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Can Cash Transfers to the Unemployed Support Economic Activity?

Evidence from South Africa

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Abstract

Persistently high unemployment has plagued South Africa over the last few decades, while concurrently there has been a dearth of state-provided income support to the working-age economically active population. In response to the pandemic the government introduced the COVID-19 Social Relief of Distress grant – the country's first unconditional cash transfer targeted at the unemployed. At the time of writing, however, no causal evidence of the grant's effects exist. We adopt a doubly robust, semi-parametric Difference-in-Differences approach on representative panel labour force data to estimate the contemporaneous and cumulative causal effects of the grant on labour market outcomes. We find robust evidence that the grant increased average employment probabilities by approximately 3 percentage points, an effect largely driven by wage and formal sector employment. Employment effects vary by duration of receipt, with larger effects estimated for the short-term which reduce to zero with additional periods of receipt. We additionally find marginally significant effects on the probability of trying to start a business, but no evidence of any effects on job search. These findings suggest that the grant has performed a multi-purpose role in providing income relief while also enabling a path towards more favourable labour market outcomes.

Keywords

Cash transfers, labour market, South Africa, COVID-19, difference-in-differences

JEL codes

D04, D31, C54, H53, J48, J68

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Résumé

L'Afrique du Sud a connu un taux de chômage élevé et persistant au cours des dernières décennies, alors que peu d'aide publiques directes étaient disponibles à la population économiquement active en âge de travailler. En réponse à la pandémie, le gouvernement a introduit la subvention COVID-19 Social Relief of Distress (SRD) - le premier transfert monétaire inconditionnel du pays destiné aux chômeurs. Au moment de la rédaction de ce rapport, il n'existe cependant aucune preuve causale des effets de cette subvention. Nous adoptons une approche semi-paramétrique doublement robuste de la différence dans les différences sur des données de panel représentatives de la population active afin d'estimer les effets causaux contemporains et cumulatifs de l'allocation sur les résultats du marché du travail. Nous trouvons des preuves solides que la subvention a augmenté les probabilités moyennes d'emploi d'environ 3 points de pourcentage, un effet largement dû à l'emploi salarié et à l'emploi dans le secteur formel. Les effets sur l'emploi varient en fonction de la durée de reception de l'aide, avec des effets plus importants estimés à court terme qui se réduisent à zéro avec des périodes supplémentaires de reception. Nous constatons en outre des effets marginalement significatifs sur la probabilité d'essayer de créer une entreprise, mais aucune preuve d'effets significatifs sur la recherche d'emploi. Ces résultats suggèrent que l'allocation SRD a joué un rôle polyvalent en apportant un supplément des revenus tout en permettant une évolution vers des résultats plus favorables sur le marché du travail.

Mots clés

Transferts monétaires, marché du travail, Afrique du Sud, Covid-19

1. Introduction

Persistently high unemployment has plagued the South African economy over the last few decades, which was only aggravated by the unprecedented crisis caused by the COVID-19 pandemic. By the end of 2021 the narrow unemployment rate had exceeded 35% - the highest on record for South Africa and amongst the highest globally. Concurrently, despite the far-reaching and progressive nature of South Africa's social protection system, prior to the pandemic there was a dearth of state-provided income support to the working-age economically active population. In this light, the government's introduction of the COVID-19 Social Relief of Distress (SRD) grant – targeted at the unemployed – played an important role in addressing this hole in the country's safety net response to the pandemic. Given the country's extent of un-employment, the transfer provided income support to millions of vulnerable, previously unreached individuals in a relatively short amount of time.

Importantly, the COVID-19 SRD grant is the first in South Africa's history to make explicit use of a labour market criterion to determine eligibility, and as such can arguably be considered as a 'labour market vulnerability transfer.' Despite not being its primary aim, it is plausible that the transfer may have played an important role in aiding economic recovery through its effects on labour market behaviour. Indeed, in the context of the pandemic, anti-poverty programmes and economic recovery policy need not be mutually exclusive. At the time of writing, however, no causal evidence exists on the effects of the grant on any outcome, and it is plausible that such effects may vary from those of pre-existing grants which are characterised by markedly different eligibility criteria. Any evidence of such effects ought to be considered by policymakers when deliberating optimal economic recovery policy.

In this paper, we seek to quantitatively investigate whether the COVID-19 SRD grant acted as a source of labour market recovery by estimating the causal effect of receipt on several labour market outcomes: the probability of job search, starting a business, and employment, respectively. Our identification is based on a doubly robust Difference-in-Differences (DiD) approach on representative and panel labour force data collected in 2020 and the beginning of 2021 and the exploitation of a credible proxy receipt identifier in the data to compare the outcomes of recipients and non-recipients from before to after the introduction of the grant. We further analyse effect heterogeneity by employment type and sectoral formality. Given that the presence of treatment timing heterogeneity in our data biases effect estimates in conventional DiD designs, we make use of Callaway and Sant'Anna's (2021) heterogeneity-robust, semi-parametric, staggered DiD estimator which we believe is most appropriate given this study's context and the estimator's less stringent modelling conditions and robustness against model misspecification. For all outcomes, we estimate overall treatment effects and examine effect heterogeneity by duration of receipt and period of initial receipt. Importantly, due to data availability, our analysis is restricted to the grant's 'first phase'; that is, prior to its amended eligibility criteria in 2021 which allowed unemployed recipients of another grant to apply.

We find robust evidence that the grant had notable labour market effects, despite the relatively small size of the transfer. Our preferred models suggest that receipt of the grant increased the probability of employment by just under 3 percentage points, an estimate which is significant at the 1% level. When we examine effect heterogeneity, we find that this effect appears strongly driven by relatively large, positive effects on wage employment and formal sector employment. We do additionally find positive but much smaller effects on the probabilities on self-employment, becoming an employer, and informal sector employment. These effects all vary by duration of receipt: Recipients experience large positive employment effects in the short-term which however reduce with additional periods of receipt. This pattern holds regardless of employment formality or type. Whilst we also find some evidence that these effects may even become negative after one full year of receipt, small subsample sizes prohibit us from making such a conclusion confidently. We also find small, positive effects on the probability of trying to start a business. We do not find strong evidence of any effects on job search. Lastly, it is notable that effects are consistently larger among individuals who first received the grant towards the end of 2020. Given that the grant's initial roll-out period was characterised by several administrative delays, this finding may be indicative of the importance of the role of the efficiency of the grant system in influencing individual outcomes. These results are strongly robust to alternative control group compositions and alternative estimands which seek to address the validity of the design.

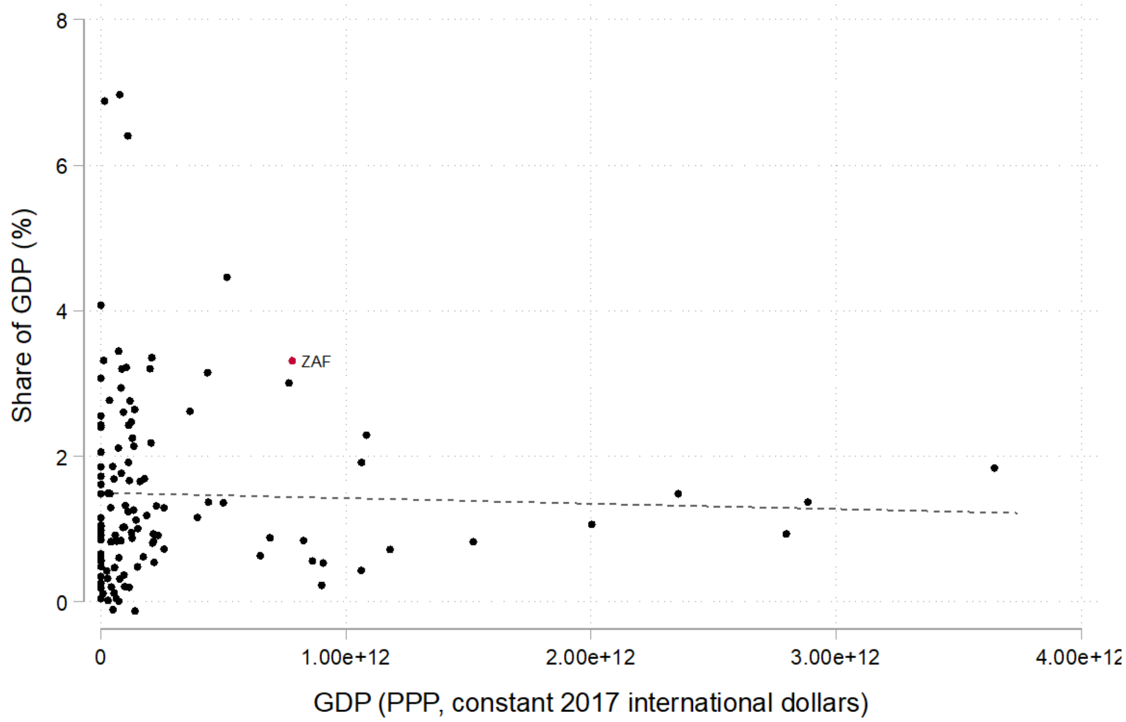
The remainder of this paper is structured as follows. Section 2 provides an overview of South Africa's cointemporary social assistance system and the introduction and evolution of the COVID-19 SRD grant, and reviews the existing empirical literature on the grant's coverage, distribution, and simulated effects on welfare. In Section 3 we describe our data, whereas in Section 4 we describe our identification strategy. We present our results in Section 5 and, following a set of robustness tests in Section 6, we conclude in Section 7.

2. Literature review

2.1. A brief overview of South Africa's contemporary social assistance system

Since democratization, social assistance has expanded significantly with nearly 18 million beneficiaries (or one in every three South Africans) just prior to the COVID-19 pandemic. The country spends a relatively large amount on social assistance given its level of economic development. As shown in Figure 1, which presents an unweighted country-level scatterplot of Gross Domestic Product (GDP) against social assistance expenditure as a share of GDP on a sample of 124 countries for which data is available, South Africa's social assistance expenditure stood at approximately 3.3%. This far exceeds the median spending amount of 1.1% in the sample and 1.4% for countries of a similar level of economic development (upper middle-income countries). Indeed, as indicated by South Africa's marker relative to the fitted regression line, the country's level of spending exceeds the predicted level given its level of economic development. However, such spending in South Africa is widely documented to be relatively well-targeted towards the poor, which is largely attributable to the use of means testing as a targeting device (Van der Berg, 2014).The majority of the country's social assistance spending (over 90%) are tax-financed, unconditional, and means-tested (except for the Foster Care Grant) cash transfers which primarily target vulnerable children, the elderly, and the disabled. These include seven (excluding the COVID-19 SRD grant) such transfers: the Child Support Grant (CSG), Older Person's Grant (OPG), War Veteran's' Grant (WVG), Disability Grant (DG), Foster Care Grant (FCG), Care Dependency Grant (CDG), and the Grant-in-Aid (GIA).

Figure 1. Unweighted scatterplot of GDP and social assistance expenditure as a share of GDP, by country



Source: Authors' own presentation.
Reproduced with permission from World Bank (2021).

Notes: This figure presents a scatterplot of government expenditure on social assistance as a share of GDP and GDP across 124 countries for which there is data. Most recent estimates for 2009-2016 used. China and India omitted as outliers for visualisation brevity, but their inclusion does not affect the predicted relationship. Red marker = South Africa. Line represents linear relationship obtained through an unweighted bivariate linear regression. Shaded region represents confidence interval.

Prior to the pandemic, the CSG represented the largest grant in the system in terms of number of grants distributed, accounting for 71% (nearly 13 million) of total grants distributed in 2019-2020. As of the end of June 2020, more than three in every five children (64.2%) in South Africa had a caregiver receive a CSG on their behalf. The grant's large take-up is largely attributable to gradual increases in the age eligibility threshold and a less stringent means test. The overwhelming majority of CSG recipients (and every other grant type with the exception of the WVG) are women. As of the end of June 2020, of the 7.2 million CSG recipients (not beneficiaries), just 166 000 (or 2.3%) were men (SASSA, 2020). The OPG and the DG – the only grant intended for working-age adults until the introduction of the COVID-19 SRD grant – represent the second and third largest grants collectively accounting for more than one in every four recipients. More than one in every two South Africans live in a household that receives income from either the CSG or OPG (Bassier et al., 2021). Although both the OPG and DG are means-tested, the benefits are more than four times larger than the CSG.

Despite the relatively comprehensive reach of South Africa's social safety net through social grants, prior to the pandemic the working-age population were not covered by any social grant with the exception of the DG as stated above. In this light, the introduction of the COVID-19 SRD grant played an important role in filling this gap through targeting the working-aged unemployed population.

2.2. The introduction and evolution of the COVID-19 Social Relief of Distress grant

Following the onset of the pandemic, the South African government quickly implemented a national lockdown on 27 March 2020. This initial lockdown lasted approximately five weeks and prohibited all non-essential activity outside the home. It entailed the closure of all schools, a curfew, domestic and international travel restrictions, a prohibition of the sale of alcohol and tobacco products, and only workers in occupations deemed essential for economic function and pandemic response were permitted to work at their usual workplace. Given these characteristics, Gustaffson (2020) shows that this initial lockdown was relatively stringent by international standards. Thereafter, the government adopted a five-level risk-adjusted lockdown strategy which implemented regulations according to the severity of contagion. In May 2020 the country moved to the less stringent 'lockdown level 4' which permitted a limited amount of social and economic activity to resume and thereafter lockdown stringency varied as the pandemic progressed. After being in place for approximately two years, this strategy was repealed in April 2022 along with most remaining pandemic-related policy restrictions.

As in many other countries, in response to the pandemic the government introduced a package of targeted economic relief measures to support firms, workers, and households partially by using a combination of existing and new social protection programmes. In particular, the social assistance system was expanded at both the intensive (or vertical) and extensive (or horizontal) margins through a largely cash-based approach. Announced on 21 April 2020, from May to October 2020 the value of every[1] existing cash transfer (or social grant) was increased and a new grant was introduced: the COVID-19 SRD grant. This latter grant, equivalent to ZAR350 per month (approximately US$50 in Purchasing Power Parity (PPP) terms), targeted support to a large group of individuals previously unreached by the system: the unemployed adult population (equivalent to about 10 million people as of the

[1] Except for the GIA, which is a supplementary cash transfer offered to recipients of other cash transfers (either a disability grant, war veteran's grant, or older person's grant) who cannot care for themselves to pay the person who takes care of them.

beginning of 2020[2]). Specifically, individuals were eligible for the grant if they were between 18 and 59 years old, unemployed, and were neither receiving (nor eligible to receive) any other social grant, unemployment insurance benefits, or other forms of government support or income, and were not residing in a government-funded or subsidised institution. The COVID-19 SRD grant is distinct in South Africa's cash transfer system given that it was the first to provide support to unemployed adults for their own benefit. Initially, the grant was conceptualised to target informally employed adults not receiving any social grants, which was not followed through due to concerns surrounding inclusion errors (Bassier et al., 2021). All applications for the grant were done electronically through one of multiple platforms[3], and payments were made into recipient bank accounts or, for the unbanked, through either mobile money transfers or physically at the South African Post Office and later at certain retail outlets.

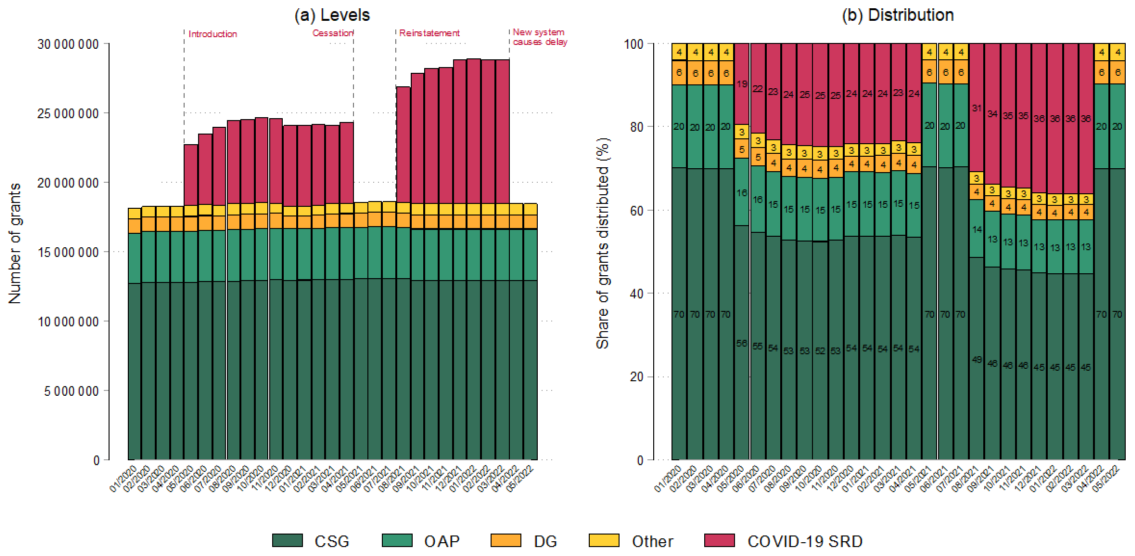
In Figure 2 we plot the absolute and relative distribution of cash transfers in South Africa by grant type from just prior to than pandemic in 2020 to May 2022. Despite initial payment delays owing to the setting up of relevant processes and systems and issues which SASSA experienced in gaining access to the correct databases from other state organisations for verification purposes (Auditor-General, 2020), the introduction of the COVID-19 SRD grant resulted in an initial expansion of the system by over 4 million previously unreached recipients – exceeding the growth of the system in the prior decade (Baskaran et al., 2020). Later in 2020 the grant was extended to April 2021 during which the reach of the grant exceed 6 million, representing approximately a quarter of all social grants distributed. During this period, it is believed that the eligibility criterion that the grant could not be held concurrently with any other social grant resulted in an unfair exclusion of many women, considering that women represent the majority of social grant recipients (85 percent as of December 2020) and the minority of employed (43 percent as of the last quarter of 2020[4]) (Casale and Shepherd, 2022). Indeed, only approximately 30 percent of COVID-19 SRD recipients during this period were women (Gronbach et al., 2022).

[2] Own calculations using microdata from Statistics South Africa's Quarterly Labour Force Survey for the first quarter of 2020.

[3] This includes a dedicated website, a messaging application (WhatsApp), USSD (Unstructured Supplementary Service Data, or text messaging), a call centre, or email.

[4] Own calculations using microdata from Statistics South Africa's Quarterly Labour Force Survey for the fourth quarter of 2020.

Figure 2. Distribution of cash transfers in South Africa by grant type, January 2020 - May 2022



Source: Authors' own calculations.
SASSA (2021; 2022).

Notes: This figure shows the number of social grants distributed per month by grant type, not the number of recipients given that eligible individuals could receive multiple grants at once. Number of COVID-19 Social Relief of Distress (SRD) grants paid for, but not in, a given month shown – there are discrepancies between the two given payment delays. CSG = Child Support Grant; OAP = Old Age Pension; DG = Disability Grant; Other includes Foster Care Grant, Care Dependency Grant, Grant-in-Aid, and War Veteran's Grant.

Three months after the first cycle of the grant ended, in July 2021 the President announced that the grant would be re-instated until March 2022 in response to a resurgence of COVID-19 infections and corresponding lockdown measures as well as a wave of social unrest in parts of the country. Additionally, the grant was expanded to allow unemployed adults who receive the CSG on behalf of their eligible child(ren) to apply. This change in eligibly criteria had a considerable effect on both the level and gender composition of the grant's recipients. As shown in Figure 2, by the end of 2021 the grant reached over 10 million people, and the majority of applicants were now women (57 percent) (SASSA, 2022).

In February 2022, the President announced a further extension of the grant to March 2023, but emphasized that any support beyond this date would need to be assessed (Ramaphosa, 2022). This extension was however characterised by payment delays and low approval rates, primarily due to the necessary administrative systems not being in place in time[5] as well as a new eligibility condition: a means test threshold of ZAR350[6] - equivalent to 56 percent of the country's national statistics office's extreme poverty line (ZAR624).[7]However, in July 2022 the government issued proposed amendments which, among others, included raising the means test threshold to this poverty line. At the time of writing, the grant had been further extended to March 2024 with no clear decision on whether it would be integrated permanently into the system. Finally, it should be noted that since its inception in 2020, the grant amount has remained unchanged in nominal terms at ZAR350, which adjusted for inflation is equivalent to approximately ZAR310 in June 2022 Rands.[8]

2.3. Existing empirical literature on the COVID-19 Social Relief of Distress grant

In this section we summarise the existing empirical literature on the COVID-19 SRD grant. Specifically, we consider studies which have sought to analyse the coverage and distribution of the grant, as well as its (simulated) effects on welfare; namely household incomes and poverty.

Several studies have made use of representative, longitudinal survey data collected during the first year of the pandemic in South Africa – the NIDS-CRAM – to analyse how the grant has been distributed across different groups of individuals and over time. Köhler and Bhorat (2020) show that application for and receipt of the grant was relatively pro-poor. For instance for every individual who lived in quintile 5 households and received the grant in June 2020, nearly four who lived in quintile 1 households received it. Close to 90% of individuals in the former group have never applied. In line with this suggestive progressivity, Visagie and Turok (2022) show that households in typically poorer areas (townships, shack dwellers, and peri-urban areas) were more significantly likely to receive the grant relative to their more affluent counterparts (suburbs), and van der Berg et al. (2022) highlight a link between the temporary cessation of the grant in 2021 and household hunger. Other studies have also considered the notably uneven distribution of receipt by gender, and how such differences persisted over time, largely due to the aforementioned eligibility criteria in the grant's initial phase (Köhler and Bhorat, 2020; Casale and Shepherd, 2022). Regarding targeting, Bhorat and Köhler (2021) show that the grant was relatively well-targeted with

[5] Specifically, because the grant was initially promulgated under the Disaster Management Act which were repealed in April 2022, it became necessary to make provisions to the existing Social Assistance Act for the grant to remain available. All existing recipients then needed to re-apply for the grant, and new conditions for application were only announced at the end of April 2022.

[6] A means test did previously exist for the grant from August 2020, but only for appeal cases for rejected applications (Gronbach et al., 2022).

[7] As set out by Statistics South Africa (2021a), the latest at the time of writing (2021) national poverty lines (per person per month) are as follows in April 2021 prices: Food Poverty Line (also referred to as the "extreme poverty line") (ZAR624); the lower-bound poverty line (ZAR890); and the upper-bound poverty line (ZAR1 335).

[8] Own calculations using Statistics South Africa's Consumer Price Index data (Statistics South Africa, 2022).

most recipients being non-employed, and specifically, holding a range of observable characteristics constant, they find that the chronically non-employed were 51 percent more likely to receive the grant relative to other groups.

Several studies consider the grant's effects on welfare. Bassier et al. (2021) use pre-pandemic nationally representative South African household survey data from 2017 to simulate how the negative economic shock of the pandemic, through a 75 percent[9] reduction in earnings, can be mitigated by different social grant interventions, with a particular focus on informal workers and their households. These interventions include a grant like the COVID-19 SRD grant, except the eligibility criteria is slightly different[10] and the grant value is over 50 percent higher (ZAR526 per month) than the realized policy.[11] They find that the simulated lockdown effect reduces per capita household incomes of informal sector workers by about 30 percent across most of the household income distribution in the absence of any grant intervention. However, after incorporating COVID-19 SRD grant income, they find that this 30 percent reduction is decreased to a reduction between 3 - 20 percent across most of the distribution. Considering poverty effects, their simulated pandemic effect increases extreme poverty[12] from 12.8 to 28.4 percent for informal worker households, but the COVID-19 SRD grant income mitigates this increase to just 16.6 percent. Given that the value of the grant used in this study is substantially higher than that realized, these simulated effects are likely overestimated to some extent.

Bhorat et al. (2021) conduct a similar empirical exercise using the same data, however they compare outcomes using the realized policy (the top-up's to existing grants as well as the introduction of the COVID-19 SRD grant) to alternative policy scenarios such as a simple ZAR500 top-up of the CSG. The authors simulate that, while the chosen policy package leads to a similar degree of poverty reduction compared to the CSG top-up, and the COVID-19 SRD grant is less progressive than the CSG top-up, the grant's key advantage is its ability to provide support to a large group of vulnerable individuals and households who otherwise would not be covered and may be amongst the most negatively affected by South Africa's lockdown. This is in line with Bassier et al.'s (2021) analysis which also simulates that a CSG top-up results in more progressive poverty-alleviating effects but at the expense of an expansion of the system's reach.

9 The choice of 75 percent in this study was based on conjecture rather than an actual estimate.

10 The authors define a recipient as some aged 18 – 59 years who is not formally employed and not receiving any other social grant.

11 The reason why this study employed different characteristics to the COVID-19 grant compared to reality was presumably because at the time of this analysis, the COVID-19 grant was yet to be introduced.

12 Using Statistics South Africa's 2017 extreme poverty line (also known as the food poverty line).

Both Bassier et al. (2021) and Bhorat et al. (2021) make use of pre-pandemic data to arrive at their results, but the results pertaining to poverty reduction hold when data collected during the pandemic is alternatively used. Using a tax–benefit microsimulation model and both pre-pandemic data and the NIDS-CRAM, Barnes et al. (2021) examine the effects of the pandemic on household incomes, poverty, and inequality in South Africa during the first wave of infections from April to June 2020. Their simulation suggests that a decline in earnings would have caused a 25 percent decline in disposable income on average, however the overall reduction was much smaller at 11 percent primarily due to the realized policy package, including the COVID-19 SRD grant. In a recent study, Bassier et al. (2022) update pre-pandemic household income data using pandemic-era labour market data (which lacks income data) to provide contemporary estimates of poverty during 2020 and 2021 in South Africa, while also simulating the poverty-reducing effect of the COVID-19 SRD grant.[13] They find that in the absence of the grant, the headcount poverty ratio[14] increased by 3 – 5.2 percentage points between the first quarter of 2020 and the last quarter of 2021, but after incorporating receipt of the COVID-19 SRD grant, this increase is reduced to 1.1 – 3.4 percentage points. This is in line with Bhorat and Köhler's (2021) simulation which uses the NIDS-CRAM data and suggests that in the absence of the COVID-19 SRD grant extreme poverty would have been at least 5 percent higher. Overall, although their results should be interpreted as purely descriptive and approximate, these studies together provide suggestive evidence on the positive welfare effects of the COVID-19 SRD grant.

[13] The main idea of the authors' approach is the changing of individual employment statuses in pre-pandemic data to match employment effects evident in the pandemic-era data, and then applying the relevant changes in incomes. The assumptions and limitations of this approach are discussed extensively in the paper.

[14] Using Statistics South Africa's upper-bound poverty line.

3. Data

Our analysis here makes use of nationally representative, longitudinal, individual-level household survey data from Statistics South Africa's (StatsSA) Quarterly Labour Force Survey (QLFS) collected in 2020 and 2021. Specifically, we use data for all four quarters of 2020 and the first quarter of 2021 (henceforth 2021Q1) for reasons outlined in Section 4 below. Conducted since 2008, the QLFS 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 South Africa's official source of labour market statistics. More information on the survey's design is available via Statistics South Africa (2008).

Prior to the onset of the COVID-19 pandemic, data was collected via face-to-face interviews from a sample of nearly 70 000 individuals in approximately 30 000 dwelling units per wave. However, following its onset, StatsSA suspended face-to-face data collection and changed the survey mode to computer-assisted telephone interviewing (CATI). To facilitate this transition, the sample that was surveyed in 2020Q1 for which StatsSA had contact numbers for were surveyed again for the remaining quarters of 2020 as well as 2021Q1. Thereafter, the easing of COVID-19-related restrictions resulted in sample rotation to resume. Because only households with valid contact numbers could be surveyed during this period, this sampling decision resulted in a reduction in the sample to approximately 70 percent of its pre-pandemic size. Given concerns of unrepresentative population estimates stemming from potential sample selection bias, StatsSA adjusted the calibrated survey weights using a bias-adjustment procedure which relied on a vector of observable characteristics (Statistics South Africa, 2020e). Although respondents may still be *unobservably* different from non-respondents, the weighted population estimates for the pandemic period do appear plausible (Köhler et al., forthcoming).[15] Throughout our analysis, we employ these sampling weights which account for original selection probabilities and non-response and are benchmarked to known population estimates of the entire civilian population of South Africa. Additionally, the above sampling decision resulted in the survey changing from being cross-sectional with a rotational panel element to an (unbalanced) longitudinal survey – a unique scenario in the survey's history. This panel aspect of the data is key to our identification strategy outlined in more detail below in Section 4.

[15] At the time of writing, an explicit review of this adjustment procedure of the sampling weights had yet to be conducted and would require more information than is available in the public documentation.

Our analysis is based on the unbalanced panel of individuals observed across the five waves of data. To identify the panel sample, we make use of household and person identifiers in the data as well as information on age, gender, and self-reported racial population group to ensure that we observe the same individual over time.[16] We allow for a one-year difference in age in either direction to account for ageing or possible measurement error. We omit all observations that exhibit an inconsistency in any of these characteristics. Furthermore, for identification reasons outlined in Section 4 below, we restrict the sample to adults aged 18 to 59 years who did not receive any unemployment benefits or other social grants (apart from the COVID-19 SRD grant). This procedure results in an unbalanced panel sample of over 52 000 observations.

Table 1 presents the varying sample sizes before and after these adjustments by wave.

Table 1. Sample sizes before and after sample restrictions

Wave	**Complete QLFS cross-sectional samples (1)**	**After primary sample restrictions (18-59 years, no UI, no other grants) (2)**	**After secondary sample restrictions (consistent unbalanced panel) (3)**
2020Q1	66 657	14 350	13 311
2020Q2	47 103	11 176	10 397
2020Q3	47 167	10 434	9 670
2020Q4	48 990	10 618	9 778
2021Q1	45 702	9 900	9 078
Total	**255 619**	**56 478**	**52 234**

Source: QLFS 2020Q1 - 2020Q4 and 2021Q1 (StatsSA, 2020a; 2020b; 2020c; 2020d; 2021b).
Authors' own calculations.

Notes: This table presents sample sizes before and after the primary and secondary sample restrictions.
UI = unemployment insurance.

Despite the magnitude of the sample size reduction, we find that these sample restrictions do not significantly affect the composition of our sample. In Table 2 we present weighted mean estimates of several observable covariates at baseline (2020Q1) for our treatment and control groups described below in Section 4 before and after the sample restrictions. Before the restrictions, COVID-19 SRD grant recipients were slightly more likely to be older and African/Black and less likely to be women, married, live in an urban area, and have a tertiary education level relative to non-recipients. After the restrictions, the signs of all

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

covariate-specific differences remain, and their magnitudes are largely similar with few exceptions. Relative to before the restrictions, recipients in the restricted sample are now even less likely to be female and married, while non-recipients are even more likely to be African/Black and live in urban areas. Both recipients and non-recipients now exhibit statistically similar mean ages of approximately 31 years. These differences in baseline levels, however, are not a concern to the validity of our identification strategy given that, in a DiD design, groups are not required to have similar baseline means in outcomes or covariates, but rather these differences should be stable from before to after treatment (Daw and Hatfield, 2018; Wing et al., 2018). We investigate whether this parallel trends assumption holds, later in the paper.

Table 2. Covariate balance before and after sample adjustments at baseline

	Before sample restrictions			**After sample restrictions**		
	Non-recipient (C)	**Recipient (T)**	**Difference**	**Non-recipient (C)**	**Recipient (T)**	**Difference**
	(1)	(2)	(3)	(4)	(5)	(6)
Age (years)	29.695	33.196	3.501***	31.143	31.659	0.516
	(20.075)	(12.858)	(0.277)	(11.705)	(10.868)	(0.315)
Female	0.515	0.406	-0.109***	0.486	0.309	-0.177***
	(0.500)	(0.491)	(0.011)	(0.500)	(0.462)	(0.014)
African/Black	0.805	0.939	0.134***	0.838	0.938	0.100***
	(0.396)	(0.240)	(0.006)	(0.369)	(0.241)	(0.008)
Married	0.264	0.184	-0.080***	0.250	0.153	-0.097***
	(0.441)	(0.387)	(0.008)	(0.433)	(0.360)	(0.011)
Urban	0.657	0.533	-0.124***	0.681	0.528	-0.153***
	(0.475)	(0.499)	(0.011)	(0.466)	(0.499)	(0.015)
Tertiary education	0.092	0.056	-0.036***	0.078	0.051	-0.027***
	(0.289)	**(0.229)**	**(0.005)**	**(0.268)**	**(0.220)**	**(0.007)**

Source: QLFS 2020Q1 - 2020Q4 and 2021Q1 (StatsSA, 2020a; 2020b; 2020c; 2020d; 2021b). Authors' own calculations.

Notes: This table presents estimates of mean values for observable covariates in the baseline period (2020Q1) for the treatment and control groups accompanied by difference estimates, separately for two samples: the sample before any sample restrictions are made and the sample thereafter. Treatment defined as receipt of the COVID-19 SRD grant (as identified by the 'other' grant in the data) in the post-treatment period. All estimates are weighted using sampling weights. Standard errors presented in parentheses and are clustered at the individual level. The magnitude and statistical significance of a given difference are estimated using t-tests. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. At baseline, the treatment subsample consists of 2 627 and 1 488 observations before and after sample restrictions, respectively, while the control subsample consists of 64 030 and 11 823 before and after sample restrictions, respectively.

4. Identification strategy

4.1. Treatment assignment

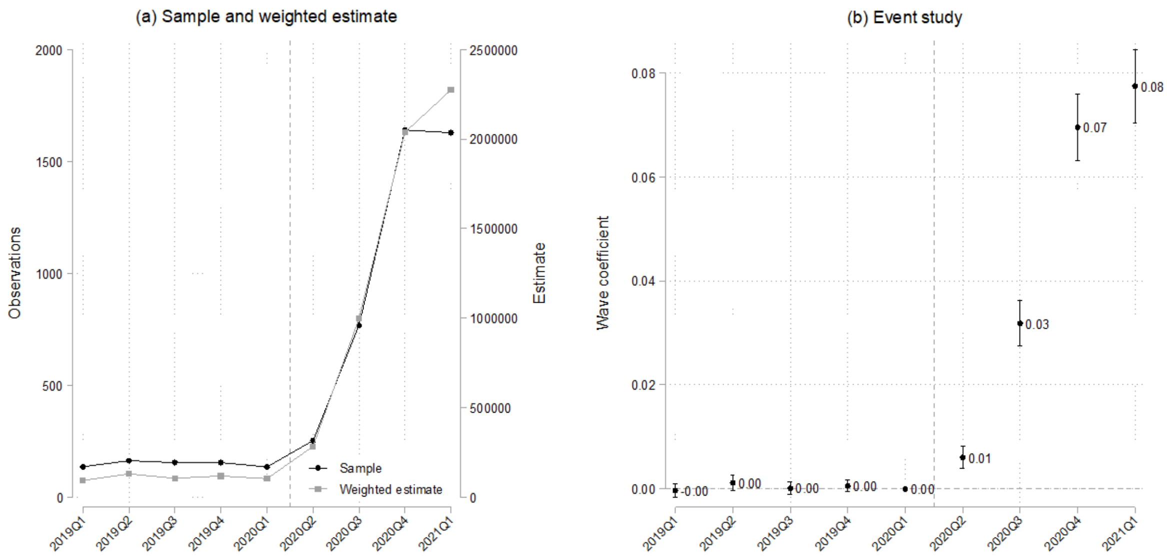
We adopt a Difference-in-Differences (DiD) approach using Callaway and Sant'Anna's (2021) semi-parametric, staggered, and doubly-robust DiD estimator to infer causality. In essence, this approach helps us obtain a credible estimate of the average treatment effect on the treated (ATT) by comparing outcomes between observationally-comparable COVID-19 SRD grant recipients and non-recipients from before to after the introduction of the grant, after accounting for variation in treatment timing. This approach requires two data availability requirements. First, we need data from prior to and after treatment (in this case, the grant). To fulfil this requirement, we make use of five waves of the QLFS from 2020Q1 - 2021Q1 where 2020Q1 serves as the pre-treatment period and most of 2020Q2 through to 2021Q1 as the post-treatment period.[17] Our justification for using these periods is three-fold: (1) our identification strategy relies on the unique but temporary panel nature of the survey, (2) considering payments of the grant occurred from the end of May 2020, it is appropriate for 2020Q1 including April and May of 2020Q2 to serve as the pre-treatment period and June in 2020Q2 through to 2021Q1 as the post-treatment period, and (3) although data from later waves are available, we restrict our post-treatment period to June 2020Q2 to 2021Q1 as this coincides with the first phase of the grant prior to its temporary cessation in April 2021. As such, our analysis and findings are restricted to the first phase of the grant.

The second data requirement is that sampled individuals need to be categorised into treatment and control groups; that is, recipients and non-recipients of the grant. However, the QLFS survey instrument does not include a question which specifically asks about receipt of the COVID-19 grant, but only the Child Support Grant (CSG), Foster Care Grant (FCG), Old Age Pension (OAP), and Disability Grant (DG) amongst the non-employed. However, the survey does include a question of receipt of 'other' welfare grants, which by process of elimination refers to the the War Veteran's Grant (WVG), Care Dependency Grant (CDG), Grant-in-Aid (GIA), or the COVID-19 grant. As shown in Figure 3, individuals were significantly more likely to be 'other' grant recipients in the post-treatment period relative to pre-treatment. In panel (a), which extends the pre-treatment period to one year prior, the number of respondents who answered affirmatively to this question increased substantially from the pre-treatment period (n=150 on average) to the post-treatment period (n=1 073 on average). Indeed, by estimating a linear regression of the binary 'other' grant indicator on time fixed effects to obtain (purely descriptive) event study estimates, panel (b) shows that for one year prior to the treatment period individuals were no more likely to be an 'other'

[17] Use of the NIDS-CRAM data as an alternative dataset here is inadequate given that the first wave was collected in the post-treatment period.

grant recipient relative to the immediate pre-treatment period of 2020Q1 (the estimates are all close to zero and are not statistically significant), but thereafter this probability increased up to 8 percentage points (a difference which is statistically significant at the 1 percent level). Given that, individually and collectively, the number of WVG, CDG, and GIA recipients was relatively constant during both the pre- and post-treatment periods according to administrative data (see Figure 2 in Section 2.2), the significant uptick in 'other' grant recipients in the post-treatment period is arguably due to the variable capturing COVID-19 SRD grant recipients. As such, we make use of this variable, as well as the grant's eligibility criteria, to indirectly identify recipients of the grant in the post-treatment period. It should additionally be noted that because individuals were only surveyed once per quarter, we are only able to observe receipt once per quarter. Although it is of course possible that recipient individuals received the grant multiple (up to three) times per quarter, due to data availability we are unable to be certain of this.

Figure 3. 'Other' social grant recipients covered in the QLFS, 2019Q1 - 2021Q1



Source: QLFS 2019Q1 - 2021Q1 (StatsSA, 2019a; 2019b; 2019c; 2019d; 2020a; 2020b; 2020c; 2020d; 2021b). Authors' own calculations.

Notes: This figure presents in panel (a) the number of observations and respective weighted population estimates of 'Other' social grant recipients as covered in the data over time, and in panel (b) event study estimates on the probability of being an 'Other' grant recipient over time relative to the immediate pre-treatment period (2020Q1). By process of elimination 'Other' includes recipients of the War Veterans' Grant, the Care Dependency Grant, the Grant in Aid, and from the last month of 2020Q2 the COVID-19 Social Relief of Distress (SRD) Grant. Event study estimates obtained from a linear (OLS) regression of a binary 'Other' grant indicator on time fixed effects, weighted using sampling weights and standard errors clustered at the primary sampling unit (PSU) level. Capped spikes represent 95 percent confidence intervals. Vertical line distinguishes the pre-treatment period from the post-treatment period where treatment refers to the introduction of the COVID-19 SRD grant.

Using 'other' grant receipt to identify COVID-19 SRD grant recipients in the post-treatment period may however be biased by contamination. Specifically, coding our binary treatment variable equal to one for 'other' grant recipients in the post-treatment period would include any possible WVG, CDG, and GIA recipients present in the data. We address this by dropping observations who are eligible for these grants as far as we can identify in the data,[18] however we are not very concerned about such contamination given the very small collective magnitude of recipients.

Recall that individuals were only eligible for the COVID-19 SRD grant if they were between 18-59 years old, unemployed, and were not receiving any other form of government support (i.e. any other social grant or unemployment insurance benefits). Additionally, at the same time as the introduction of the grant, the values of all other existing grants were topped-up. To avoid possibly confounding our estimated effect, we restrict our sample to non-employed individuals[19] aged 18-59 years who were not receiving any alternative grant or unemployment insurance benefits. Our treatment group comprises individuals who ever reported receipt of the COVID-19 SRD grant (indirectly identified as an 'other' grant recipient) in the post-treatment period and our control group comprises those who report non-receipt . Overall then, our approach compares temporal outcomes of (1) non-employed adults aged 18-59 who neither receive unemployment insurance benefits nor any social grant (control group: n = 48 259 observations in the panel) to (2) non-employed adults aged 18-59 who neither receive unemployment insurance benefits nor any social grant *with the exception of the 'other' grant* (treatment group: n = 3 975 observations in the panel). Described in more detail below, in our modelling we account for variation in duration of receipt among recipients – that is, some recipients received the grant just once and others multiple times.

To examine whether the identifying assumption of our DiD approach – that is, in the absence of the grant the trends of the outcomes of recipients would have been similar to non-recipients on average – we estimate the mean levels of covariates and outcomes by receipt status and period and the corresponding between-group within-period differences as well as the between-group between-period differences. We present these estimates in Table 3. Recall that balanced mean levels of covariates or outcomes by receipt status within each period is not a requirement in a DiD strategy. However what is important is that the *difference* in the mean levels of a given covariate, but not outcome, by receipt status is

18 WVG recipients are automatically excluded because an individual is only eligible for the grant if they are at least 60 years old (our analysis is restricted to those aged 18 – 59 years). GIA recipients are automatically excluded because an individual is only eligible for the grant if they receive the DG or OPG, who we can identify in the data and exclude from our sample. Unfortunately, given the eligibility criteria of the CDG, we are unable to exclude potential recipients of this grant because the relevant variables (parental status, income, child age, and child disability status) are not available in the data.

19 In any case, grant receipt is only asked of the non-employed in the QLFS, so restricting the sample to this group helps make our treatment and control groups more comparable.

constant from before to after the introduction of the grant. The analysis on covariates is equivalent to a placebo falsification test where the DiD model is estimated separately on covariates which theoretically should not be affected by grant receipt, whereas the analysis on outcomes is equivalent to unconditional DiD estimates. We find that in our sample at baseline, relative to non-recipients, recipients are of a statistically similar age and are significantly more likely to be male (which is in line with survey and administrative data (Casale and Shepherd, 2022; Gronbach et al., 2022) as discussed in Section 2) and self-reported African/Black, and less likely to be married, reside in an urban area, and have a tertiary-level education, as shown in the third column. All differences are statistically significant at the 1% level. Following the introduction of the grant, as shown in the second-last column, the magnitude, direction, and statistical significance of all differences was unchanged, with one exception: Recipients were statistically significantly older than non-recipients on average; however only marginally by less than one year. It is therefore unsurprising that the between-period differences for all covariates, apart from age, are all close to zero and are statistically insignificant, as shown in the last column. These estimates are in support of the parallel trends assumption and hence the validity of our DiD design. Although the significant estimate on age may be of concern, in the section to follow we describe our adoption of Callaway and Sant'Anna's doubly robust estimator which accounts for such inter-group temporal differences.

Table 3. Covariate balance by receipt status and period

	Pre-period			Post-period			Diff-in-diff.
	Non-recipient	Recipient	Diff.	Non-recipient	Recipient	Diff.	
	(1)	(2)	(2) – (1)	(3)	(4)	(4) – (3)	[(4) – (3)] – [(1) – (2)]
Age (years)	31.579	31.911	0.333	31.423	32.34	0.917***	0.585***
	(11.805)	(10.901)	(0.306)	(11.880)	(10.793)	(0.285)	(0.206)
Female	0.482	0.309	-0.173***	0.470	0.312	-0.158***	0.014
	(0.500)	(0.462)	(0.013)	(0.499)	(0.463)	(0.012)	(0.009)
African/Black	0.834	0.931	0.097***	0.829	0.928	0.100***	0.003
	(0.372)	(0.253)	(0.009)	(0.377)	(0.258)	(0.009)	(0.006)
Married	0.260	0.166	-0.095***	0.254	0.154	-0.100***	-0.005
	(0.439)	(0.372)	(0.011)	(0.435)	(0.361)	(0.010)	(0.008)
Urban	0.681	0.537	-0.144***	0.690	0.550	-0.140***	0.004
	(0.466)	(0.499)	(0.014)	(0.462)	(0.497)	(0.013)	(0.009)
Tertiary education	0.083	0.051	-0.032***	0.078	0.052	-0.026***	0.006
	(0.275)	(0.220)	(0.006)	(0.268)	(0.223)	(0.006)	(0.005)

Source: QLFS 2020Q1 – 2020Q4 and 2021Q1 (StatsSA, 2020a; 2020b; 2020c; 2020d; 2021b). Authors' own calculations.

*Notes: This table presents estimates of mean values for observable covariates for the treatment and control groups accompanied by difference estimates in the periods before and after the COVID-19 SRD grant was introduced. Treatment defined as receipt of the COVID-19 SRD grant (as identified by the 'other' grant in the data) in the post-treatment period. Sampling weights employed and standard errors, presented in parentheses, 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.*

4.2. Model specifications

Before accounting for variation in treatment timing, our model can be described as the following canonical DiD specification for individual i in wave t which is estimated using Ordinary Least Squares (OLS):

$$y_{it} = \beta_0 + \beta_1 SRD_i + \beta_2 Post_t + \beta_3 (SRD_i \times Post_t) + \gamma \boldsymbol{X_{it}} + \varepsilon_{it} \tag{1}$$

where y_{it} is our binary labour market outcome variable of interest. We are interested in three outcomes in particular: the probability of job search, the probability of reporting trying to start a business, and the probability of ever gaining employment in the post-period. Job search is measured through the question "In the last four weeks, were you looking for any kind of work?", while trying to start a business is through "In the last four weeks, were you trying to start any kind of business?". Employment is defined as per Statistics South Africa's conventional definition of working for at least one hour in the reference week or not working because of a temporary absence but still having a job to return to. By generating additional outcomes conditional on gaining employment in the post period, we also analyse effect heterogeneity by employment type (wage employment or employee, employer, self-employment, or persons helping unpaid in their household business) and sectoral formality.[20] For each variable, individuals were coded as one if they responded affirmatively and zero if negatively. SRD_i is the binary treatment indicator equal to one for individuals who reported receipt of the COVID-19 SRD grant (indirectly identified as an 'other' grant) at least once in the post-treatment period and zero otherwise, and $Post_t$ is the binary post-treatment indicator equal to one for all observations from June in 2020Q2 to 2021Q1 and zero otherwise. We control for a vector of observable time-varying covariates, $\boldsymbol{X_{it}}$, to reduce residual variance and improve the precision of our estimate, which includes age in years, marital status, a binary urban indicator, highest level of education, and a binary indicator of whether an individual was currently attending an educational institution. ε_{it} is the regression error term. β_3 then represents our coefficient of interest, measuring the estimated average causal effect of COVID-19 SRD grant receipt on our outcome of interest for recipients in the treatment period.

However, a recent and emerging econometric literature has shown that when a DiD design has more than two time periods and heterogenous treatment timing (in other words, units are treated at different points in time – a common occurrence in empirical work), estimates obtained from the above canonical specification are often severely biased and do not correspond with interpretable causal parameters (Borusyak and Jaravel, 2017; Athey and Imbens, 2018; Imai et al., 2018; de Chaisemartin and D'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021; Roth et al., 2022). In brief, this is primarily because such models make both 'clean' comparisons (between treated and not-yet- treated units) as well as 'forbidden' comparisons (between units who are both already-treated but in varying periods) (Roth et al., 2022). Fortunately, a variety of different heterogeneity-robust estimators have been proposed that strictly only use 'clean' comparisons to avoid these issues. These estimators often produce similar answers to one another, however the appropriate one depends on the study context. In our study here, while we estimate the conventional 'problematic' DiD estimator for comparison, we employ Callaway and Sant'Anna's (2021) semi-parametric DiD estimator which we believe is most appropriate in our context of multiple time periods and heterogenous treatment timing. The intuition behind this approach is that only never-treated and/or not-yet treated units (i.e. 'good' comparisons) should be used as the control group, otherwise estimates will be biased. The key concept behind this approach is the group-time average treatment effect, $ATT(g,t)$, where group g refers to the time period that treated units are first treated (here, when individuals first receive the COVID-19 SRD grant), defined as follows:

$$ATT(g,t) = [EY(g)_t - EY(C)_t] - \left[EY(g)_{g-1} - EY(C)_{g-1}\right] \quad (3)$$

[20] Sectoral formality of employment is defined as per Statistics South Africa's conventional definition. The formal sector includes workers who are registered for personal income tax, while the informal sector only includes employees who are not registered for personal income tax and work in establishments that employ fewer than five workers, and all other who are not registered for any tax.

where $EY(g)_t$ is the mean value of the outcome for group g at time t, and $EY(C)_t$ is the mean value of the outcome for the control group C (here, individuals who either never received the grant or had not yet received it) at time t. The first term then calculates the difference in outcomes at time t while the second calculates the difference in outcomes at time $g-1$, which is the period before the first treatment period for group g. This process then can result in a potentially large number of $ATT(g,t)$'s to consider which may be cumbersome to report, as opposed to the singular ATT in conventional DiD studies. However, a particularly attractive feature of the estimator is that it can be used to construct several useful aggregations, including the aggregation of all effects in the post-treatment period for all treatment groups into a singular ATT, the aggregation of such effects for each group g (*how do effects vary depending on when the grant was received?*), and the event study aggregation to study effect dynamics (*how do effects vary by length of exposure or number of times the grant was received*).[21] We make use of these aggregations in our analysis here.

Importantly, two other unique features of this estimator are that it does not require strongly balanced panel data, and that it allows for cases where the parallel trends assumption holds either unconditionally or conditionally (that is, only after controlling for a vector of observable characteristics). In the latter case, the estimator allows researchers to flexibly incorporate covariates into the modelling to obtain more comparable treatment and control groups through three alternative estimands: outcome regression (OR) adjustment using OLS; inverse probability weighting (IPW) with stabilised weights, and a doubly robust (DR) estimand based on Sant'Anna and Zhao (2020). While these approaches are equivalent from the identification perspective, they are not from an inference perspective (Callaway and Sant'Anna, 2021). The OR approach requires a correctly specified model of the outcome evolution of the control group, making it explicitly linked with the conventional conditional parallel trends assumption. The IPW approach avoids relying on such a model restriction but instead requires a correctly specified model of the propensity score of individual i belonging in group g, and that they are either in group g or an appropriate comparison group. On the other hand, the DR approach combines these approaches and thus relies on modelling both the evolution of the outcome *and* the propensity score, however it only requires one but not necessarily both to be correctly specified. Therefore, the DR approach is particularly attractive because it relies on less stringent modelling conditions and enjoys additional robustness against model misspecification. As such, although we report both unconditional and conditional results, for the latter we employ the DR estimand using only time-varying covariates (those included in $\boldsymbol{X_{it}}$) as recommended. We do however re-

[21] It should be noted that the group-time aggregations only allow one to obtain estimates for individuals who first received the grant in 2020Q2, 2020Q3, and 2020Q4 (therefore excluding 2021Q1). Although a subset of individuals in our sample do receive the grant in 2021Q1, we cannot calculate the $ATT(g,t)$ for these individuals because by the time we reach 2021Q1 this group can only function as a comparison group for the earlier ones.

estimate all conditional models using the other two alternative estimands as a robustness test.

It should be noted that this approach assumes that treatment is an 'absorbing state'. In other words, treatment is irreversible: once a unit is treated, they remain treated for the remainder of the panel, such that treatment exposure is 'weakly increasing' (it either remains the same or increases). Although there are very few instances in our data where treatment switches on then off again (i.e. individuals report receipt of the grant in one subperiod post-treatment and then not again later), we believe this to be a fair assumption given that it implies individuals do not "forget about their treatment experience" (or grant receipt in this case). An alternative estimator by de Chaisemartin and D'Haultfœuille (2020) does allow for treatment turning on and off, however only subject to a 'no carryover' assumption which imposes that potential outcomes only depend on *current* treatment status and not on full treatment *histories* (Roth et al., 2022). Given the possibility of dynamic and cumulative effects of grant receipt, we believe this assumption is inappropriate here and thus proceed with Callaway and Sant'Anna's (2021) approach.

All our model estimates are weighted using sampling weights while our standard errors are clustered at the panel (individual) level and are estimated using a multiplicative wild bootstrap procedure (the mammen approach) with 1 000 replications.

5. Results

In this section we present our estimates of average effects of COVID-19 SRD grant receipt using Callaway and Sant'Anna's (2021) estimator. We first present the results for our three primary outcomes and thereafter examine effect heterogeneity by employment type and sectoral formality. In each section, we estimate several aggregations described above to examine heterogenous and dynamic effects of grant receipt; specifically, we analyse how effects vary (i) depending on when individuals received the grant and (ii) by 'treatment exposure' (in other words, by how long individuals had received the grant for). This latter aggregation also allows us to obtain pre-treatment estimates which can be used to gauge the plausibility of the parallel trends assumption.

5.1. Overall effect estimates

Table 4 presents the aggregated treatment effect estimates. Overall, we find evidence of a highly statistically significant and positive effect of COVID-19 SRD grant receipt on the probability of employment, but only a marginally significant and small effect on trying to start a business and no effect on job search. Regarding employment, our preferred estimate in the top panel of column (6) suggests that receipt of the grant increased employment probabilities by just under 3 percentage points on average, which is quite precisely estimated and is significant at the 1% level. This estimate is marginally lower in magnitude but not statistically significantly different from the unconditional estimate in column (5). The aggregated group-time average treatment effect estimates presented in the bottom panel of the table suggest that this positive effect was driven by those who first received the grant towards the end of 2020: initial receipt in 2020Q2, 2020Q3, and 2020Q4 increased average employment probabilities by 3.7, 5.2, and 7 percentage points, respectively, however the 2020Q2 estimate is only marginally significant at the 10% level. The unconditional group-time estimates exhibit a similar pattern. This finding may be attributable to several reasons. For instance, recipients experiencing a greater propensity to engage in job search relative to non-recipients during a period of relatively lower lockdown stringency and greater physical mobility (as discussed by Köhler et al. (forthcoming), the lockdown regulations of earlier periods, such as 2020Q2, prohibited many non-essential activities outside of one's household). Alternatively, by the end of 2020 and relative to earlier periods, the efficiency of the State's grant administration system may have increased with a greater amount of time to adjust to the processing and administering of a new grant to a new pool of previously unreached recipients, resulting in higher take-up rates (as implied in Figure 2).

Table 4. Aggregated average treatment effect estimates of COVID-19 SRD grant receipt

	Pr(Job search)		Pr(Try to start business)		Pr(Employment)	
	Unconditional	Conditional (doubly robust)	Unconditional	Conditional (doubly robust)	Unconditional	Conditional (doubly robust)
	(1)	(2)	(3)	(4)	(5)	(6)
ATT	0.037**	0.009	0.015**	0.012*	0.035***	0.029***
	(0.016)	(0.017)	(0.007)	(0.007)	(0.010)	(0.010)
First treatment group-specific effects						
Mean	0.043***	0.015	0.016**	0.013**	0.044***	0.038***
	(0.016)	(0.016)	(0.006)	(0.007)	(0.009)	(0.010)
2020Q2	0.018	-0.004	0.008	0.006	0.032*	0.037*
	(0.032)	(0.031)	(0.017)	(0.019)	(0.019)	(0.020)
2020Q3	0.029	-0.006	0.018**	0.015**	0.060***	0.052***
	(0.022)	(0.023)	(0.008)	(0.008)	(0.014)	(0.014)
2020Q4	0.081***	0.060*	0.019*	0.015	0.074***	0.070***
	(0.031)	(0.032)	(0.011)	(0.011)	(0.017)	(0.019)
Observations	52 161	51 828	52 161	51 828	52 161	51 828

Source: QLFS 2020Q1 - 2020Q4 and 2021Q1 (StatsSA, 2020a; 2020b; 2020c; 2020d; 2021b). Authors' own calculations.

Notes: This table presents Difference-in-Differences (DiD) estimates of the effect of receipt of the COVID-19 SRD grant on the three primary outcomes of interest. All models are estimated using the Callaway and Sant'Anna (2021) DiD estimator, while conditional models are estimated by additionally incorporating Sant'Anna and Zhao's (2020) doubly robust (DR) estimand. The bottom panel presents the aggregated ATT's across all periods for each first-treatment cohort. Observations never treated and those not yet treated at the time of treatment used as the control group. Sampling weights employed and standard errors, presented in parentheses, are clustered at the panel (individual) level and are estimated using a multiplicative wild bootstrap procedure (mammen approach) with 1 000 replications. ATT = average treatment effect on the treated. Only time-varying controls included: age, marital status, a binary urban indicator, highest level of education, and a binary indicator of whether an individual was currently attending an educational institution. *** p<0.01; ** p<0.05; * p<0.10.

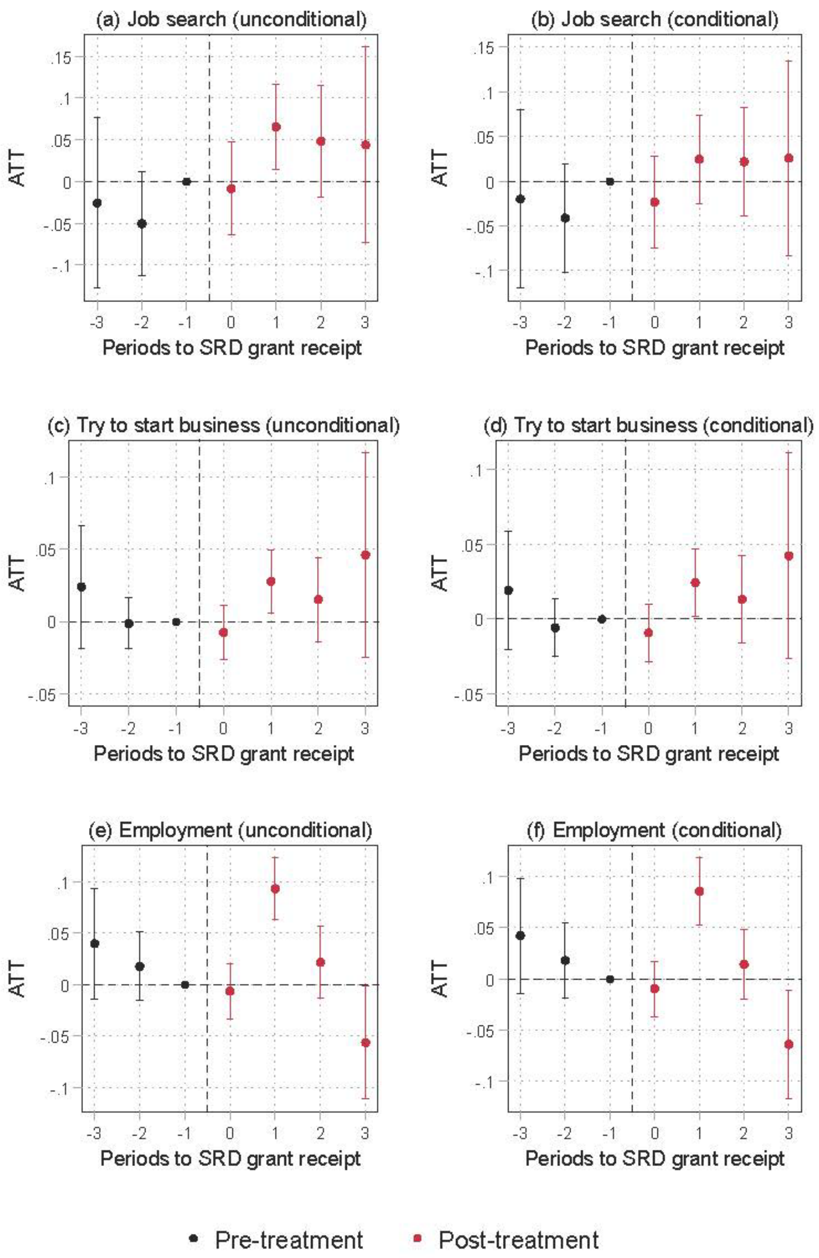
Regarding the probability of trying to start a business, we find only marginal significant evidence of a positive effect. The estimates shown in column (4) suggest that receipt of the grant increased this probability by just 1.2 percentage points, an estimate which is only significant at the 10% level. The aggregated group-time estimates reveal that this effect was driven by individuals who first received the grant in 2020Q3 – the estimate of which is significant at the 5% level. The estimates for all other periods are however statistically insignificant. Concerning job search, the magnitude of the overall effect in column (2) is statistically insignificant, in contrast to the significant results from the unconditional model, with one exception: The aggregated group-time estimates reveal a significant (at the 10% level), positive effect of 6 percentage points among individuals who first received the grant in the last quarter of 2020. A comparison of these results to those obtained using the 'naïve' models, as presented in Table A1 in the appendix, shows that those in the latter are upward-biased with respect to job search and trying to start a business and downward-biased with respect to employment, regardless of whether the conditional or unconditional approach is taken. However, as discussed in Section 4.2 and as opposed to our preferred approach, it should be kept in mind that these 'naïve' estimates do not have a clear interpretation in the presence of treatment timing heterogeneity (Borusyak and Jaravel, 2017; de Chaisemartin and D'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021).

We next examine the dynamics of these estimated effects using Callaway and Sant'Anna's (2021) event study aggregation. Specifically, we examine effect heterogeneity by length of exposure to treatment (or "duration of receipt"), so effects are estimated and scaled for each period relative to the period first treated using all individuals regardless of when they first received the grant. This aggregation is particularly useful for two reasons: (1) it allows us to analyse beyond the immediate impact of the grant (as discussed by Eyal and Woolard (2011a), heterogeneity in effects by exposure to grants are important to consider as they may speak to them being seen as transitory or permanent income shocks), and (2) it allows us to examine whether grant recipients and non-recipients were statistically similar on outcome dynamics in the pre-treatment period, which we use to gauge the plausibility of the parallel trends assumption.

We find that the effect of COVID-19 SRD grant receipt on employment was initially large but decreased substantially the longer recipients were exposed to the grant. As shown in panels (e) and (f) in Figure 4, we find that mean employment probabilities for recipients and non-recipients were comparable prior to receipt. The pre-treatment ATT estimates in both the unconditional and conditional models are statistically insignificantly different from zero, which support the plausibility of the parallel trends assumption. Regarding post-treatment dynamics, the "on impact" (at $t = 0$) average effect on employment is close to zero and is statistically insignificant. However being exposed to the grant for one additional quarter raises this effect to 8.6 percentage points – highly significant at the 1% level. Thereafter, the effect dissipates and approaches zero, and after a fourth quarter (or one complete year) of receipt the estimate becomes negative and statistically significant at the 5% level. The unconditional estimates resemble a similar pattern, although the latter estimate remains statistically similar to zero, which may be due to a relatively small subsample of four-period recipients in our data considering the relatively larger confidence interval.

Regarding job search effect dynamics, we find that like employment, the "on impact" effect of COVID-19 SRD grant receipt was close to zero and statistically insignificant, however these insignificant effects persisted the longer recipients were exposed to the grant. As shown in panels (a), while the unconditional effect estimate of two quarters of exposure is positive and statistically significant, all others are statistically similar to zero. Similarly, panel (b) shows that the magnitudes of all conditional effect estimates are close to zero and statistically similar to it, including that of the statistically significant unconditional estimate. Regardless of the length of exposure to the grant, these estimates suggest that the grant did not induce job search in the labour market.

Figure 4. Event study treatment effect estimates of COVID-19 SRD grant receipt, by outcome and model



Source: QLFS 2020Q1 - 2020Q4 and 2021Q1 (StatsSA, 2020a; 2020b; 2020c; 2020d; 2021b). Authors' own calculations.

Notes: This figure presents unconditional and conditional event study estimates of the effect of COVID-19 SRD grant receipt on the three primary outcomes of interest. All models are estimated using the Callaway and Sant'Anna (2021) DiD estimator, while conditional models are estimated by additionally incorporating Sant'Anna and Zhao's (2020) doubly robust (DR) estimand. ATT's are estimated for each period relative to the period first treated, across all first-treatment cohorts. Here, 'periods to COVID-19 SRD grant receipt' indexes the length of exposure to grant receipt. Observations never treated and those not yet treated at the time of treatment used as the control group. Sampling weights employed and standard errors are clustered at the panel (individual) level and are estimated using a multiplicative wild bootstrap procedure (mammen approach) with 1 000 replications. Capped spikes represent 95% confidence intervals.

On the other hand, the estimates in panel (d) show that while the "on impact" effect of receipt of the grant on the probability of trying to start a business was, again, statistically insignificant from zero, exposure to the grant for one additional quarter increased this probability by 2.4 percentage points – significant at the 1% level. Estimates for longer periods of grant exposure remain positive and grow in magnitude, however are statistically insignificant. As noted above, may be due to a relatively small subsample of four-period recipients in our data considering the relatively larger confidence interval. As such, we cannot rule out the possibility of larger effects for longer exposure durations. Depending on the validity of such effect dynamics, this finding would then be indicative of larger labour market benefits to receiving the grant for longer periods of time as compared to a once-off receipt, a finding which has been previously documented for other grants in the South African literature (Eyal and Woolard, 2011a).

5.1.1. Effect heterogeneity by employment type

Table 5 presents the aggregated treatment effect estimates by employment type. Overall, we find that the highly statistically significant, positive effect on the probability of employment observed above was driven by a positive effect on the probability of wage employment. As shown in column (2), the results from the conditional model suggest that receipt of the grant increased the average wage employment probability by 2.3 percentage points, statistically significant at the 1% level. The aggregated group-time average treatment effect estimates show that, like the overall employment probability effects, this positive effect was driven by those who first received the grant towards the end of 2020: initial receipt in 2020Q2, 2020Q3, and 2020Q4 increased average wage employment probabilities by 3.1, 4.3, and 5.5 percentage points, respectively. We estimate no statistically significant ATT on the probability of being an employer, as shown in column (4), with the estimate being close to zero in magnitude and statistically insignificant. However, we do observe positive and significant albeit relatively small effects among individuals who first received the grant again towards the end of 2020. On the other hand, we do estimate a statistically significant and positive effect on the probability of self-employment, however the effect is relative small (less than 1 percentage point) and only marginally significant. We find no evidence that receipt of the grant had any effect on the probability of engaging in an unpaid household business. As shown in column (8), all estimates in this regard are close to zero in magnitude and statistically insignificant.

Table 5. Aggregated average treatment effect estimates of COVID-19 SRD grant receipt, by employment type

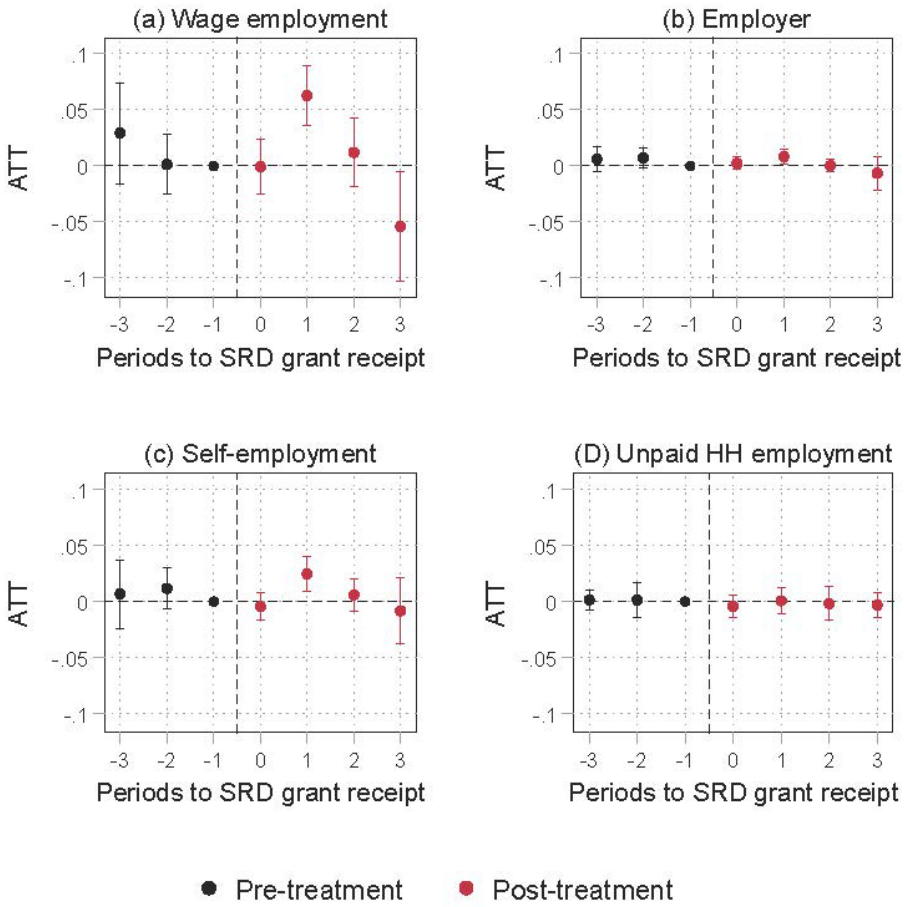
	Pr(Wage employment)		Pr(Employer)		Pr(Self-employment)		Pr(Unpaid household employment)	
	Unconditional	Conditional (doubly robust)	Unconditional	Conditional (doubly robust)	Unconditional	Conditional (doubly robust)	Unconditional	Conditional (doubly robust)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	0.026***	0.023***	0.004*	0.003	0.012***	0.009*	-0.002	-0.002
	(0.009)	(0.009)	(0.002)	(0.002)	(0.004)	(0.005)	(0.003)	(0.003)
First treatment group-specific effects								
Mean	0.034***	0.030***	0.005**	0.005**	0.014***	0.011**	-0.002	-0.002
	(0.008)	(0.008)	(0.002)	(0.002)	(0.005)	(0.005)	(0.003)	(0.003)
2020Q2	-0.028	0.031*	-0.004	-0.003	-0.003	-0.004	-0.002	-0.003
	(0.019)	(0.024)	(0.005)	(0.005)	(0.009)	(0.009)	(0.003)	(0.004)
2020Q3	0.046***	0.043***	0.005**	0.004*	0.018***	0.013**	-0.001	-0.001
	(0.011)	(0.011)	(0.003)	(0.002)	(0.006)	(0.006)	(0.005)	(0.005)
2020Q4	0.058***	0.055***	0.010**	0.010**	0.018*	0.016	-0.002	-0.002
	(0.013)	(0.014)	(0.004)	(0.004)	(0.011)	(0.012)	(0.004)	(0.004)
Observations	52 161	51 828	52 161	51 828	52 161	51 828	52 161	51 828

Source: QLFS 2020Q1 - 2020Q4 and 2021Q1 (StatsSA, 2020a; 2020b; 2020c; 2020d; 2021b).
Authors' own calculations.

Notes: This table presents Difference-in-Differences (DiD) estimates on the effect of receipt of the COVID-19 SRD grant on the probability of job search and the probability of trying to start a business, using the Callaway and Sant'Anna (2021) DiD estimator. The bottom panel presents the aggregated ATT's across all periods for each first-treatment cohort. All models control for individual and time fixed effects. Observations never treated and those not yet treated at the time of treatment used as the control group. Sampling weights employed and standard errors, presented in parentheses, are clustered at the panel (individual) level and are estimated using a multiplicative wild bootstrap procedure (mammen approach) with 1 000 replications. *** p<0.01; ** p<0.05; * p<0.10.

In Figure 5, as before, we analyse the heterogeneity of these estimated effects by employment type by duration of grant receipt, through the use of the event study aggregation. We again find no evidence of an “on impact” effect of receipt of the grant for any outcome; however, after one additional quarter of receipt, we observe a rise in effect estimates for the probabilities of wage employment, self-employment, and becoming an employer. Specifically, as shown in panel (a), one additional quarter of receipt increased the probabilities of wage employment by nearly 6.3 percentage points, significant at the 1% level. This while, as shown in panels (c) and (d), effects on the probabilities of self-employment and becoming an employer were both still significant but notably lower in magnitude at 2.5 and 0.8 percentage points, respectively. As before, these effects reduced markedly in magnitude with increased quarters of receipt, with the effect of one full year of receipt on wage employment becoming negative while those on other outcomes remained insignificant. These dynamics closely resemble that of the probability of employment previously observed in Figure 4, which suggests that, even though it is apparent that the overall employment effects were driven by wage employment, effect variation by duration of receipt was largely not a function of employment type. Engaging in an unpaid household business serves as the exception for which, as shown in panel (d), we find no evidence of any significant effect by duration of receipt.

Figure 5. Event study treatment effect estimates of COVID-19 SRD grant receipt, by employment type



Source: QLFS 2020Q1 - 2020Q4 and 2021Q1 (StatsSA, 2020a; 2020b; 2020c; 2020d; 2021b). Authors' own calculations.

Notes: This figure presents conditional event study estimates of the effect of COVID-19 SRD grant receipt on employment probabilities by employment type. All models are estimated using the Callaway and Sant'Anna (2021) DiD estimator which additionally incorporate Sant'Anna and Zhao's (2020) doubly robust (DR) estimand. ATT's are estimated for each period relative to the period first treated, across all first-treatment cohorts. Here, 'periods to COVID-19 SRD grant receipt' indexes the length of exposure to grant receipt. Observations never treated and those not yet treated at the time of treatment used as the control group. Sampling weights employed and standard errors are clustered at the panel (individual) level and are estimated using a multiplicative wild bootstrap procedure (mammen approach) with 1 000 replications. Capped spikes represent 95% confidence intervals.

5.1.2. Effect heterogeneity by employment formality

Table 6 presents the aggregated treatment effect estimates by sectoral formality of employment. Overall, we find that the highly statistically significant, positive effect on the probability of employment observed earlier was driven by a positive effect on the probability of formal sector employment. As shown in column (2), we estimate that receipt of the grant increased the average formal sector employment probability by 2.2 percentage points, significant at the 1% level. As before, we observe that this effect appears driven by individuals who first received the grant towards the end of 2020. The estimates obtained from the equivalent unconditional model, as shown in column (1), are not statistically significantly different from their conditional counterparts. We do not find any strong evidence of any effect on the probability of informal sector employment. In both columns (3) and (4), the effect estimates are close to zero in magnitude and are statistically insignificant. However, we do observe a positive and significant effect of 3.1 percentage points among individuals who first received the grant in the last quarter of 2020. Despite the significance of this latter estimate, its magnitude is 40% lower than the formal sector equivalent in column (2).

Table 6. Aggregated average treatment effect estimates of COVID-19 SRD grant receipt, by employment formality

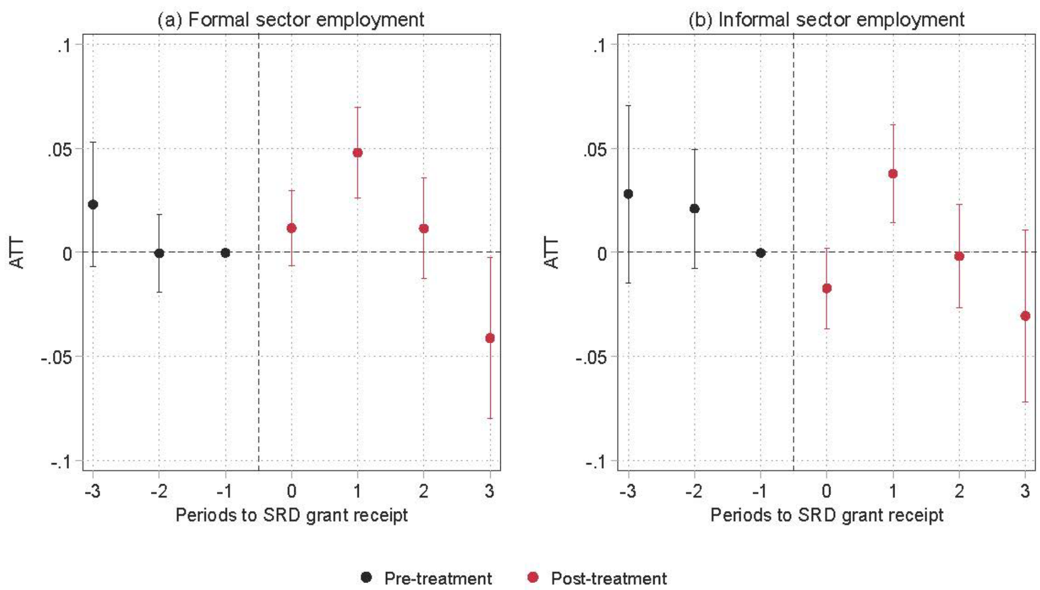
	Pr(Formal sector employment)		**Pr(Informal sector employment)**	
	Unconditional	Conditional (doubly-robust)	Unconditional	Conditional (doubly-robust)
	(1)	(2)	(3)	(4)
ATT	0.024***	0.022***	0.010	0.007
	(0.006)	(0.007)	(0.007)	(0.007)
First treatment group-specific effects				
Mean	0.030***	0.028***	0.015**	0.011
	(0.006)	(0.006)	(0.007)	(0.008)
2020Q2	-0.018	-0.019	-0.019	-0.020
	(0.015)	(0.015)	(0.013)	(0.014)
2020Q3	0.040***	0.035***	0.017	0.014
	(0.009)	(0.009)	(0.011)	(0.011)
2020Q4	0.049***	0.051***	0.037***	0.031**
	(0.010)	(0.011)	(0.013)	(0.013)
Observations	52 161	51 828	52 161	51 828

Source: QLFS 2020Q1 - 2020Q4 and 2021Q1 (StatsSA, 2020a; 2020b; 2020c; 2020d; 2021b). Authors' own calculations.

Notes: This table presents Difference-in-Differences (DiD) estimates of the effect of receipt of the COVID-19 SRD grant on the probabilities of formal or informal sector employment. All models are estimated using the Callaway and Sant'Anna (2021) DiD estimator, while conditional models are estimated by additionally incorporating Sant'Anna and Zhao's (2020) doubly robust (DR) estimand. The bottom panel presents the aggregated ATT's across all periods for each first-treatment cohort. Observations never treated and those not yet treated at the time of treatment used as the control group. Sampling weights employed and standard errors, presented in parentheses, are clustered at the panel (individual) level and are estimated using a multiplicative wild bootstrap procedure (mammen approach) with 1 000 replications. ATT = average treatment effect on the treated. Only time-varying controls included: age, marital status, a binary urban indicator, highest level of education, and a binary indicator of whether an individual was currently attending an educational institution. *** p<0.01; ** p<0.05; * p<0.10.

As before, we analyse the heterogeneity of these estimated effects by employment sectoral formality by duration of grant receipt through the use of the event study aggregation in Figure 6. We again find no statistically significant evidence of an "on impact" effect of receipt of the grant for any of the two outcomes; however, it is notable that the sign of the formal sector estimate is positive while that of the informal sector estimate is negative. The latter estimate is marginally significant at the 10% level. Apart from this difference, the dynamics for both outcomes largely resemble one another and that of overall employment probabilities. After one additional quarter of receipt, the effect estimates rise to approximately 4.8 and 3.8 percentage points for formal and informal sector employment probabilities, respectively. Thereafter, with additional periods of receipt both effect estimates reduce to become negative in magnitude and marginally significant at the 10% level after one full year of receipt. These dynamics closely resemble that of the probability of employment previously observed in Figure 4. This suggests that, as before, effect variation by duration of receipt was largely not a function of employment formality.

Figure 6. Event study treatment effect estimates of COVID-19 SRD grant receipt, by employment sectoral formality



Source: QLFS 2020Q1 - 2020Q4 and 2021Q1 (StatsSA, 2020a; 2020b; 2020c; 2020d; 2021b). Authors' own calculations.

Notes: This figure presents conditional event study estimates of the effect of COVID-19 SRD grant receipt on employment probabilities by employment formality. All models are estimated using the Callaway and Sant'Anna (2021) DiD estimator which additionally incorporate Sant'Anna and Zhao's (2020) doubly robust (DR) estimand. ATT's are estimated for each period relative to the period first treated, across all first-treatment cohorts. Here, 'periods to COVID-19 SRD grant receipt' indexes the length of exposure to grant receipt. Observations never treated and those not yet treated at the time of treatment used as the control group. Sampling weights employed and standard errors are clustered at the panel (individual) level and are estimated using a multiplicative wild bootstrap procedure (mammen approach) with 1.000 replications. Capped spikes represent 95% confidence intervals.

6. Robustness tests

In our analysis above, our estimates of effects at time t make use of a control group who comprise the relevant individuals who were not-yet treated by t as well as those who were never treated in the panel. However, this approach then invertedly includes individuals who may anticipate receiving the grant in later periods, which may introduce of source of bias in our causal estimates. Fortunately, Callaway and Sant'Anna's (2021) estimator allows one to control for treatment anticipation behaviour through a control group restriction. As such, as a robustness check we re-estimate our overall and sub-group models but restrict the control group to only include individuals who were never treated (that is, who never received the grant in the panel). We report the aggregated treatment effect estimates for all outcomes in Table 7. We find that our main estimates above are very consistent in terms of sign, magnitude, and statistical significance to those under a more restricted control group. For every outcome considered, the largest magnitude of the difference between the estimates is 0.1 percentage points. The same results can be reported for the difference in standard errors, resulting in identical levels of statistical significance with one exception. Although the estimated effect of receipt on self-employment is constant at 0.9 percentage points, the marginally lower standard error using the never-treated control group results in a more statistically significant estimate – now at the 5% level. Despite this difference, our main results are not qualitatively affected. Overall, these estimates suggest that our main results are not sensitive to the inclusion of not-yet treated individuals in our control group.

Table 7. Aggregated average treatment effect estimates of COVID-19 SRD grant receipt, restricting control group to the never-treated only

	Primary outcomes			By employment type				By employment formality	
	Pr(Job search)	Pr(Try to start business)	Pr(Employment)	Pr(Wage employment)	Pr(Employer)	Pr(Self-employment)	Pr(Unpaid household employment)	Pr(Formal sector employment)	Pr(Informal sector employment)
ATT (control = NT + NYT)	0.009	0.012*	0.029***	0.023***	0.003	0.009*	-0.002	0.022***	0.007
	(0.017)	(0.007)	(0.010)	(0.009)	(0.002)	(0.005)	(0.003)	(0.007)	(0.007)
ATT (control = NT)	0.010	0.012*	0.028***	0.022***	0.003	0.009**	-0.002	0.021***	0.006
	(0.016)	(0.007)	(0.009)	(0.009)	(0.002)	(0.004)	(0.003)	(0.007)	(0.007)

Source: QLFS 2020Q1 - 2020Q4 and 2021Q1 (StatsSA, 2020a; 2020b; 2020c; 2020d; 2021b).
Authors' own calculations.

Notes: This table presents Difference-in-Differences (DiD) estimates of the effect of receipt of the COVID-19 SRD grant on all outcomes of interest, by varying control group composition. All models are estimated using the Callaway and Sant'Anna (2021) DiD estimator which additionally incorporate Sant'Anna and Zhao's (2020) doubly robust (DR) estimand. Control group either includes observations never treated (NT) only, or observations NT as well as those not-yet treated (NYT) at the time of treatment. Sampling weights employed and standard errors, presented in parentheses, are clustered at the panel (individual) level and are estimated using a multiplicative wild bootstrap procedure (mammen approach) with 1 000 replications. ATT = average treatment effect on the treated. Only time-varying controls included: age, marital status, a binary urban indicator, highest level of education, and a binary indicator of whether an individual was currently attending an educational institution.
*** p<0.01; ** p<0.05; * p<0.10.

As an additional robustness test, we explore the sensitivity of our results to our choice of estimand to incorporate covariates into the modelling to obtain more comparable groups of recipients and non-recipients. As described in Section 4.2, our preferred approach adopts Sant'Anna and Zhao's (2020) doubly robust (DR) estimand which relies on less stringent modelling conditions and additional robustness against model misspecification relative to the alternative outcome regression (OR) and inverse probability weighting (IPW) estimands. Here we re-estimate all conditional models using these other two alternative estimands and present the results in Table 8. Recall that the OR approach requires a correctly specified model of the outcome evolution of the control group, while the IPW approach requires a correctly specified model of the propensity score of individual i belonging in group g and that they are either in group g or an appropriate comparison group. We find that, for all outcomes, our effect estimates are largely insensitive to the choice of estimand. Although qualitatively our main results hold, in some instances the magnitude and statistical significance of a given estimate increases. For instance, the coefficient for job search effects rises to 0.016 and 0.022 using the OR and IPW estimands, respectively; however, in all cases it remains statistically insignificant. Similarly, the coefficient for trying to start a business rises to 0.014 and 0.015 using the OR and IPW estimands respectively and becomes more significant in the latter case, as is the case for self-employment. For all other outcomes, the magnitudes of the coefficients and levels of statistical significance are relatively constant. Overall, these results suggest that our main estimates are not sensitive to the choice of estimand.

Table 8. Aggregated average treatment effect estimates of COVID-19 SRD grant receipt, using alternative estimators

	Main estimator	Alternative estimator	
	Doubly robust	Outcome regression adjustment (OLS)	IPW with stabilised weights
	(1)	(2)	(3)
Pr(Job search)	0.009	0.016	0.022
	(0.017)	(0.017)	(0.017)
Pr(Try to start business)	0.012*	0.014*	0.015**
	(0.007)	(0.007)	(0.007)
Pr(Employment)	0.029***	0.030***	0.031***
	(0.010)	(0.010)	(0.010)
By employment type			
Pr(Wage employment)	0.023***	0.023***	0.024***
	(0.009)	(0.009)	(0.009)
Pr(Employer)	0.003	0.003	0.003
	(0.002)	(0.002)	(0.002)
Pr(Self-employment)	0.009*	0.010**	0.010**
	(0.005)	(0.004)	(0.004)
Pr(Unpaid household employment)	-0.002	-0.002	-0.002
	(0.003)	(0.003)	(0.003)
By employment formality			
Pr(Formal sector employment)	0.022***	0.022***	0.022***
	(0.007)	(0.007)	(0.007)
Pr(Informal sector employment)	0.007	0.008	0.009
	(0.007)	(0.007)	(0.007)

Source: QLFS 2020Q1 – 2020Q4 and 2021Q1 (StatsSA, 2020a; 2020b; 2020c; 2020d; 2021b). Authors' own calculations.

Notes: This table presents Difference-in-Differences (DiD) estimates of the effect of receipt of the COVID-19 SRD grant on all outcomes of interest, by alternative estimand. All models are estimated using the Callaway and Sant'Anna (2021) conditional DiD estimator and vary only by how covariates are incorporated into the modelling. Control group includes observations never treated as well as those not-yet treated at the time of treatment. Sampling weights employed and standard errors, presented in parentheses, are clustered at the panel (individual) level and are estimated using a multiplicative wild bootstrap procedure (mammen approach) with 1 000 replications. Only time-varying controls included for each estimand: age, marital status, a binary urban indicator, highest level of education, and a binary indicator of whether an individual was currently attending an educational institution. *** p<0.01; ** p<0.05; * p<0.10.

7. Conclusion

The South African economy has been characterised by extreme levels of unemployment over the last few decades, while concurrently there has been a dearth of state-provided income support to this group. In this light, the government's introduction of the COVID-19 Social Relief of Distress (SRD) grant in response to the pandemic played an important role in addressing this hole in the safety net and served as the country's first cash transfer to make explicit use of a labour market eligibility criterion. It is therefore plausible that the transfer may have had markedly different labour market effects relative to pre-existing transfers, and hence may have been a means of both income support and labour market recovery. However, at the time of writing, no causal evidence exists on the effects of the grant on any outcome. In this paper, we make use of representative and panel labour force data to estimate the contemporaneous and cumulative causal effects of receipt of the COVID-19 SRD grant on several labour market outcomes including job search, starting a business, and employment. We further consider effect heterogeneity for the latter outcome by employment type and sectoral formality. To do so, we exploit a credible proxy variable of receipt in the data and adopt Callaway and Sant'Anna's (2021) doubly robust, semi-parametric, and staggered Difference-in-Differences (DiD) estimator which is robust to multiple time periods, treatment timing heterogeneity, and model misspecification. To the authors' knowledge, this study provides the first causal estimates of the effects of receipt of the grant.

We find clear evidence that the COVID-19 SRD grant had a small yet significant overall impact across the four periods of disbursement on key labour market outcomes. Our preferred model thus suggests that receipt of the grant increased average employment probabilities by approximately 3 percentage points. Our heterogeneity analysis points to this effect being largely driven by a positive effect on wage employment and formal sector employment, although we do also estimate positive but much smaller effects on the probabilities of self-employment, becoming an employer, and informal sector employment. It is crucial to note however, that employment effects all vary by duration of receipt - with larger effects estimated in the short-term reducing then steadily to zero with additional exposure to the grant up to one year. While we do not find strong evidence of any effects on job search, we estimate small but only marginally significant effects on the probability of trying to start a business. For most outcomes, effects are consistently larger among individuals who first received the grant towards the end of 2020, which may speak to the non-negligible role of the efficiency of the grant system in influencing individual outcomes. These results are strongly robust to alternative control group compositions and alternative estimands which seek to address the validity of the design.

Overall, our findings suggest that the COVID-19 SRD grant has performed a multi-purpose role in providing income relief to a large group of vulnerable, previously unreached individuals while also enabling a path towards more favourable labour market outcomes. This study is therefore in line with the literature on the positive labour supply effects of unconditional cash transfers on labour supply in South Africa (Samson, 2004; Ardington et al., 2009; Eyal and Woolard, 2011b; Tondini, 2017) in contrast to the literature on negative effects (Bertrand et al., 2003; Mutasa, 2012; d'Agostino and Scarlato, 2016; Abel, 2019). It can also be argued then that the transfer acted as both a passive *and* active labour market policy, despite not being explicitly designed to do so. As discussed by McKenzie (2017), while traditional active labour market policies have tended to yield mild if any effects, there is significant scope for improvements for better alternatives. While other factors also need to be considered, our results suggest that cash transfers may be one such avenue to explore possibly in combination with other more traditional active labour market policy interventions.

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Appendix

Table A1. Average treatment effect estimates of COVID-19 SRD grant, using the 'naïve' difference-in-differences estimator

	Pr(Job search)		**Pr(Try to start business)**		**Pr(Employment)**	
	(1)	(2)	(3)	(4)	(5)	(6)
ATT	0.153***	0.085***	0.038***	0.034***	0.015**	0.006
	(0.012)	(0.012)	(0.007)	(0.007)	(0.007)	(0.007)
Controls?	N	Y	N	Y	N	Y
Observations	52 234	51 901	52 234	51 901	52 234	51 901

Source: QLFS 2020Q1 – 2020Q4 and 2021Q1 (StatsSA, 2020a; 2020b; 2020c; 2020d; 2021b). Authors' own calculations.

Notes: This table presents Difference-in-Differences (DiD) estimates of the effect of receipt of the COVID-19 SRD grant on the three main outcomes of interest. All models are estimated using the canonical DiD specification which does not account for multiple time periods and treatment timing heterogeneity. Sampling weights employed and standard errors, presented in parentheses, are clustered at the panel (individual) level. Only time-varying controls included: age, marital status, a binary urban indicator, highest level of education, and a binary indicator of whether an individual was currently attending an educational institution. *** p<0.01; ** p<0.05; * p<0.10.

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