levels, with the initial lockdown period classified as level 5. Regulation under levels 4 (1 to 31 May) and 3 (1 June to 17 August) gradually permitted specific categories of 'non-essential' work to resume. Estimates using pre-crisis data suggest that just 40% of the employed were permitted to work under level 5, rising to 71% under level 3 (Francis *et al*., 2020).[1]

Although the pandemic continues to pose important risks to public health, South Africa's lockdown was always expected to lead to substantial short- and long-term economic costs. Official labour force data shows that there were approximately 2.2 million fewer people employed in the second quarter of 2020 relative to the first[2] – essentially erasing the last 10 years of job growth in the economy. Only a partial recovery can be observed in data from the third and fourth quarters of the year, with net employment still down 1.4 million relative to pre-pandemic levels. Research conducted during the lockdown suggests that job losses have been concentrated among a range of already vulnerable groups, particularly individuals in the poorest households (Köhler and Bhorat, 2020), less-skilled and low-wage workers (Jain *et al*., 2020; Ranchhod and Daniels, 2020), informal workers (Benhura and Magejo, 2020), those with transient employment or persistent non-employment histories (Espi *et al*., 2020), those living in poor urban communities (Visagie and Turok, 2020), and women – particularly the poorest (Hill and Köhler, 2020; Casale and Posel, 2020; Casale and Shepherd, 2020). Many of these findings are consistent with those observed in labour markets across the world (International Labour Organization (ILO), 2020).

Despite the large amount of important work that has already been done to measure the various socio-economic impacts of South Africa's lockdown, many of these studies are largely descriptive in nature. However, changes in net employment from before to during the pandemic need not be solely attributable to lockdown policy, but additionally to other pandemic-related factors such as mortality and morbidity, foreign policy, and consumption-related behavioural responses. In this paper, we seek to address these confounders and isolate the effect of South Africa's lockdown policy on employment in particular. To do so, we use representative labour force data – the Quarterly Labour Force Survey (QLFS) – and employ a quasi-experimental econometric approach – a propensity score-matched (PSM) difference-in-differences (DiD) approach – to estimate the immediate causal effect of the country's lockdown on employment by exploiting industry-level variation in legislated permission to work and the coincidental timing of the national lockdown and the data collection dates of the QLFS. Simply put, we examine the effect of the lockdown on the probability of employment amongst workers who were not permitted to work, relative to those who were.

Several findings from our analysis stand out. In our descriptive analysis, we show that employment loss was concentrated amongst the youth, those with lower levels of formal education, those living in urban areas, the private sector, non-union members, the secondary sector (particularly manufacturing and construction), and low- and semi-skilled workers. Notably, the lockdown disproportionately affected informal-sector workers, who accounted for one in every two net jobs lost, despite

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[1] The assumptions to arrive at these estimates are discussed in detail in Francis *et al*. (2020).
[2] A similar change in employment is observed if one alternatively uses year-on-year changes.

representing just 25% of pre-pandemic employment. This latter finding is consistent with our quasi-experimental findings. We find that the national lockdown decreased the probability of employment for those not permitted to work, by eight percentage points relative to the control group – a finding that holds across several robustness tests. We find larger effects for more stringent lockdown levels and distinct sub-groups – specifically own-account workers (most of whom are in the informal sector) – who experienced a nearly three times larger negative employment effect than the overall average treatment effect. This latter finding is indicative that working in the informal sector seems to be a key determinant of not being employed during the lockdown period. Finally, we use our main estimated effect to approximate the proportion of aggregate job loss that is directly attributable to the lockdown policy as opposed other pandemic-related factors discussed above. We calculate that of the 2.2 million quarter-on-quarter contraction in employment, South Africa's specific lockdown policy directly accounted for about 583 500, or 26.1% of total jobs lost. However, depending on assumptions, this pure lockdown effect may be as low as 285 000 (12.7% of total jobs lost). Overall, this suggests that the majority of South Africa's short-term job loss can be attributed to other pandemic-related factors, such as foreign policy or changes in consumer behaviour.

The rest of the paper is structured as follows. In Section 2, we describe our data, identification strategy, and present several descriptive statistics on labour market outcomes prior to and during the first three months of South Africa's national lockdown. In Section 3, we present and discuss our main findings from our quasi-experimental models. In Section 4, we reflect on our results and conclude.

2.   Data and identification strategy

2.1.   The Quarterly Labour Force Survey

The analysis in this paper uses individual-level survey data from Statistics South Africa's (StatsSA) Quarterly Labour Force Survey (QLFS). The QLFS is a cross-sectional, nationally representative household survey, conducted every quarter since 2008, that contains detailed information on a wide array of demographic and socioeconomic characteristics and labour market activities for individuals aged 15 years and older. There are a number of important differences in the 2020 QLFS data that are worth noting in some detail here. Prior to the COVID-19 pandemic in South Africa, the QLFS sample consisted of nearly 70 000 individuals living in approximately 30 000 dwelling units, with data being collected via face-to-face interviews. However, towards the end of March 2020, StatsSA suspended face-to-face data collection as a result of COVID-19. Because of this, 621 sampled dwelling units (or 2% of the sample) were not interviewed in the quarter 1 dataset. To adjust for this, StatsSA used the panel component of the survey and made imputations where possible, using data from the previous quarter.

To continue providing labour market statistics for the remainder of the year during the national lockdown, StatsSA changed its data collection mode from face-to-face interviews 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 contact numbers was surveyed again in 2020Q2. The result was that the 2020Q2 data included about 71% of the 2020Q1 sample because not all dwelling units had contact numbers.[3] The obvious concern here is that this will produce 2020Q2 estimates that suffer from selection bias; that is, it is likely that the underlying characteristics of 'telephone' and 'non-telephone' households are different. For example, we know from the 2020Q1 data that individuals in 'non-telephone households' were significantly more likely to be unemployed relative to those in 'telephone households'. To address this source of bias, StatsSA took a number of

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[3] Additionally, amongst those who did have contact numbers, some contact numbers were found to be invalid or were not answered during data collection, and some households indicated that they were no longer residing at the dwelling units they had occupied during 2020Q1. All of these were regarded as non-contact and were adjusted for during the weighting processes.

steps to adjust the calibrated survey weights, using the 2020Q1 data and several bias-adjustment factors (StatsSA, 2020d), which we do not discuss in detail here.

Table 1 below presents an overview of the sample sizes and weighted estimates of the South African labour market for 2020Q1 and 2020Q2. We use the relevant bias-adjusted sampling weights provided by StatsSA unless otherwise indicated and restrict the sample to the working-age population (those aged 15 to 64 years). Looking at the aggregated data, the bias-adjusted 2020Q2 weights appear to be appropriately computed. From an unweighted sample of 66 657 individuals, the weighted estimate of the South African population in 2020Q1 is 57.8 million. The relevant 2020Q2 estimate is just under 58 million, despite the 2020Q2 sample consisting of nearly 20 000 fewer individuals. In contrast, the weighted estimates of specific labour market groups (such as the labour force and number of employed) are statistically significantly different in size between quarters, which is expected given the pandemic and associated government responses. Despite this, it should be noted that the sampling bias adjustments by StatsSA relied on observable characteristics, such as age, gender, and race; however, respondents may still be unobservably different from non-respondents, and hence possibly from the broader population. At the time of writing, an explicit external review of the construction of these weights has yet to be conducted and would require more information than is available in the public QLFS documentation.

Table 1: Sample sizes and weighted population estimates, by quarter

| | 2020Q1 | | 2020Q2 | |
	Unweighted	Weighted	Unweighted	Weighted
Total	66 657	57 792 395	47 103	57 973 917
Working-age population	41 827	38 873 945	29 495	39 021 017
Labour force	24 549	23 452 204	13 023	18 443 066 *
Employed	17 044	16 382 555	10 001	14 148 215 *
Unemployed	7 505	7 069 649	3 022	4 294 851 *
Discouraged	3 149	2 918 028	1 865	2 470 782 *
Not economically active	14 129	12 503 712	14 607	18 107 168 *

Source: QLFS 2020Q1 and 2020Q2 (StatsSA, 2020a and 2020b). Authors' own calculations.
Notes: [1] Relevant estimates weighted using sampling weights. [2] Labour market groups restricted to the working age (15 to 64 years). [3] Official (narrow) definitions of unemployment used. [4] * denotes statistical significance of a different 2020Q2 estimate relative to the relevant 2020Q1 estimate at the 95% confidence level.

2.2. Identification strategy: Propensity Score-Matched Difference-in-Differences

Our aim in this paper is to estimate the causal effect of South Africa's national lockdown on employment probability, for which we require a suitable identification strategy. Using vocabulary from the randomised evaluation literature, the ideal way to estimate a causal effect entails randomised assignment of treatment (in this case, a national lockdown). Such randomisation would, subject to several conditions, allow us to directly measure the effect of the policy in isolation. In the context of South Africa's national lockdown, however, treatment was not assigned randomly. Every worker in the country was legally obligated to adhere to the lockdown regulations as they were specified and adjusted over time. However, being permitted and able to continue working was dependent on job type, which does provide a neat division of 'treated' and 'untreated' individuals over time. As such, we estimate the causal effect of the lockdown by exploiting between-industry variation in legislated permission to work, as per the relevant Government Gazettes. We cross-examine these lockdown regulations with over 150 three-digit industry codes in the QLFS data to identify individuals who were and were not permitted to work. To address selection bias and ensure that employment probabilities are driven only by differences in treatment, we then employ a propensity score matching (PSM) reweighting technique that seeks to provide a comparable set of individuals across our treatment and control groups. We then use the timing of the national lockdown, and the timing of the QLFS data

collection interviews, to estimate difference-in-differences (DiD) models on a matched panel sample. This approach is outlined in more detail below.

2.2.1. Difference-in-differences

Our DiD approach exploits across-group (treatment and control) and across-time (before and during the national lockdown) variation. We use the 2020Q1 QLFS (January to March 2020) as our pre-treatment period and the 2020Q2 QLFS (April to June 2020) as our post-treatment period.[4] This is motivated by the observation that the lockdown was implemented from the end of March 2020, coinciding with the change in QLFS quarters. We thus can compare employment outcomes effectively for those not permitted to work versus those permitted to work over the period. Specifically, our treatment group consists of all the individuals in our sample who, as per legislation, were *not* permitted to work during the national lockdown. Our control group thus consists of those who were legally permitted to work. We additionally include in the control group anyone who was able to work due to specific characteristics of their occupation and sector. This sub-category of workers includes those working in the public sector and those, amongst the employed, who report working from home.[5,6] In our analysis to follow, we estimate several specifications using alternative control group definitions to examine the sensitivity of our results.

Importantly, South Africa's lockdown rules were not time-invariant. As noted above, from April 2020 the country adopted a five-stage risk-adjusted lockdown strategy which outlined who was permitted to work at each lockdown level. To account for this, we make use of QLFS 2020Q2 'interview date' data provided by StatsSA, which indicates whether an individual was surveyed in April, May or June 2020. These periods fortunately coincide with changes in the national lockdown levels, with Level 5 in place from 1 to 30 April, Level 4 from 1 to 31 May, and Level 3 from 1 to 30 June in the 2020Q2 data.[7] For example, individuals were included in the treatment group if they were not permitted to work under Level 5 regulations and they were interviewed in April during Level 5, and similarly for Levels 4 and 3. Regardless of permission to work as per legislation or lockdown level, all individuals working in the public sector or working from home were assigned to our main control group. In some instances, firms in a given industry were permitted to operate, but only at partial capacity. However, we cannot identify which workers were permitted to work in these 'limited capacity industry' situations. To address this, we assign relevant individuals to the control group (i.e. 'permitted to work') if they were permitted to work in a 'limited capacity industry', in which the legislated capacity was equal to or exceeded 50%. In our analysis, we use alternative thresholds to examine the sensitivity of our results to this assumption.

Based on the PSM reweighting approach discussed below, our DiD model is estimated according to the following specification using Ordinary Least Squares (OLS):

$$y_{it} = \beta_0 + \beta_1 Treatment_i + \beta_2 Post_t + \beta_3 Treatment_i \times Post_t + \beta_4 \boldsymbol{X}_{it} + \gamma_i + \varepsilon_{it},$$

where y_{it} is the outcome of interest for individual i in time period t (in this case, a binary employment variable using the conventional definition[8]), $Treatment_i$ is a binary treatment variable, $Post_t$ is a

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[4] It should be noted that our identification strategy cannot account for seasonality, which may be important to note considering that the South African economy went into recession prior to the pandemic in 2020Q1.

[5] The relevant work-from-home variable was included as an additional variable in the 2020Q2 QLFS as part of a special COVID-19 module and was only asked of the employed. We exploit the panel nature of the 2020Q1 and 2020Q2 QLFS datasets to impute responses in 2020Q1 based on individuals' 2020Q2 responses to this question.

[6] We include the unemployed who have worked before in the sample and use the relevant three-digit previous industry variable to assign them to treatment and control groups.

[7] We cannot account for any changes in legislature within lockdown levels, given that the frequency of the interview date data is monthly.

[8] Here we make use of StatsSA's conventional definition of employment as per the standard ILO definition. This includes any working-aged individual who worked even for an hour in the week prior to the survey, as well as those who did not work because they were temporarily

binary variable equal to one for the post-treatment period (2020Q2) and zero otherwise (2020Q1), and ε_{it} is the regression error term. Furthermore, even though PSM accounts for pre-existing observational differences between individuals in the treatment and control groups, and the matched DiD approach controls for pre-existing unobservable differences (under the parallel trend assumption) and time-variant observational differences, we further control for a vector of pre-existing individual-level characteristics, $\boldsymbol{X}_{it}$ (including a categorical national lockdown level variable), to improve (i) the plausibility of the DiD identifying assumption and (ii) the efficiency of our estimates. Finally, we exploit the panel nature of the data to control for individual fixed effects (FE), represented by γ_i. β_3 is the main coefficient of interest, as it measures the causal effect of the onset of lockdown policy; that is, the average difference in outcomes between the treatment and control groups in the post-treatment period relative to the pre-treatment period.

2.2.2. Propensity Score Matching

Individuals in the treatment and control group may differ by characteristics other than treatment itself, which will likely bias the employment effects we are trying to identify. To address such selection bias, we use a common approximate matching technique – PSM – which seeks to identify similar individuals across treatment and control groups. Put differently, PSM attempts to ensure balance in a set of common observable characteristics across treatment and control groups in the pre-treatment period. The idea is to compare individuals who, conditional on a set of observables, have very similar probabilities of being in the treatment group (propensity scores), even though those individuals differ with regard to actual treatment status. If two individuals have the same propensity scores conditional on a vector of observable covariates but one is in the treatment group and the other is not, then the two individuals are regarded as observationally exchangeable and differences in their observed outcomes of interest are attributable to differences in treatment.[9]

We first estimate propensity scores, and thereafter use these probabilities to construct and use inverse probability weights (IPW) in our DiD regressions on the matched sample. To estimate these scores, we use a logit model to estimate the probability of being in the treatment group based on a vector of observable covariates.[10] These include age, age squared, sex, race, marital status, highest level of education, province, household geographic area, lockdown level, and type of employment. Our inclusion of specific covariates in the propensity score model is guided by the aim of credibly satisfying the conditional independence assumption (CIA): conditional on the propensity score, the outcome of interest is independent of treatment. This entails including variables that are thought to be related to both the treatment and the outcome of interest but are unaffected by the treatment itself. We adopt a parsimonious model and avoid including too many variables, given that doing so may exacerbate the common support problem.[11] Using nearest-neighbour matching using a relatively small caliper of 0.02, we match exclusively on pre-treatment data, given that post-treatment characteristics may be endogenous, as follows:

$$Pr(z_i) \stackrel{\text{def}}{=} Pr(Treatment_i = 1 \mid \boldsymbol{X}_i),$$

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absent but definitely had a job to return to. Because the conventional inclusion of this latter group may not be appropriate in the context of a lockdown, we make use of several alternative employment definitions as a robustness test in Section 3.3.

[9] Assuming the conditional independence assumption (CIA) holds; that is, treatment (legislature not permitting work) conditional on the propensity score is independent of potential outcomes or is "as good as random".

[10] The choice of using a logit as opposed to a probit model for the binary treatment case is not critical, because these models usually yield similar estimates; however, the former is used because the logistic distribution has higher density mass in the bounds (Caliendo and Kopeinig, 2008).

[11] When there is an insufficient overlap in observables of individuals in the treatment and control groups to find appropriate matches (Bryson et al., 2002).

where $Treatment_i$ is a binary variable equal to one if an individual is included in the treatment group and zero if included in the control group, and $\boldsymbol{X}_i$ is the vector of observable covariates discussed above. We then generate IPWs using these scores to reweight observations as follows:

$$w_i = \delta_i \left\{ \frac{Treatment_i}{z_i} - \frac{1-Treatment_i}{1-z_i} \right\},$$

where w_i is the final inverse probability weight of individual i, which is equal to the QLFS sampling weight (δ_i) multiplied by a function of the dichotomous treatment variable and the estimated propensity score z_i.[12] This function is equivalent to $\frac{1}{z_i}$ for treated observations and $\frac{1}{1-z_i}$ for control observations, and is based on inverse-probability regression (Brunell and DiNardo, 2004). That is, it weighs up treated observations with lower propensity scores and control observations with higher propensity scores. Table 2 presents diagnostic statistics to examine covariate balance between treatment groups in the raw and matched samples. Our PSM approach appears to have worked relatively well. For every covariate in the matched sample, the standardised differences are all close to zero, and the variance ratios are all close to one.[13] This is reflected by the propensity score histogram in Figure 1 which highlights sufficient overlap in the distribution of propensity scores across treatment groups and implies that matching on the estimated propensity score balanced the covariates.

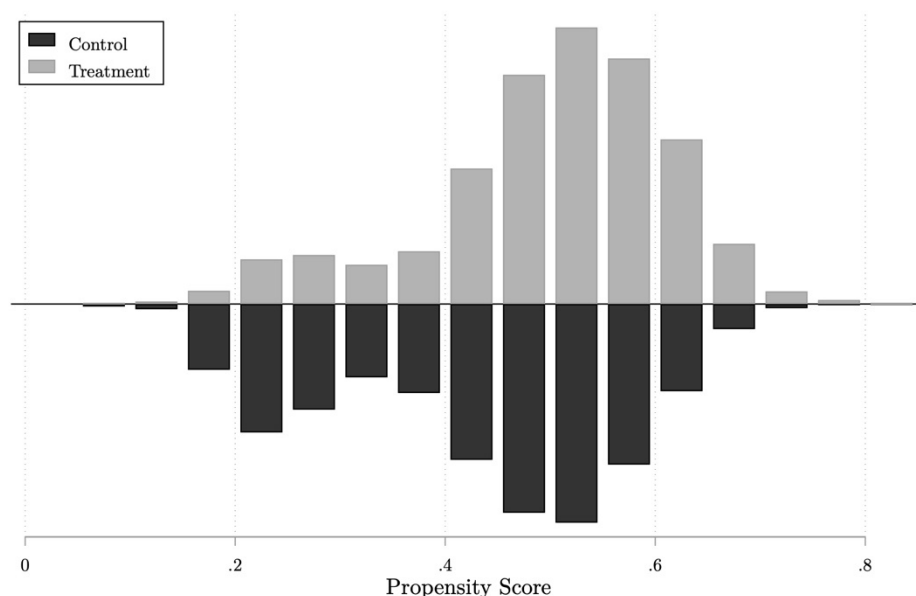**Table 2: Propensity score matching balance summary diagnostics of pre-treatment covariates**

Variable	Standardised differences		Variance ratio	
	Raw	Matched	Raw	Matched
Sex				
Male	0.211	0.026	0.982	0.993
Female	-0.252	-0.020	0.923	0.991
Race				
Black/African	0.090	-0.021	0.880	1.035
Coloured	-0.040	0.007	0.904	1.019
Indian/Asian	0.006	0.011	1.037	1.072
White	-0.093	0.019	0.747	1.070
Education				
Primary education or less	0.086	0.053	1.214	1.122
Secondary incomplete	0.216	-0.013	1.114	0.997
Secondary complete	0.042	-0.012	1.033	0.992
Tertiary	-0.411	-0.020	0.469	0.948
Age and geographic area				
Age	-0.115	0.058	1.003	1.042
Urban	0.064	-0.007	0.935	1.008
Traditional areas	0.005	-0.002	1.007	0.997
Farms	-0.152	0.025	0.488	1.161

Source: QLFS 2020Q1 (StatsSA, 2020a). Authors' own calculations.
Notes: [1] Labour market groups restricted to the working-age population (15 to 64 years). [2] Propensity scores estimated for the panel through logit regression on a pre-treatment vector of covariates using a caliper of 0.02 and independent and identically distributed (i.i.d.) standard errors.

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[12] This latter function is normalised using *min-max normalisation* prior to being included in this formula.
[13] Despite these results suggesting success in achieving balance of observables, inference here is regarded as informal because we do not have standard errors for these statistics.

Figure 1: Kernel density plots and histogram of propensity scores, by treatment group and sample

2.3.   Descriptive statistics

Prior to presenting and discussing our quasi-experimental estimates, we present several descriptive statistics of aggregate and between-group changes in net employment since the beginning of the QLFS in 2008, highlighting the recent effects of the pandemic. Expectedly, the pandemic led to a substantial reduction in the number of employed in the country. Relative to 2020Q1, there were more than 2.2 million less employed people in 2020Q2 – a 14% decrease, which is equivalent to employment levels between 2008 and 2012. This drop in employment was coupled with a decrease in the number of official (searching) unemployed individuals (by nearly 40%, or 2.7 million), and an even larger absolute increase in the number of economically inactive individuals (by 33%, or more than 5 million).[14] These shifts can to a large extent be explained by the nature of the national lockdown policy, which restricted the ability of people to work and to search for work. Together, these large shifts explain the, if observed alone, misleading decrease in the official unemployment rate from 30% to 23% – the lowest recorded since the start of the QLFS – reflecting a simple definitional consequence. These unusual changes in employment, unemployment and inactivity have been observed in labour markets across the world (ILO, 2020), but must be accepted as nothing more than a statistical anomaly brought about by the inability of the unemployed to search for jobs.

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[14] This latter group are not classified amongst the discouraged unemployed because, when asked why they were not looking for work, individuals in this group responded with reasons 'Other' than discouragement. This reason can be attributed to the national lockdown policy, which restricted any activity deemed 'non-essential' outside the home.

Figure 2: **Trends in key labour market indicators, 2008Q1-2020Q2**
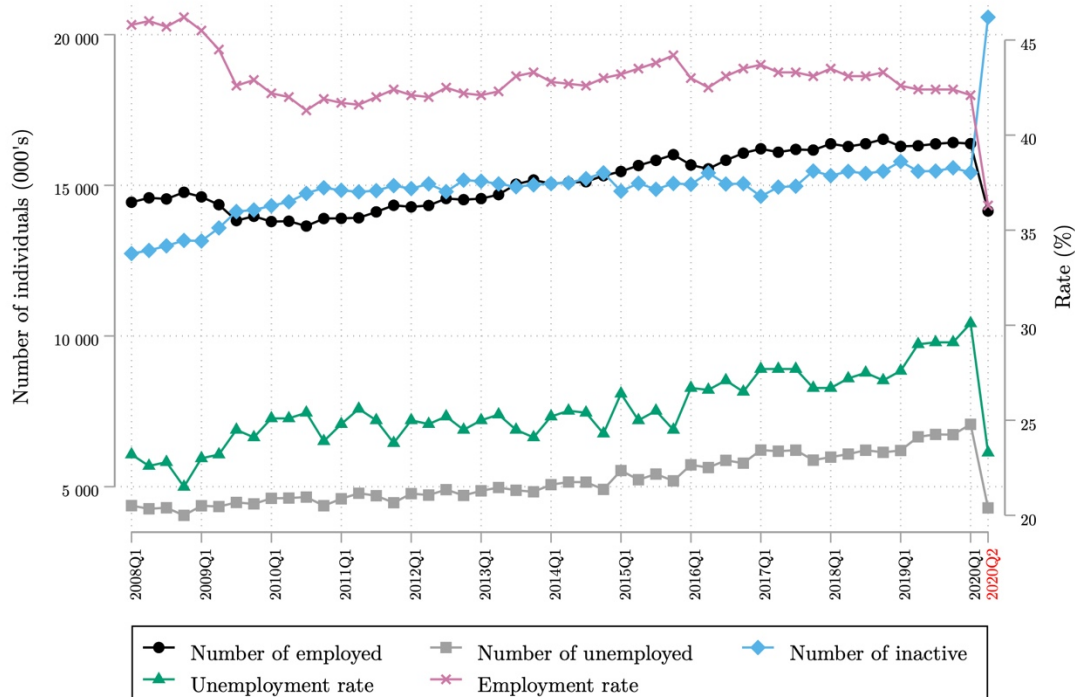
Source: QLFS 2008Q1 to 2020Q2 (StatsSA). Authors' own calculations.
Notes: [1] All estimates weighted using relevant sampling weights. [2] Official (narrow) definition of unemployment used throughout.

The observed changes in aggregate labour market outcomes above are important to consider, however they hide substantial underlying between and within-group variation. Along multiple dimensions in South African labour market, the distribution of job loss has been uneven. Geographically, Figure 3 highlights the significant variation in net employment changes across the country. By province, Gauteng (GP), KwaZulu-Natal (KZN), and the Western Cape (WC) – the largest provinces in terms of their national employment shares – experienced the largest absolute reductions in employment with approximately 1.4 million fewer people employed. Gauteng alone accounts for nearly 30% of total jobs lost. In relative terms, however, the Northern Cape, Free State, and Limpopo were hardest hit, with 23%, 17%, and 17% fewer people employed, respectively.

Figure 3: District-level variation in net employment loss, 2020Q1-2020Q2
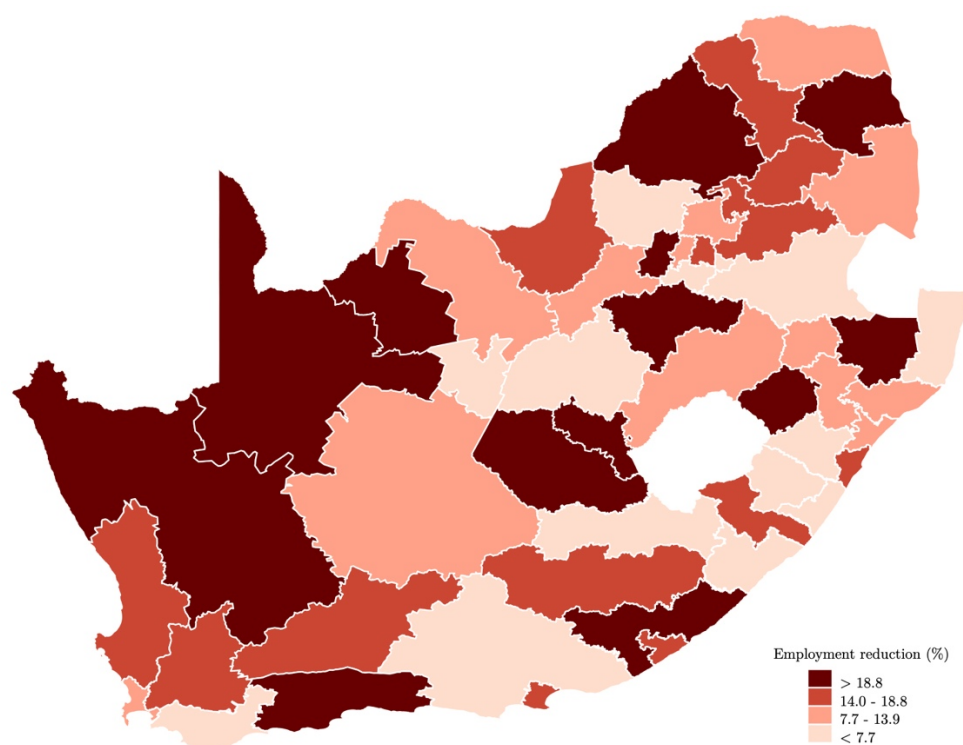
Source: QLFS 2020Q1 and 2020Q2 (StatsSA, 2020a and 2020b). Authors' own calculations.
Notes: [1] Sample restricted to working-age population (15 to 64 years). [2] All estimates weighted using relevant sampling weights.

Just as the distribution of job loss was unequal, so too is that of recovery. Figure 4 presents a scatterplot of job loss and recovery rates for each of South Africa's 52 municipal districts, weighted by share of national employment and colour-coded by province. The majority of districts are located in the upper-left quadrant, with most exhibiting a severe immediate employment contraction and a subsequent modest recovery. Of the largest districts, eThekwini in KZN – who experienced the most severe contraction of 17%, closely followed by Ekurhuleni and the City of Johannesburg in GP (16% and 14%) – had recovered almost 60% of net employment lost from 2020Q1-2 by the end of the year. On the other hand, the cities of Cape Town and Johannesburg had recovered just under a third (32%) and 16% of lost employment, respectively.

Figure 4: District-level weighted scatterplot of net employment loss and recovery, by province

Authors' own calculations. Source: QLFS 2020Q1, 2020Q2, and 2020Q4 (StatsSA, 2020a, 2020b, and 2020c).
Notes: [1] Sample restricted to working-age population (15 to 64 years). [2] All estimates weighted using relevant sampling weights. [3] Markers weighted by pre-pandemic share of national employment in 2020Q1. [4] Job loss = percentage reduction in net employment from 2020Q1 to 2020Q2; job recovery = net employment gain from 2020Q2 to 2020Q4 as a percentage of net employment loss from 2020Q1 to 2020Q2. [5] EC = Eastern Cape, FS = Free State, GP = Gauteng, KZN = KwaZulu-Natal, LP = Limpopo, MP = Mpumalanga, NC = Northern Cape, NW = North West, WC = Western Cape, ETH = eThekwini, EKU = Ekurhuleni, JHB = City of Johannesburg, CPT = City of Cape Town, TSH = Tshwane.

Demographically, perhaps most significantly, youth accounted for about half (50.6%, or 1.1 million) of employment loss, despite representing only over a third (36.6%) of pre-pandemic employment. African/Black individuals accounted for nearly 78% of the aggregate reduction in net employment, or 1.7 million people – a slightly disproportionate burden given the group's share of pre-pandemic national employment of 75%. On the other hand, just 150 000 (7%) fewer White individuals were employed despite accounting for 11.3% of the pre-pandemic employed. Considering gender, although men accounted for a slightly higher share of employment loss (55.5%), women were slightly disproportionately affected given their smaller share of pre-pandemic employment (43.7%). Employment loss was disproportionately concentrated amongst individuals with relatively lower levels of formal education, those living in urban areas, those working in the informal sector, the private sector, and the non-unionised. Individuals whose highest level of education is less than Grade 12 (matric) or equivalent accounted for more than 70% of employment loss, despite representing only 45% of pre-pandemic employment. Notably, although employment loss in the informal sector and private households together represent about half of total employment loss, these sectors accounted for just under 28% of pre-pandemic employment, highlighting a disproportionate burden. Remarkably, almost all (93.8%) jobs lost were in the private sector, despite the public sector accounting for 17.4) of pre-pandemic employment. Similarly, nearly all those who lost jobs (95.2%) were non-union members.
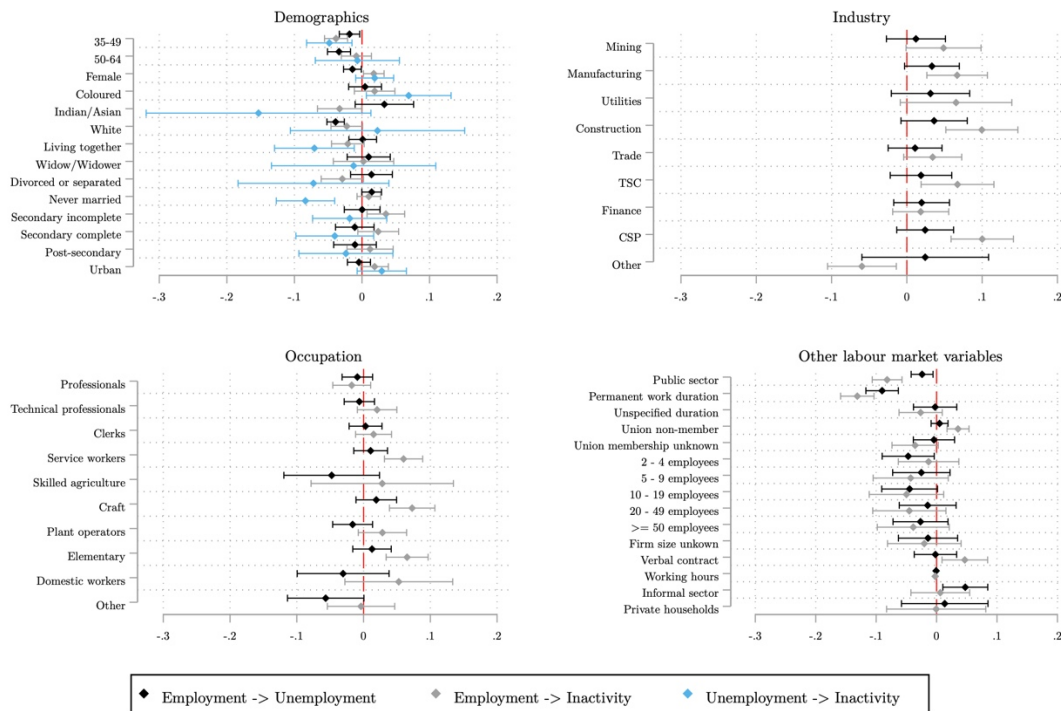
The tertiary sector accounted for most of the total employment decrease (67.1%), but this is not unexpected given that the sector accounted for most jobs (72.2%) prior to the pandemic. The remaining job loss (30.6%) was accounted for by the secondary sector, exceeding its share of pre-pandemic employment. These latter job losses were mostly in manufacturing (334 000 jobs lost) and

construction (297 000 jobs lost). The primary sectors appear to have been relatively well insulated from the negative employment effects. By occupation, lower- and semi-skilled workers account for almost all jobs lost, with employment levels amongst high-skilled workers remaining statistically unchanged. Amongst low-skilled occupations, one in every four (or 250 000) domestic workers lost their jobs, accounting for 11.2% of total employment loss despite representing just 6% of the pre-pandemic employed. More than half a million (530 000) other less-skilled workers lost their jobs, including farm labourers, manufacturing labourers, helpers and cleaners in offices and hotels, and street food vendors. Amongst the semi-skilled, shares of total job loss by occupation largely followed pre-pandemic employment shares. The notable exception is craft workers (for example, bricklayers, motor vehicle mechanics, and building electricians) who alone accounted for 20% of total employment loss despite representing just 12% of pre-pandemic employment.

The inability for many to participate in the labour market due to the lockdown policy resulted in many of the previously employed and jobseekers to shift into inactivity. Nearly one in every four (22.05%) of those who were employed in 2020Q1 were no longer employed in the following quarter, with most (16.14%) becoming economically inactive. More than half (55%) of the searching unemployed in 2020Q1 became inactive the next quarter. By exploiting the longitudinal nature of the 2020 QLFS data due to the change in survey mode, we examine the conditional correlates of transitioning into various labour market states through the use of multivariate linear probability models (LPMs) by regressing select dependent variables on a vector of pre-pandemic demographic and labour market characteristics. We present the results of these models visually in several coefficient plots in Figure 5, while the complete results are presented in Table A1 in the Appendix. Our results confirm much of our prior descriptive analysis but provide a more in-depth account of the dynamics of the South African labour market during the beginning of the pandemic. Broadly, the pandemic disproportionately affected workers in the informal sector, the youth, and those with lower levels of formal education, whereas union members and public sector workers exhibit a notable extent of job protection.

Specifically, our estimates suggest that those employed in the informal sector were significantly *more* likely to become unemployed, whereas those *less* likely to experience such a transition include women, older individuals, White relative to African/Black individuals, and those whose contract is of a permanent nature. We observe no significant variation in the probability of transitioning from employment to unemployment by industry or occupation. Notably, those working in the public sector were significantly *less* likely to transition from employment to either unemployment or inactivity. Considering the latter transition, women were *more* likely to become inactive after being employed (as opposed to becoming unemployed, as observed above), in addition to those with less than a complete secondary education, union *non*-members, and those with verbal employment contracts. Youth were also more likely to experience an employment-inactivity transition relative to older individuals.

Figure 5: Coefficient plots of conditional probabilities of transitioning between labour market statuses, 2020Q1-2020Q2

3. Quasi-experimental results

We now turn to presenting our main results as per our PSM-DiD regression models. First, Table 3 presents the unweighted and weighted estimates of our treatment groups and outcome of interest – employment – prior to and during the first three months of the national lockdown.[15] Employment among both the treatment and control groups reduced substantially – by 13.6% on aggregate. This observation that both groups experienced a reduction in employment is not a concern for our DiD analysis given that this can be considered as a group 'fixed effect', which is controlled for in the model. However, and notably, this contraction in employment was much more severe among the treatment group which exhibited a reduction of 24.5% - a nearly 5 fives larger reduction than the control group (4.9%). This simple between-group comparison over time is suggestive of a significant treatment effect of the national lockdown, explored in more detail in our PSM-DiD regressions to follow.

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[15] The sample sizes in the treatment period are expectedly smaller given the reduction in the QLFS sample.

Table 3: Sample sizes and weighted estimates of treatment groups, by period

	Pre-lockdown (2020Q1)			During lockdown (2020Q2)		
	Treatment	Control	Total	Treatment	Control	Total
Observations (unweighted)	10 355	11 999	22 354	6 439	8 240	14 679
Observations (weighted)	9 933 383	11 236 771	21 170 154	9 171 160	11 327 857	20 499 017
Employed (unweighted)	7 471	9 572	17 043	3 770	6 231	10 001
Employed (weighted)	7 293 494	9 088 427	16 381 921	5 508 271	8 639 945	14 148 215

Source: QLFS 2020Q1 and 2020Q2 (StatsSA, 2020a and 2020b). Authors' own calculations.
Notes: [1] Sample restricted to working-age individuals (15-64 years). [2] Relevant estimates weighted using sampling weights.

3.1. Main results

Given the importance of the parallel trends assumption in identifying causal effects in a DiD design, we estimate and present an event study plot to examine whether the treatment and control groups were comparable on dynamics in the pre-treatment period. Including leads and lags in the DiD model in this way allows one to check both the degree to which the post-treatment treatment effects were dynamic, and whether the two groups were comparable on outcome dynamics pre-treatment (Cunningham, 2020). We present the unconditional model coefficients and 95% confidence intervals in Figure 6 to examine the parallel trends assumption prior to controlling for a vector of covariates, individual FEs, and the matched sample. Firstly and importantly, all of the pre-treatment coefficients are not statistically significant from zero, implying that individuals in the treatment group did not statistically significantly differ from those in the control group on average. Secondly, post-treatment the probability of employment decreases and is statistically significantly different from zero – indicative of a significant treatment effect.

Figure 6: Event study plot of the unconditional effect of the lockdown on the probability of employment
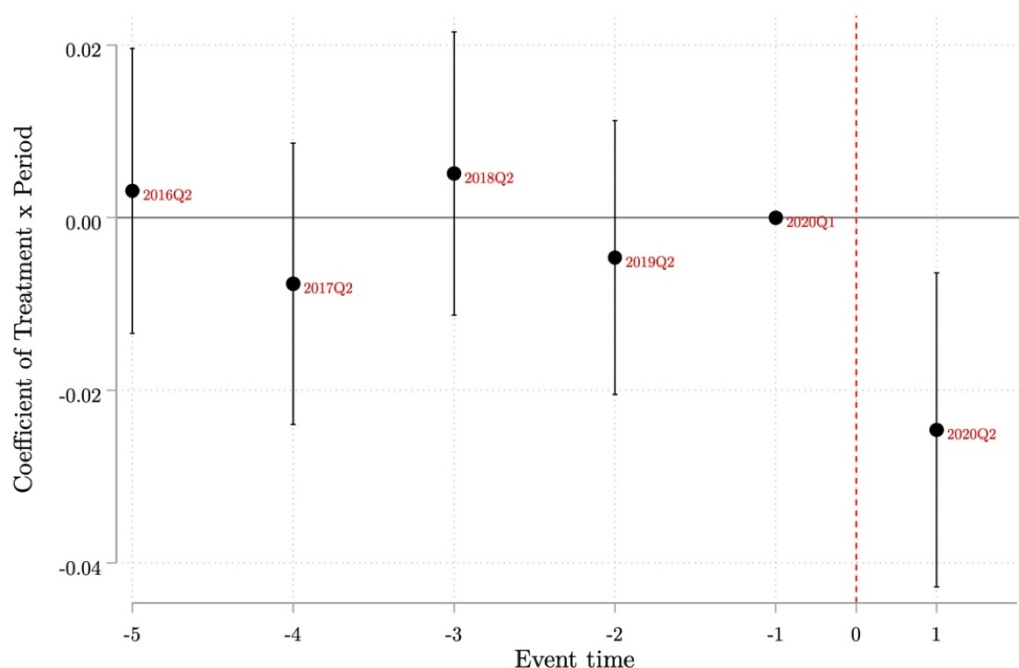
Source: QLFS 2016Q2, 2017Q2, 2018Q2, 2019Q2, 2020Q1, and 2020Q2 (StatsSA). Authors' own calculations.
Notes: [1] Figure presents OLS coefficients of interaction terms of treatment and period from the DiD regression of the effect of the lockdown on the probability of employment, without controlling for individual FEs or the vector of control variables. [2] Sample restricted to working-

age population (15 to 64 years). [3] All estimates weighted using relevant sampling weights. [4] Standard errors clustered at the individual level. [5] Capped spikes represent 95% confidence intervals. [6] Treatment group here excludes those who can work from home due to data limitations in pre-treatment years.

We next present our main PSM-DiD estimates for the estimated effect of South Africa's national lockdown on the probability of employment in Table 4, where the coefficient of interest represents the difference in employment probability between those permitted and not permitted to work from before to during the national lockdown period. Our preferred model is (6) which is restricted to the matched sample and controls for individual FEs and a vector of covariates. We estimate that those not permitted to work (treatment) were 6.4 percentage points more likely to be employed *prior* to the intervention on average. From before to after the onset of the lockdown, those permitted to work exhibited a decrease in the probability of employment of 7.3 percentage points. Our main estimate of interest – the DiD term – suggests that the lockdown decreased the probability of employment for those not permitted to work by 8 percentage points relative to the control group, significant at the 1% level.

Table 4: Propensity score-matched difference-in-difference estimates of the effect of the lockdown on employment

| Sample: | Probability of employment | | | | | |
| | Unmatched | | | Matched | | |
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.075***	-0.048***	0.061***	-0.064***	-0.057***	0.064***
	(0.007)	(0.008)	(0.013)	(0.009)	(0.010)	(0.015)
Post	-0.084***	-0.079***	-0.065***	-0.089***	-0.092***	-0.073***
	(0.006)	(0.009)	(0.008)	(0.010)	(0.015)	(0.014)
Treatment x Post	-0.049***	-0.055***	-0.085***	-0.023	-0.025	-0.080***
	(0.012)	(0.012)	(0.010)	(0.017)	(0.017)	(0.012)
Constant	0.809***	0.135***	0.170	0.959***	0.056	-0.159
	(0.005)	(0.050)	(0.299)	(0.021)	(0.104)	(0.326)
Controls	N	Y	Y	N	Y	Y
Fixed effects	N	N	Y	N	N	Y
Observations	37 033	36 583	27 303	22 328	22 278	15 576
Adjusted R^2	0.020	0.104	0.081	0.020	0.100	0.097

Source: QLFS 2020Q1 and 2020Q2 (StatsSA, 2020a and 2020b). Authors' own calculations.
Notes: [1] Sample restricted to working-age population (15 to 64 years). [2] All estimates weighted using relevant inverse probability weights. [3] Standard errors clustered at the individual level.

In Table 5, we analyse how the treatment effect varies by lockdown level, hypothesising that the effect will be larger during more stringent lockdown levels where a larger group of workers were not permitted to work (for instance, level 5) relative to more lenient levels (level 3). When we disaggregate treatment by lockdown level,[16] we find (as expected) that the estimated effect is larger for more stringent lockdown levels: those not permitted to work in level 5 were 9.3 percentage points less likely to be employed during the lockdown relative to the control group, while for level 4 this decreases to 7.8 percentage points. We find no significant effect on differential employment probabilities during level 3. This latter null result may be driven by the fact that most individuals in our sample were permitted to work in level 3. On the other hand, the latter estimation relied on a particular small, matched sample due to a small treatment group. As such, we do not consider this estimate reliable considering the small sample size.

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[16] We do so by only including individuals in the treatment group for a given lockdown level if they were not permitted to work in the lockdown level and they were interviewed during the lockdown level.

Table 5: Propensity score-matched difference-in-difference estimates of the effect of the lockdown on employment, by lockdown level

Sample:	Matched			
Lockdown level:	Overall	Level 5	Level 4	Level 3
	(1)	(2)	(3)	(4)
Treatment	0.064***	0.012	0.208*	0.197
	(0.015)	(0.038)	(0.116)	(0.221)
Post	-0.073***	-0.058***	-0.046	-0.176*
	(0.014)	(0.016)	(0.029)	(0.092)
Treatment x Post	-0.080***	-0.093***	-0.078**	-0.021
	(0.012)	(0.022)	(0.032)	(0.123)
Constant	-0.159	0.637	0.369	-4.826
	(0.326)	(0.496)	(1.011)	(5.346)
Controls	Y	Y	Y	Y
Fixed effects	Y	Y	Y	Y
Observations	15 576	10 752	7 328	642
Adjusted R^2	0.097	0.125	0.110	0.390

Source: QLFS 2020Q1 and 2020Q2 (StatsSA, 2020a and 2020b). Authors' own calculations.
Notes: [1] Sample restricted to working-age population (15 to 64 years). [2] All estimates weighted using relevant inverse probability weights. [3] Standard errors clustered at the individual level.

3.2. Effect heterogeneity: Triple difference-in-differences estimates

The above estimated effect ought to be interpreted as an average treatment effect on the treated (ATT); however, it is plausible that this effect varies between different groups of workers, particularly among those in more vulnerable labour market positions. To investigate possible heterogeneity, we re-estimate our above PSM-DiD model by interacting the DiD term with binary variables for specific vulnerable sub-groups using triple difference-in-difference models.[17] The results of these models for select sub-groups of individuals are presented in Table 6.

We find statistically significant, negative effects for two distinct groups: individuals living in urban areas versus those in rural areas, and own-account workers versus employees. We estimate a particularly large effect for the latter group. Specifically, our estimates suggest that the national lockdown decreased the probability of employment for those who live in urban areas and were not permitted to work by 5.3 percentage points relative to the control group. Amongst own-account workers, the relative effect was a reduction in the probability of employment by 22.4 percentage points – an effect nearly three times larger compared to the ATT of eight percentage points observed above.[18] We do not find any evidence of variation in effects by sex, age, education, or skill level. Importantly, given that the vast majority of own-account workers work in the informal sector (86.4% of own-account workers, or 1.4 million workers as of 2020Q1), this result is arguably indicative of the disproportionate effect of the lockdown on informal sector workers – in line with our descriptive analysis in Section 2.[19] What this suggests is that, while the ostensible disproportionate effects amongst other vulnerable groups (the youth, less-educated, and less-skilled) seem to be muted in these conditional estimates, working in the informal sector seems to be a key determinant of not being employed during the lockdown period.

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[17] Also referred to as difference-in-difference-in-difference (DiDiD) models.

[18] Importantly, these effects by group (i.e. for urban individuals and own-account workers) do not imply that only these groups were affected by the lockdown, but rather that the effects relevant to them are statistically significantly different relative to their counterparts (i.e. non-urban individuals and non-own-account workers).

[19] The informal sector here includes workers in private households. As opposed to own-account workers, we are unable to estimate a triple DiD effect for an explicit informal sector group of workers, given that only the employed were asked the relevant question in the QLFS.

Table 6: Propensity score-matched triple difference-in-difference estimates

Group:	Male	Urban	Youth	Completed secondary or more	Own account worker	Low-skill worker
Treatment	0.064**	0.074*	0.069***	0.056***	0.060***	0.082***
	(0.031)	(0.042)	(0.018)	(0.020)	(0.020)	(0.023)
Post	-0.105***	-0.074***	-0.065***	-0.069***	-0.074***	-0.063***
	(0.023)	(0.024)	(0.014)	(0.016)	(0.014)	(0.015)
Treatment x Post	-0.049**	-0.033	-0.076***	-0.099***	-0.053***	-0.084***
	(0.024)	(0.027)	(0.017)	(0.020)	(0.015)	(0.018)
Group	0.002	n. e.	-0.015	-0.023	0.128***	0.034
	(0.060)		(0.050)	(0.033)	(0.048)	(0.028)
Treatment x Group	0.000	-0.012	-0.010	0.025	0.049	-0.051*
	(0.035)	(0.043)	(0.035)	(0.030)	(0.057)	(0.030)
Post x Group	0.046**	0.000	-0.025	-0.007	-0.009	-0.037
	(0.021)	(0.026)	(0.024)	(0.021)	(0.038)	(0.023)
Treatment x Post x Group	-0.036	-0.053*	0.010	0.042	-0.224***	0.038
	(0.030)	(0.032)	(0.033)	(0.030)	(0.075)	(0.035)
Constant	-0.168	-0.169	0.726***	-0.202	-0.144	-0.181
	(0.354)	(0.350)	(0.084)	(0.348)	(0.345)	(0.343)
Controls	Y	Y	Y	Y	Y	Y
Fixed effects	Y	Y	Y	Y	Y	Y
Observations	15 576	15 576	15 576	15 576	15 576	15 576
Adjusted R^2	0.096	0.095	0.092	0.095	0.098	0.094

Source: QLFS 2020Q1 and 2020Q2 (StatsSA, 2020a and 2020b). Authors' own calculations.
Notes: [1] Sample restricted to working-age population (15 to 64 years). [2] All estimates weighted using relevant inverse probability weights. [3] Standard errors clustered at the individual level. [4] n.e. = not estimated. [5] All models estimated on the matched sample.

3.3. Robustness tests

In this section, we conduct several robustness tests to examine the sensitivity of our results to alternative (i) control group definitions, (ii) 'limited capacity industry' assumptions, and (iii) employment definitions. Regarding (i), in our main results our treatment group consistently consists of individuals legally not permitted to work during a given lockdown level at the time they were interviewed, and the control group consists of individuals who were legally permitted to work, as well as anyone able to work during the lockdown (measured by working in the public sector or from home). Here we re-estimate our PSM-DiD models to examine the implications of including the latter two groups of workers in the control group. Regarding (ii), in our main results we assume that individuals were permitted to work if their industry's legislated capacity was equal to, or exceeded, 50%, and not otherwise. This is an arbitrary threshold and has implications for who is included in our control group, thus influencing our results. To examine the sensitivity of our results to this assumption, we re-estimate our PSM-DiD models using several alternative threshold assumptions to assign relevant individuals to the control group, as outlined in Table A3 in the appendix.[20]

Regarding (iii), our use of the conventional definition of employment as per ILO standards may not be the most appropriate in the lockdown context. As discussed by Ranchhod and Daniels (2020), under the conventional definition, the employed includes those workers temporarily absent from work. In usual circumstances this group constitutes a small proportion of the employed (less than a percent in

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[20] Table A3 presents the four alternative assumptions we make, and their implications for our treatment-group samples. Under a 'very progressive' assumption, we assign individuals to the control group if *any* proportion of their industry was permitted to work. As expected, this results in the relatively largest control group of 31 500 observations. Under the 'very conservative' assumption, we assign individuals to the control group only if 100% of their industry was permitted to work. This results in a much larger treatment group and smaller control group. Intuitively, moving from 'very progressive' to 'very conservative' increases (decreases) the size of our treatment (control) group. Our main results – which use the '50%' assumption – can be regarded as moderate in this regard.

2020Q1) with half attributing their absence to vacation leave. In the lockdown context, however, this group no longer represents a negligible part of the labour force. During the lockdown in 2020Q2, 6% of the employed were absentee workers – a more than eightfold increase, with most (89%) attributing their absence not to vacation leave but "other reasons". Here, we make use of several alternative employment definitions to examine the sensitivity of our main results. We summarise the results of these tests through the use of a coefficient plot in Figure 7, where the complete regression results are presented in Tables A2, A4, and A5 in the appendix.

Regardless of control group definition, employment definition, or 'limited capacity industry' assumption, we continue to find statistically significant and negative employment effects which vary between 4 and 8 percentage points, with the exception of one even larger negative effect. The estimated effect is smallest (50% of the estimate of our preferred control group) when the control group either includes only those legally not permitted to work, or additionally those who work in the public sector (the effects based on these two definitions are not statistically different from one another). When the control group consists of those who were permitted to work or could work from home, the estimate increases to -0.074, which is not statistically different from the estimate for our preferred control group: -0.080. This suggests that our main result is slightly sensitive to control group criteria. Irrespective of this, however, the estimated effect is consistently negative and statistically significant – in line with our overall finding. Although the estimate under the very progressive 'limited capacity industry' assumption is nearly two times larger in magnitude compared to our main estimate, we note that in three of the four assumptions we find very similar results to that of our main estimate. Arguably, this 'very progressive' assumption – that is, individuals are permitted to work if *any* capacity of their firm in their industry was permitted to work – is not a plausible assumption. We therefore take the remaining estimates as evidence that our main result under the moderate '50%' is fairly robust to alternative assumptions. Lastly, when alternative employment definitions are used as opposed to the conventional definition, we consistently estimate significant negative effects ranging between 4.2 and 5.6 percentage points – none of which are statically different from our main estimate.

Figure 7: Coefficient plot of summary of robustness test estimates
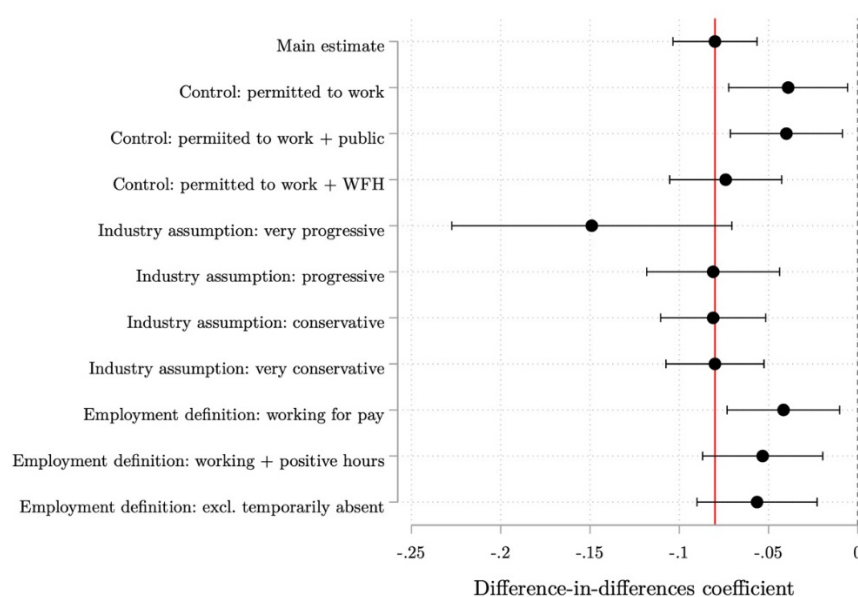
Source: QLFS 2020Q1 and 2020Q2 (StatsSA, 2020a and 2020b). Authors' own calculations.
Notes: [1] Figure presents estimated coefficients and 95% confidence intervals for the DiD term from PSM-DiD regressions for multiple robustness tests. Complete model results presented in Table A2, A4, and A5 in the appendix. [2] Sample restricted to working-age population (15 to 64 years). [3] All estimates weighted using relevant inverse probability weights. [4] Standard errors clustered at the individual level. [5] Red vertical line equal to the main estimate of -0.08. [6] WFH = work from home.

4. Conclusion

South Africa imposed a quick, relatively stringent national lockdown in response to the COVID-19 pandemic to prepare the necessary health infrastructure, as well as to delay and minimise the spread of the virus. Although the pandemic continues to pose important risks to public health, the lockdown was always expected to result in substantial short- and long-term economic costs. Several studies using data collected during South Africa's lockdown show that these costs have been disproportionately borne by several vulnerable groups, such as less-skilled, low-wage, informal, and female workers.[21] Although important, these studies are however largely descriptive in nature and therefore cannot distinguish employment effects attributable to domestic lockdown policy versus other pandemic-related factors. In this paper, in addition to providing a detailed descriptive account of the effects of the COVID-19 pandemic on employment in the South African labour market, we exploit quasi-experimental variation in the country's national lockdown policy to estimate the causal effect of the country's lockdown policy on the probability of employment. By cross-examining the relevant legislature with three-digit industry codes in representative labour force data, we exploit the coincidental timing of the lockdown and survey data collection dates through the use of a propensity score-matched (PSM) difference-in-differences (DiD) approach.

Our descriptive analysis shows that, of the 2.2 million fewer individuals employed in the first few months of the lockdown, short-term net employment loss was concentrated amongst the youth, those with lower levels of formal education, those living in urban areas, lower- and semi-skilled workers, and those working in the secondary sector. Almost all employment loss was observed in the private sector or amongst union non-members. Notably, the lockdown disproportionately affecting individuals working in the informal sector – despite these workers accounting for just under 25% of pre-pandemic employment. Considering outcomes other than employment, we document the notable changes in the distribution of working hours and the substantial increase in inactivity. This latter observation is characteristic of national lockdown policy, which induced an inability for both job-losers and jobseekers to engage in the labour market.

In our quasi-experimental analysis, we attempt to estimate the causal employment effect of South Africa's national lockdown policy itself, isolating it from other pandemic-related factors such as foreign policy and consumption-related behavioural responses. Our results suggests that, relative to the control group, the national lockdown decreased the probability of employment for those not permitted to work by 8 percentage points in the short-term. This significant and negative effect holds when subjected to several robustness tests relating to varying control group definitions, employment definitions, and assumptions regarding industries in which individuals were permitted to work, but at limited capacity. Because this estimate reflects the difference in employment probabilities between the treatment and control groups as a result of the policy, we can use it to approximate the proportion of aggregate job loss that is directly attributable to the lockdown policy as opposed other pandemic-related factors discussed above. Moreover, our research design takes into account any factor that would otherwise affect both groups – in other words, economy-wide effects. We estimate that, of the 2.2 million quarter-on-quarter contraction in employment, South Africa's specific lockdown policy directly accounted for 583 480, or 26.1% of total jobs lost.[22] However, this pure lockdown effect may have some degree of uncertainty. Accounting for our robustness test results, the magnitude of this estimate may be as low as 284 500 (12.7% of total jobs lost). Overall, this suggests that the majority

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[21] See Benhura and Magejo (2020), Casale and Posel (2020), Casale and Shepherd (2020), Hill and Köhler (2020), Jain *et al.* (2020), and Ranchhod and Daniels (2020).

[22] We arrive at this estimate by augmenting the percentage reduction in employment exhibited among the control group (5%: what the treatment group would have experienced in the absence of treatment under the parallel trends assumption) with the estimated ATT of 8 percentage points, and then using this augmented estimate (13%) we calculate the reduction in employment among the treatment group and multiply it by approximately 0.62 (the ATT proportion of the augmented estimate).

of South Africa's short-term job loss can be attributed to other pandemic-related factors, such as foreign policy or changes in consumer behaviour.

We observe significant heterogeneity by lockdown level, with an estimated effect of nearly 10 percentage points for the most stringent level. Our triple difference-in-differences analysis suggests that two distinct sub-groups were affected significantly: individuals living in urban areas (versus those in rural areas) and own-account workers (versus employees). The estimated effect for the latter group was nearly three times larger than the overall average treatment effect, indicative that working in the informal sector seems to be a key determinant of not being employed during the lockdown period. The vulnerability of the informal sector in South Africa to the economic consequences of the pandemic is of concern, given that their informality presents a challenge for government to provide targeted relief. Moreover, the informal sector has struggled to bounce back. By the end of 2020, although the number of job-seekers has returned to pre-pandemic levels, with employment remaining significantly lower and inactivity higher, only one third of jobs lost in the informal sector had been regained. To prevent further widening labour market inequalities, our analysis emphasises the importance of effective policy the support the livelihoods of those in the informal economy. In future work, we intend to estimate the effects of the lockdown on alternative extensive labour market outcomes other than employment such as unemployment and inactivity, as well as intensive-margin outcomes like working hours and wages among those who remained employed. Considering that wage inequality is the predominant driver of income inequality in South Africa, availability of this latter data will also allow us to examine heterogeneity in effects across the wage distribution. Importantly, more data will give us new information on the nature and extent of recovery in the labour market and, unfortunately, the scale of permanent job destruction across the South African economy.

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Appendix

Table A1: Linear probability model estimates of labour market status transition probabilities, 2020Q1 to 2020Q2

	(1)	(2)	(3)	(4)	(5)
	Employment --> Unemployment		Employment --> Inactivity		Unemployment--> Inactivity
35–49	-0.027***	-0.018**	-0.054***	-0.038***	-0.048**
	(0.009)	(0.009)	(0.010)	(0.010)	(0.020)
50–64	-0.046***	-0.034***	-0.018	-0.008	-0.006
	(0.010)	(0.010)	(0.013)	(0.014)	(0.038)
Female	-0.012*	-0.013*	0.043***	0.019**	0.019
	(0.007)	(0.008)	(0.008)	(0.009)	(0.017)
Coloured	0.006	0.005	-0.004	0.021	0.069*
	(0.015)	(0.015)	(0.018)	(0.018)	(0.038)
Indian/Asian	0.007	0.034	-0.050**	-0.033*	-0.153
	(0.022)	(0.026)	(0.021)	(0.020)	(0.101)
White	-0.033***	-0.039***	-0.014	-0.023	0.023
	(0.008)	(0.008)	(0.014)	(0.014)	(0.078)
Living together	0.012	0.001	-0.002	-0.021	-0.068*
	(0.012)	(0.012)	(0.014)	(0.015)	(0.036)
Widow/Widower	0.023	0.008	0.017	-0.003	-0.012
	(0.017)	(0.019)	(0.025)	(0.027)	(0.074)
Divorced or separated	0.019	0.014	-0.029	-0.026	-0.072
	(0.018)	(0.019)	(0.019)	(0.019)	(0.068)
Never married	0.028***	0.014	0.032***	0.010	-0.083***
	(0.008)	(0.009)	(0.010)	(0.011)	(0.026)
Secondary incomplete	-0.000	0.000	0.007	0.037**	-0.018
	(0.014)	(0.016)	(0.015)	(0.017)	(0.033)
Secondary complete (matric)	-0.029**	-0.011	-0.049***	0.025	-0.040
	(0.014)	(0.017)	(0.016)	(0.018)	(0.035)
Post-secondary	-0.051***	-0.010	-0.118***	0.013	-0.021
	(0.014)	(0.019)	(0.016)	(0.021)	(0.042)
Urban	-0.009	-0.006	0.008	0.016	0.029
	(0.009)	(0.010)	(0.011)	(0.012)	(0.022)
Eastern Cape	0.027*	0.025	0.021	0.026	0.060
	(0.016)	(0.017)	(0.020)	(0.020)	(0.043)
Northern Cape	-0.005	-0.019	0.031	0.019	0.213***
	(0.019)	(0.017)	(0.026)	(0.025)	(0.054)
Free State	0.002	-0.002	0.031	0.032	0.164***
	(0.016)	(0.017)	(0.022)	(0.022)	(0.044)
KwaZulu-Natal	-0.032**	-0.047***	0.009	0.003	0.211***
	(0.013)	(0.013)	(0.017)	(0.017)	(0.041)
North West	0.000	-0.001	0.008	0.027	0.178***
	(0.018)	(0.018)	(0.024)	(0.024)	(0.053)
Gauteng	0.025**	0.020	0.012	0.016	0.037
	(0.012)	(0.012)	(0.016)	(0.016)	(0.037)
Mpumalanga	-0.035**	-0.045***	-0.013	-0.010	0.335***
	(0.014)	(0.014)	(0.019)	(0.019)	(0.042)
Limpopo	-0.002	-0.009	0.075***	0.057***	0.165***
	(0.016)	(0.017)	(0.021)	(0.021)	(0.050)
Public sector		-0.023**		-0.081***	
		(0.011)		(0.015)	
Permanent nature		-0.092***		-0.133***	
		(0.016)		(0.017)	
Unspecified duration		-0.004		-0.028	
		(0.022)		(0.022)	
Union non-member		0.005		0.035***	
		(0.008)		(0.011)	

	(1)	(2)	(3)	(4)	(5)
	Employment --> Unemployment		Employment --> Inactivity		Unemployment--> Inactivity
Union membership unknown		-0.004		-0.034	
		(0.021)		(0.023)	
Between 2 and 4 employees		-0.045*		-0.012	
		(0.026)		(0.030)	
Between 5 and 9 employees		-0.021		-0.041	
		(0.029)		(0.038)	
Between 10 and 19 employees		-0.041		-0.050	
		(0.028)		(0.037)	
Between 20 and 49 employees		-0.010		-0.044	
		(0.029)		(0.037)	
50 or more employees		-0.022		-0.038	
		(0.028)		(0.036)	
Do not know firm size		-0.010		-0.022	
		(0.030)		(0.037)	
Do not have written contract		-0.001		0.050**	
		(0.021)		(0.023)	
Weekly working hours		-0.001		-0.002***	
		(0.000)		(0.000)	
Informal sector		0.049**		0.005	
		(0.023)		(0.029)	
Private households		0.017		-0.004	
		(0.043)		(0.049)	
Mining and quarrying		0.012		0.046	
		(0.024)		(0.030)	
Manufacturing		0.035		0.066***	
		(0.022)		(0.024)	
Utilities		0.031		0.062	
		(0.032)		(0.045)	
Construction		0.038		0.099***	
		(0.027)		(0.029)	
Wholesale and retail trade		0.012		0.034	
		(0.022)		(0.023)	
TSC		0.019		0.068**	
		(0.025)		(0.029)	
Finance		0.020		0.018	
		(0.022)		(0.023)	
CSP		0.026		0.099***	
		(0.023)		(0.025)	
Other		0.025		-0.059**	
		(0.051)		(0.028)	
Professionals		-0.008		-0.014	
		(0.014)		(0.017)	
Technical professionals		-0.006		0.021	
		(0.014)		(0.018)	
Clerks		0.004		0.016	
		(0.015)		(0.016)	
Service and shop workers		0.010		0.062***	
		(0.015)		(0.018)	
Skilled agricultural		-0.050		0.018	
		(0.042)		(0.062)	
Craft and related trades		0.020		0.074***	
		(0.018)		(0.021)	
Plant and machine operators		-0.016		0.029	
		(0.018)		(0.022)	
Elementary occupation		0.014		0.066***	
		(0.017)		(0.019)	
Domestic workers		-0.028		0.057	
		(0.041)		(0.048)	
Other		-0.057		0.001	

	(1)	(2)	(3)	(4)	(5)
	Employment --> Unemployment		Employment --> Inactivity		Unemployment--> Inactivity
		(0.035)		(0.031)	
Constant	0.113***	0.180***	0.186***	0.231***	0.576***
	(0.023)	(0.052)	(0.024)	(0.058)	(0.056)
Observations	8 934	7 586	10 108	8 449	4 109
Adjusted R^2	0.024	0.062	0.031	0.097	0.037

Source: QLFS 2020Q1 and 2020Q2 (StatsSA, 2020a and 2020b). Authors' own calculations.
Notes: [1] Sample restricted to working-age population (15 to 64 years). [2] All estimates weighted using relevant sampling-adjusted weights for 2020Q2. [3] Reference groups = 15-34, African/Black, Married, Primary education or less, Western Cape, Limited job duration, Union member, Firm size = 1 employee, Formal sector, Agriculture, Legislators. [4] Standard errors clustered at the individual level.

Table A2: Propensity score-matched difference-in-difference estimates, by varying control group definition

Control group definition:	Legally permitted to work	Legally permitted to work, public sector	Legally permitted to work, work-from-home	Legally permitted to work, public sector, work-from-home **(main group)**
Treatment	0.028	0.039**	0.056***	0.064***
	(0.017)	(0.020)	(0.016)	(0.015)
Post	-0.098***	-0.094***	-0.079***	-0.073***
	(0.017)	(0.015)	(0.016)	(0.014)
Treatment x Post	-0.039**	-0.040**	-0.074***	-0.080***
	(0.017)	(0.016)	(0.016)	(0.012)
Constant	-0.179	-0.184	-0.178	-0.159
	(0.347)	(0.347)	(0.348)	(0.326)
Controls	Y	Y	Y	Y
Fixed effects	Y	Y	Y	Y
Observations	15 576	15 576	15 576	15 576
Adjusted R^2	0.090	0.091	0.094	0.097

Source: QLFS 2020Q1 and 2020Q2 (StatsSA, 2020a and 2020b). Authors' own calculations.
Notes: [1] Sample restricted to working-age population (15 to 64 years). [2] All estimates weighted using relevant inverse probability weights. [3] Standard errors clustered at the individual level. [4] All models estimated on the matched sample.

Table A3: Treatment group sample sizes by varied 'limited capacity industry' assumptions

Group	Description	Treatment	Control	Total
Very progressive	Permitted to work if **any** capacity of firm is permitted	5 534	31 497	37 031
Progressive	Permitted to work if **at least 25%** of firm is permitted	12 611	24 420	37 031
Main results	Permitted to work if **at least 50%** of firm is permitted	14 348	22 683	37 031
Conservative	Permitted to work if **at least 75%** of firm is permitted	19 384	17 647	37 031
Very conservative	Permitted to work **only if 100%** of firm is permitted	20 616	16 415	37 031

Source: QLFS 2020Q1 and 2020Q2 (StatsSA, 2020a and 2020b). Authors' own calculations.
Notes: [1] Sample restricted to working-age population (15 to 64 years).

Table A4: Propensity score-matched difference-in-difference estimates, by 'limited industry capacity' assumption

Assumption:	Very progressive	Progressive	Main results	Conservative	Very conservative
Treatment	0.059*	0.058***	0.064***	0.046***	0.058***
	(0.031)	(0.016)	(0.015)	(0.016)	(0.018)
Post	-0.047	-0.084***	-0.073***	-0.089***	-0.063***
	(0.032)	(0.016)	(0.014)	(0.014)	(0.015)
Treatment x Post	-0.149***	-0.081***	-0.080***	-0.081***	-0.080***
	(0.040)	(0.019)	(0.012)	(0.015)	(0.014)
Constant	0.179	0.302	-0.159	0.021	0.123
	(0.533)	(0.364)	(0.326)	(0.330)	(0.328)
Controls	Y	Y	Y	Y	Y
Fixed effects	Y	Y	Y	Y	Y
Observations	7 504	15 094	15 576	19 039	19 039
Adjusted R^2	0.188	0.098	0.097	0.088	0.098

Source: QLFS 2020Q1 and 2020Q2 (StatsSA, 2020a and 2020b). Authors' own calculations.
Notes: [1] Sample restricted to working-age population (15 to 64 years). [2] All estimates weighted using relevant inverse probability weights. [3] Standard errors clustered at the individual level. [4] All models estimated on the matched sample.

Table A5: Propensity score-matched difference-in-difference estimates, by varying employment definition

	Main results	Working for someone for pay	Employed + working positive hours	Excluding absentee workers
	(1)	(2)	(3)	(4)
Treatment	0.064***	0.056***	0.052**	0.053**
	(0.015)	(0.021)	(0.021)	(0.021)
Post	-0.073***	-0.083***	-0.104***	-0.102***
	(0.014)	(0.015)	(0.016)	(0.016)
Treatment x Post	-0.080***	-0.042***	-0.053***	-0.056***
	(0.012)	(0.016)	(0.017)	(0.017)
Constant	-0.159	0.127	-0.318	-0.291
	(0.326)	(0.376)	(0.367)	(0.367)
Controls	Y	Y	Y	Y
Fixed effects	Y	Y	Y	Y
Observations	15 576	13 313	14 693	14 754
Adjusted R^2	0.097	0.082	0.105	0.105

Source: QLFS 2020Q1 and 2020Q2 (StatsSA, 2020a and 2020b). Authors' own calculations.
Notes: [1] Sample restricted to working-age population (15 to 64 years). [2] All estimates weighted using relevant inverse probability weights. [3] Standard errors clustered at the individual level. [4] All models estimated on the matched sample. [5] Employment definitions as follows: (1) conventional, (2) conventionally employed but only if working for someone else for cash or in-kind pay, (3) conventionally employed but only if working positive, non-zero hours, (4) conventional, but temporarily absent workers excluded from sample.