The Labour Market Effects of Cash Transfers to the Unemployed:
Evidence from South Africa

By Haroon Bhorat and Timothy Köhler

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Abstract

This paper considers the labour market effects of an unconditional cash transfer targeted at the unemployed in a context of extreme unemployment. Using a staggered, heterogeneity robust difference-in-differences design applied to panel labour force survey data, we estimate the contemporaneous and dynamic effects of a new transfer introduced in South Africa, the Social Relief of Distress grant, the first labour market-linked transfer in the country's history. We find that, on aggregate, receipt has positive effects on the probabilities of job search, trying to start a business, and employment. The latter effects are driven by effects on wage and informal sector employment. We show that employment effects are positive for the unemployed who are either actively searching for work or trying to start a business, as well as for those who are not, but they are substantially larger for the former. This indicates that the transfer both encourages and improves the efficiency of labour market activity by addressing labour market constraints but highlights the importance of active labour market engagement for improving employment prospects through the transfer. However, these employment effects are only evident in the short-term and quickly become and remain null in the longer-term. These results suggest that cash transfers can help reduce labour market constraints but such gains need not translate into better longer-term employment prospects in high-unemployment contexts.

Keywords:

Cash transfers; Labour market; Unemployment; South Africa; COVID-19; Social Relief of Distress grant.

JEL classification:

D04, D31, C54, H53, J48, J68.

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1 Introduction

Cash transfers, both unconditional and conditional, are a key component of social protection systems around the world. They served as the most-widely used intervention during the COVID-19 pandemic, having reached an estimated 1.4 billion people globally (Gentilini et al., 2022; Gentilini, 2022). Most of these programmes introduced during this period were temporary, although notably many have persisted beyond the pandemic. While poverty alleviation is often the main goal, a well-established empirical literature provides evidence of largely positive effects on a range of other welfare outcomes such as education, health, and early childhood development to name a few (for instance, see reviews by Leroy et al., 2009; Ranganathan & Lagarde, 2012; Baird et al., 2014; Honorati et al., 2015; García & Saavedra, 2017; Bastagli et al., 2019; Millán et al., 2019).

Despite these benefits, many policymakers and members of the public express concern that these programmes may create disincentives to work and a culture of dependency. In fact, these beliefs are correlated with less extensive and generous social assistance programmes (Banerjee et al., 2017). The underlying economic theory is, however, ambiguous. The standard model of labour supply predicts that when an individual receives an increase in non-labour income, they trade off the gain (more income) from working more against the cost (less leisure), and if leisure is regarded as a normal good, an increase in non-labour income causes them to demand more leisure and less work.[1] However, this model does not account for the presence of various market failures which the provision of cash transfers may help overcome. For instance, liquidity or credit constraints may prohibit an individual's ability to search for work or to invest in capital to start or expand a business (Rose, 2001; Gertler et al., 2012; Ervin et al., 2017; Baird et al., 2018; Daidone et al., 2019), while insurance constraints may reduce their willingness to engage in productive but risky activities with high potential rewards because of the risk of failure (Hennessy, 1998; Serra et al., 2006; Baird et al., 2018; Bastagli et al., 2019). The interplay of these channels means that the theoretical effect of cash transfers on work is ambiguous.

Empirically, the predictions of the standard model are very seldom seen. For the most part, the empirical literature does not support a negative labour supply effect (Banerjee et al., 2017; Baird et al., 2018; Handa et al., 2018), and in several cases positive effects are instead found (Alzúa et al., 2013; Bandiera et al., 2017; Salehi-Isfahani & Mostafavi-Dehzooei, 2018). This suggests that both the sign and magnitude of effects may be driven by two specific factors: Firstly, the details of the programme design, and secondly, the underlying economic conditions (Banerjee et al., 2017; Millán et al., 2019). This paper considers the case in which both of these factors are explicitly characterised by unemployment. Involuntary unemployment is also not accounted for in the standard model, and a large extent of it has ambiguous implications. On the one hand, the marginal utility of leisure may be too low for individuals to reduce their labour supply in response to increases in non-labour income (Salehi-Isfahani & Mostafavi-Dehzooei, 2018). On the other hand, while cash transfers may improve an individual's ability to participate in the labour market, a severe scarcity of jobs means that such gains may not translate into better employment prospects (Banerjee & Sequeira, 2023).

We estimate the labour market effects of a new cash transfer introduced in South Africa in response to the pandemic: the Social Relief of Distress (SRD) grant. Distributed monthly from the end of May 2020, the SRD grant is an unconditional, non-contributory cash transfer of ZAR350 (US$50 in Purchasing Power Parity (PPP) terms.) per person per month, equivalent to just 6 percent of the median monthly wage (Köhler & Bhorat, 2023). While not explicitly designed to affect labour market behaviour, the transfer is targeted at a large group of previously unreached, vulnerable individuals: unemployed working-age adults who, given the country's extreme rate of unemployment (exceeding 30 percent under the narrow definition even prior to the pandemic), comprised over 10 million people or one quarter of the working-age population.[2] The transfer reached about this many recipients at its peak in a progressively-distributed way (Köhler & Bhorat, 2020; Bhorat et al., 2021; Bassier et al., 2022; Turok & Visagie, 2022), and importantly, is distinct in South Africa's social assistance system in that it is the first in the country's history to make explicit use of a labour market eligibility criterion for targeting, and the first intended for working-age adults for their own consumption.[3] It has thus played an important role in addressing a long-lasting gap in the safety net. While the transfer was initially intended to be temporary, it has since experienced several extensions and, at the time of writing, remained in place with no clear decision on whether it would be integrated permanently into the system in either its current or an alternative design. The potential labour market effects of doing so has been hotly contested, with some arguing that doing so may reduce unemployment through increases in job search and economic activity (Orkin et al., 2022), while others express concern that doing so may slow economic growth through a rise in public debt and taxation and, ultimately, increase unemployment (Hollander et al., 2024). To our knowledge, this paper is the first to provide causal evidence of the SRD grant's effects on any outcome.

We adopt a staggered, heterogeneity-robust, and semi-parametric difference-in-differences (DiD) design and apply it on a nationally representative, individual-level household survey dataset conducted during 2020 and 2021. We leverage a unique, temporary panel aspect of the data, a plausible proxy variable to indirectly identify recipients to overcome data limitations, and several sample restrictions to avoid any confounding effects from the introduction of other policies during the same period. We estimate contemporaneous and dynamic effects on three outcomes – the probabilities of job search, trying to start a business, and employment – and additionally analyse effect heterogeneity by employment type and formality. Importantly, we estimate effects during the transfer's first year or 'phase', prior to a change in eligibility criteria.

We find evidence of positive but heterogenous effects of SRD grant receipt on labour market outcomes. On average, receipt increases the probability of job search by 4.4 percentage points, trying to start a business by 4.0 percentage points, and employment by 6.0 percentage points. The employment effects are driven by effects on wage employment and, to a lesser extent, self-employment, which may be explained by the dominance of wage jobs in the country. We further detect positive effects on both formal and informal sector employment, with larger effects on the latter which may be due to relatively low barriers to entry (Mattos & Ogura, 2009; Davies & Thurlow, 2010; Asmal et al., 2024).[4] Our analysis of potential mechanisms suggests that these employment effects are explained by the transfer's effects on labour market constraints at both the extensive (looking for work or trying to start a business at all) and intensive (how one looks for work or starts a business conditional on already doing so) margins. Receipt of the transfer appears to shift a subset of the unemployed who are not actively searching for work or trying to start a business into these activities, but to a greater extent, it improves the efficiency of these activities among those already doing so, both resulting in gains in employment prospects. This suggests that active labour market engagement is a crucial mechanism through which cash transfers may enable recipients to achieve better employment prospects. Together, these mechanisms are consistent with the presence of liquidity and insurance constraints well-documented in the literature (Rose, 2001; Baird et al., 2018; Salehi-Isfahani & Mostafavi-Dehzooei, 2018; Bastagli et al., 2019) as well as qualitative evidence of the transfer's labour market effects

(Plagerson et al., 2023; Venter et al., 2024). We additionally show that effects on all outcomes tend to be larger among individuals who first received the transfer towards the end of 2020 and beginning of 2021, which possibly highlights the roles of greater physical mobility from less stringent pandemic-related restrictions (Köhler et al., 2023) and gains in the efficiency of the State's administration system resulting in more stable and dependable transfers (de Janvry & Sadoulet, 2006; Bastagli et al., 2016; Gennetian et al., 2021).

Importantly, however, we show that the transfer's average employment effects are only evident in the short-term and do not persist with longer periods of receipt. The "on impact" effect is relatively large at 9.6 percentage points; however, it quickly dissipates to become null from two quarters of receipt and remains so for at least one full year of receipt. The dynamics of other outcomes vary but yield qualitatively similar results. This suggests that the short-term labour market gains of the transfer do not translate into sustained longer-term benefits, at least with respect to the transfer's initial design. We argue that this null, longer-term employment effect is likely due to the extent and nature of unemployment in South Africa. Specifically, the combination of a general scarcity of jobs and a largely structural nature of unemployment, specifically with respect to skills mismatch (Banerjee et al., 2008; Pauw et al., 2008) and unusually high barriers to entry into both the formal and informal sectors (Kingdon & Knight, 2004; Banerjee et al., 2008; Davies & Thurlow, 2010; Hausmann et al., 2023; Asmal et al., 2024), means that gains in a recipient's ability to search for work or start a business due to the transfer need not translate into better employment prospects. This is strongly consistent with recent experimental evidence which shows that, while reductions in search costs increase job search, the failure of jobs to materialize immediately in such a high-unemployment context leads job-seekers to look for work closer to home which, however, does not improve their employment odds since nearby jobs are also scarce (Banerjee & Sequeira, 2023). Overall, our results suggest that while the SRD grant in its initial design does address some constraints to job search and starting a business, leading some recipients to gain jobs initially, the existence of other significant constraints means that such gains do not translate into better employment prospects in the longer-term.

Our analysis makes several contributions to existing literatures and holds important policy implications. First, it contributes to the large empirical literature on the labour market effects of cash transfers in low- and middle-income countries (Rose, 2001; Serra et al., 2006; Gertler et al., 2012; Banerjee et al., 2017; Ervin et al., 2017; Salehi-Isfahani & Mostafavi-Dehzooei, 2018; Bastagli et al., 2019). Our findings are consistent with most studies which do not support a negative labour supply effect as predicted by the standard theoretical model. Notably, they primarily provide novel evidence of such effects when a transfer is explicitly targeted at the unemployed in a high-unemployment context, showing that cash transfers can help reduce job search constraints but, in such contexts, such gains need not translate into better employment prospects in the long-term. Hence, our findings may hold relevance in other high-unemployment low- and middle-income countries. Second, our analysis of mechanisms is consistent with the dominant mechanisms – liquidity or credit and insurance constraints – documented in various contexts (Baird et al., 2018). Third, our study contributes to the existing literature on these effects in South Africa in particular (Bertrand et al., 2003; Posel et al., 2006; Williams, 2007; Sienaert, 2008; Ardington et al., 2009; Ranchhod, 2009; Eyal & Woolard, 2011b; Leibbrandt et al., 2013; d'Agostino & Scarlato, 2012; Tondini, 2017; Abel, 2019), and holds important policy implications for the future of the transfer in the country. In particular, our finding of a short-term, positive employment effect that does not persist into the longer-term suggests that the SRD grant may not be an appropriate policy tool to reduce the country's extreme level of unemployment, at least in its initial design. While this does not negate the transfer's important effects on other outcomes such as poverty and hunger, it suggests that if improving the functioning of the labour market is the goal, alternative or additional approaches are required.

The rest of this paper is organised as follows. Section 2 provides an overview of South Africa's contemporary social assistance system, the introduction and evolution of the SRD grant, and a review of existing empirical studies of its effects on welfare outcomes. In Section 3 we describe our data and identification strategy, respectively. The results are presented in Section 4 followed by several robustness tests in Section 5. Section 6 concludes.

2 Literature review

2.1 South Africa's contemporary social assistance system

South Africa spends a relatively large amount on social assistance given its level of economic development. In recent years, social assistance expenditure has stood at approximately 3.3 percent of Gross Domestic Product (GDP), more than double the 1.4 percent spent by the median upper-middle income country (World Bank, 2021). Most of this expenditure – over 90 percent – is used to distribute tax-financed, unconditional, non-contributory, and means-tested cash transfers. The remainder is allocated to public works programs and school feeding schemes. These transfers – referred to as 'social grants' in South Africa[5] – are available to citizens as well as permanent residents and refugees and, prior to the pandemic and introduction of the SRD grant, primarily targeted vulnerable children, the elderly, and the disabled. Specifically, seven transfers were available: the Child Support Grant (CSG), Older Person's Grant (OPG)[6], War Veterans' Grant (WVG), Disability Grant (DG), Foster Care Grant (FCG), Care Dependency Grant (CDG), and the Grant-in-Aid (GIA).[7] All transfers are means-tested with the exception of the FCG. The CSG represents the largest transfer in the system in terms of the number of transfers distributed, accounting for over half (South African Social Security Agency, 2023), which is largely due to gradual increases in the transfer's age eligibility threshold and a less stringent means test relative to other transfers. The monthly monetary values of transfers vary considerably, from ZAR510 (US$73 PPP) per person for the CSG to ZAR2,090 (US$300 PPP) for the OPG and DG as of 2023.[8]

These cash transfers serve as an important source of household income for a large share of the South African population. As of May 2023, 26 million transfers were paid to 19 million recipients (South African Social Security Agency, 2023).[9][10] In addition to direct receipt, many individuals also indirectly benefit. In 2022, more than two in every three (68 percent) individuals in the country co-resided in the same household as a transfer recipient.[11] In addition to high coverage, it is widely documented that these transfers are relatively well-targeted to the poor, largely due to the use of means testing as a targeting device (Woolard et al., 2011; Van der Berg, 2014a,b; World Bank, 2021; Gronbach et al., 2022). A large empirical literature documents how this wide and progressive coverage has translated into positive, causal effects on a wide array of welfare outcomes including life satisfaction (Alloush & Wu, 2023), physical and mental health (Duflo, 2003; Case & Jensen, 2004; Eyal & Burns, 2019; Ohrnberger et al., 2020a,b), school attendance (Williams, 2007), food expenditure (d'Agostino et al., 2018), and hunger (Williams, 2007) to name a few. Descriptive evidence also suggests that these transfers have reduced both income poverty and, to a lesser extent, income inequality (Case & Deaton, 2001; Leibbrandt et al., 2010; Van der Berg et al., 2010; Woolard et al., 2011; Van der Berg, 2014a). These results have led to the social assistance system becoming regarded as one of South Africa's most important policy successes in the post-apartheid period.

Evidence on the labour market effects of these transfers are, however, mixed. While none are explicitly designed to affect labour market behaviour, the empirical literature provides evidence of both positive and negative effects on recipients and co-resident household members. Considering the OPG, which is targeted at the elderly, Bertrand et al. (2003) estimate a reduction in the working hours and labour force participation of prime-aged household members. However, their study has been criticised for their exclusion of non-household members from their sample (Leibbrandt et al., 2013). After retaining these members, Ardington et al. (2009) find that OPG receipt has a positive employment effect among prime-aged adults, which they show is due to labour migration. This is consistent with the findings of both Sienaert (2008) and Posel et al. (2006), and also provides evidence of intra-household income redistribution (Leibbrandt et al., 2013). Ranchhod (2009) also finds evidence of positive effects on employment among resident middle-aged and older adults. In contrast, Abel (2019) finds that the presence of OPG recipients reduces employment of both previously employed and unemployed prime-aged adults. Considering the CSG, which is received by caregivers on behalf of their eligible child(ren), Eyal & Woolard (2011b) estimate a positive effect on both the labour force participation and employment of recipient mothers. Williams (2007) also estimates a positive effect on mother's broad labour market participation, while Tondini (2017) finds a positive effect on formal employment in particular. However, d'Agostino & Scarlato (2012) estimates a negative effect of CSG receipt on the employment of co-residing household members, particularly women. Regarding the DG - the only transfer targeted at working-age adults for their own consumption until the introduction of the SRD grant - Mitra (2010) finds no evidence of any effect on labour market behaviour, while Mitra (2009) and Mutasa (2012) estimate negative effects on labour force participation. The former's identification strategy, however, does not allow them to arrive at a definitive conclusion.

2.2 The introduction and evolution of the Social Relief of Distress grant

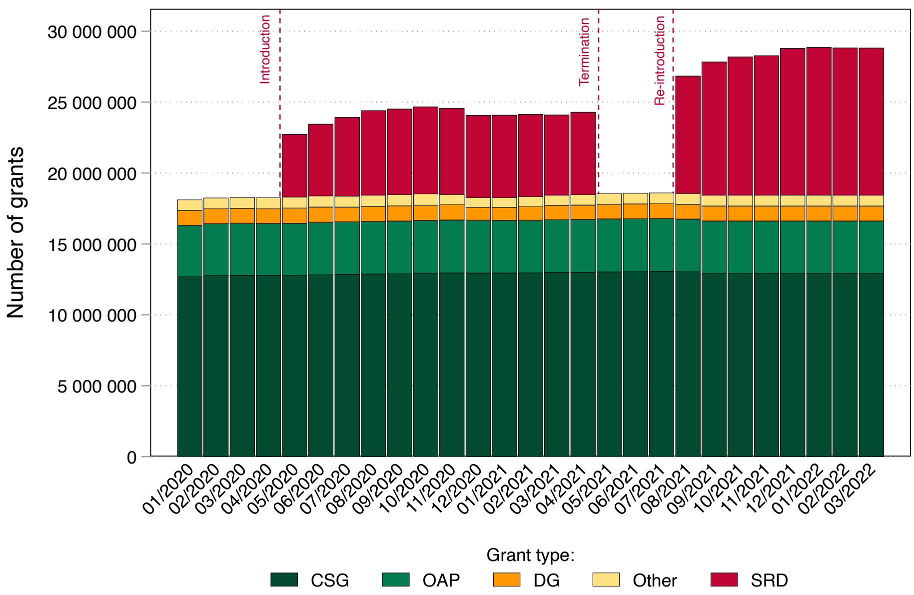
Following the first confirmed COVID-19 case in the country on 5 March 2020, the South African government declared a National State of Disaster on 15 March 2020 followed by the implementation of a nationwide lockdown or 'stay-at-home' order on 27 March 2020. This initial lockdown lasted until the end of April 2020 and was relatively stringent by international standards (Bhorat et al., 2020; Gustaffson, 2020), making no allowances for any non-essential activity outside the home. All schools were closed, public gatherings were prohibited, domestic and international travel were banned, a curfew was enforced, and sector-specific restrictions meant that only workers in occupations deemed 'essential' for economic function and pandemic response were permitted to work at their usual workplace. Additionally, the sale of alcohol and tobacco products was forbidden, with the latter regulation making the country one of only three in the world to do so (Filby et al., 2022). During this period, GDP and employment fell by 6 and 14 percent year-on-year, respectively, with the latter disproportionately affecting those already in precarious labour market positions such as the youth, women, those with lower levels of education, and the informally-employed to name a few (Casale & Posel, 2021; Casale & Shepherd, 2022; Ranchhod & Daniels, 2021; Kohler et al., 2022; Rogan & Skinner, 2022; Shifa et al., 2022; Bassier et al., 2023; Köhler et al., 2023; Yu et al., 2023). Thereafter, the government adopted a five-level risk-adjusted strategy which implemented national lockdown regulations according to the severity of contagion.[12] In April 2022, the National State of Disaster was repealed followed by all remaining pandemic-related restrictions in June 2022. By May 2023 when the World Health Organisation announced that it no longer considers COVID-19 to be a global health emergency, South Africa had reported over 4 million confirmed cases and 100,000 deaths (World Health Organisation, 2023). However, the estimated true number of the latter is 2.5 to 3 times larger (Bradshaw et al., 2022), consistent with global estimates (Msemburi et al., 2023).

In response to the pandemic and similar to the international context, the government introduced a broad economic policy package to provide largely cash-based relief to both firms and households. Initially amounting to 10 percent of GDP, this package primarily comprised tax relief measures and a combination of existing and new social protection and labour market programmes. Support to households largely leveraged off the country's pre-existing social assistance infrastructure and included an expansion at both the intensive and extensive margins. Announced on 21 April 2020, from May to October 2020 the value of every[13] existing transfer was increased, and a new transfer was introduced: the SRD grant of ZAR350 (US$50 PPP) per person per month, equivalent to roughly 6 percent of the median monthly wage in the labour market (Köhler & Bhorat, 2023). The SRD grant holds particular relevance in the labour market as it is the first in the country's history to make explicit use of a labour market eligibility criterion and target a large group of previously unreached individuals - unemployed adults - representing 11 million people using the broad definition, or one quarter of the working-age population[14]).[15]

Specifically, individuals were eligible for the transfer if they were between 18 and 59 years old, unemployed, and were neither receiving nor eligible to receive any other cash transfer, unemployment insurance benefits, or other forms of government support or income, and were not residing in a government-funded or subsidised institution. All applications for the transfer were done electronically through one of multiple platforms[16] and payments were made into recipient bank accounts, and for the unbanked through either mobile money transfers or physically at the South African Post Office and later at certain retail outlets. As noted in the preceding section, prior to the SRD grant the DG was the only transfer which a small subset of working-age adults were eligible to receive for their own consumption. This is despite the persistence of extreme, structural unemployment throughout the post-apartheid period, exceeding 30 percent under the narrow definition even prior to the pandemic.[17] This lack of support for the working-aged unemployed is rooted in the fact that, as in Latin America and elsewhere, the structure of South Africa's social assistance system relies on the assumption that only 'dependent' categories (such as the elderly, disabled, and children) are in need of support, while prime-aged, able-bodied individuals are presumed to be able to support themselves through the labour market (Ferguson, 2015).

Despite initial payment delays owing to the establishment of relevant systems and issues which the South African Social Security Agency (SASSA) - the public entity responsible for the administration of social assistance - experienced in accessing the databases required for applicant verification (Auditor-General of South Africa, 2020), the introduction of the SRD grant resulted in a relatively rapid and substantial expansion of the system's reach. As shown in Figure 1 which presents trends in the distribution of cash transfers by type from just prior to the pandemic to 2022, the SRD grant was distributed from the end of May 2020 and initially brought over four million individuals into the system, growing to six million by the end of 2020 and accounting for one quarter of all cash transfers distributed. This exceeded the growth of the whole system during the decade prior (Baskaran et al., 2020). While the SRD grant was initially only available for six months until October 2020, it was thereafter extended until April 2021. During this period of the transfer's 'first phase', one of its eligibility criteria - that the transfer could not be held concurrently with any other cash transfer - was heavily criticised for unfairly excluding many women. This is because women represented the majority of other cash transfer recipients - 85 percent as of December 2020 (South African Social Security Agency, 2021) primarily as caregivers who received the CSG on behalf of their eligible child(ren) - and the minority of

Figure 1: Distribution of cash transfers in South Africa by type, January 2020 – March 2022



[a] Author's own calculations. Source: South African Social Security Agency (2021, 2022)
[b] Notes: This figure shows the number of cash transfers distributed per month by transfer type. This is not equivalent to the number of recipients given that primary caregivers who receive the CSG receive it on behalf of their eligible child(ren) and some recipients receive multiple transfers simultaneously. Number of Social Relief of Distress (SRD) grants paid for, but not in, a given month shown – there can be 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. Vertical reference lines indicate the timing of the introduction, temporary termination, and re-introduction of the SRD grant.

employed - 43 percent as of the last quarter of 2020 (Casale & Shepherd, 2022).[18] Indeed, only approximately 30 percent of SRD recipients during this period were women (Gronbach et al., 2022).

Subsequently, in July 2021 following a resurgence of COVID-19 infections, corresponding lockdown measures, and a wave of social unrest in parts of the country, the SRD grant was not only re-introduced for a 'second phase' until March 2022 but expanded to allow unemployed CSG recipients to apply. This latter change had a considerable effect on both the level and gender composition of the transfer's recipient population. As shown in Figure 1, by the end of 2021 the transfer reached over 10 million people, with the majority now being women (South African Social Security Agency, 2022). Later, the transfer experienced several additional extensions while its amount remained unchanged in nominal terms.[19] At the time of writing, it remained in place until March 2025 with no clear decision on whether it would be integrated permanently into the system in either its current or an alternative design. The potential labour market effects of doing so has been hotly contested. Some argue that doing so may reduce unemployment through increases in job search and economic activity (Orkin et al., 2022), while others express concern that doing so may slow economic growth through a rise in public debt and taxation and, ultimately, increase unemployment (Hollander et al., 2024).

2.3 Existing empirical evidence of the Social Relief of Distress grant

In this section, we synthesise the existing empirical literature on the coverage, distribution, and (simulated) effects of the SRD grant on welfare. Several studies have used the National Income Dynamics Study - Coronavirus Rapid Mobile Survey (NIDS-CRAM) - a representative, longitudinal survey conducted during the first year of the pandemic in South Africa - to analyse the transfer's between-group distribution over time. Köhler & Bhorat (2020) show that application for and receipt of the transfer was relatively pro-poor, highlighting that for every recipient in the richest quintile of households in June 2020, there were nearly four in the poorest quintile. 90 percent of those in the former group never even applied. Turok & Visagie (2022) also highlight the transfer's progressive distribution, showing that individuals in households in typically poorer areas were significantly more likely to report receipt. Van der berg et al. (2022) highlight a link between the temporary cessation of the transfer in 2021 and a rise in household hunger. Other studies have considered the notably uneven distribution of receipt by gender during the transfer's 'first phase', as previously mentioned (Köhler & Bhorat, 2020; Casale & Posel, 2021; Casale & Shepherd, 2022). On targeting, Bhorat & Köhler (2021) show that the transfer was relatively well-targeted to the unemployed, particularly the chronically unemployed.

Several studies consider the grant's effects on welfare. Bassier et al. (2021) use pre-pandemic nationally representative household survey data from 2017 to simulate how the negative economic shock of the pandemic, through a reduction in earnings and increase in money-metric poverty, can be mitigated through different cash transfer interventions, including a transfer similar to the SRD grant.[20] They show that their SRD-esque transfer partially but significantly mitigated the simulated negative effect of the pandemic on earnings and poverty.[21] Bhorat et al. (2021) conduct a similar exercise using the same data, but comparing outcomes using the realised policy to alternative policy scenarios. Their findings suggest that, while the chosen policy package of the SRD grant combined with a temporary increase in pre-existing transfers is less progressive than a simple but large increase of the CSG alone, it leads to a similar degree of poverty reduction. In this light, both Bhorat et al. (2021) and Bassier et al. (2021) stress one of the transfer's key advantages is its ability to provide support to a large group of vulnerable individuals who otherwise would not have been covered.

Both Bhorat et al. (2021) and Bassier et al. (2021) make use of pre-pandemic data to arrive at their results, but their results pertaining to poverty reduction largely 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) simulate 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 realised policy package, including the SRD grant. 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, while also simulating the poverty-reducing effect of the SRD grant. Their findings suggest that in the absence of the transfer, the headcount poverty rate[22] would have increased by up to 5.2 percentage points between the first quarter of 2020 and the last quarter of 2021, but with the transfer in place, this increase is mitigated to up to 3.4 percentage points. This is consistent with Bhorat & Köhler (2021) who, using the NIDS-CRAM data, simulate that extreme poverty would have been at least 5 percent higher in the transfer's absence.[23] Overall, although these results should be interpreted as descriptive and approximate, together they provide suggestive evidence on the positive welfare effects of the SRD grant.

3 Data and methodology

3.1 Data

We use nationally representative, longitudinal, individual-level household survey data from StatsSA's Quarterly Labour Force Survey (QLFS) collected in 2020 and 2021. Conducted since 2008, the QLFS is a cross-sectional survey with a rotational panel component that contains detailed information on a wide array of demographic and socioeconomic characteristics and labour market activities for individuals aged 15 years and older, and serves as South Africa's official source of labour market statistics. A detailed description of the survey's sampling design is available in Statistics South Africa (2008). Importantly, as experienced by many national statistics offices around the world in response to the pandemic (Betcherman et al., 2020; Daniels et al., 2022; Kugler et al., 2023), at the end of March 2020 StatsSA had to temporarily suspend face-to-face data collection activities and shift to computer-assisted telephone interviewing (CATI) to continue providing labour market statistics (Statistics South Africa, 2020f).[24] To facilitate this, and unlike in previous quarters, the sample that was surveyed in 2020Q1 for which StatsSA had contact numbers for was surveyed again for the remaining quarters of 2020 as well as 2021Q1. The result was that the cross-sectional samples from 2020Q2 to 2021Q1 included 70 to 74 percent of the 2020Q1 sample because not all dwelling units had valid contact numbers (see Table A1 in the appendix).[25] Thereafter, from 2021Q2 the easing of pandemic-related restrictions allowed for the resumption of sample rotation and face-to-face collection of telephone numbers. However, CATI as the survey mode remained in place. Only from 2022Q1 did StatsSA re-introduce face-to-face interviews (Statistics South Africa, 2022b).

The consequence of the above sampling changes is that the QLFS changed from a primarily cross-sectional survey to an unbalanced longitudinal survey from 2020Q1 to 2021Q1 - a unique scenario in the survey's history. This temporary panel aspect of the data has important implications for the scope of research that had previously been infeasible with the data, and is key to our identification strategy outlined in more detail in the section to follow. Additionally, however, the fact that only a select subset of the 2020Q1 sample could be surveyed from 2020Q2 to 2021Q1 raises sample selection concerns. To address this, StatsSA adjusted the calibrated survey weights using bias adjustment factors based on respondents' 2020Q1 data (Statistics South Africa, 2020f). While an explicit review of these weights had not yet been conducted at the time of writing (and would require more information than is available in the public documentation), they do appear to be appropriately computed, as indicated by Köhler et al. (2023) who provide a more comprehensive overview of the pandemic-induced changes to the survey. We employ these sampling weights throughout our analysis.

Our analysis uses the unbalanced panel of individuals observed across the five waves of data from 2020Q1 to 2021Q1. As such and importantly, our analysis is restricted to the transfer's initial design or 'first phase', described in the previous section. To identify the panel sample, we make use of household and person identifiers available 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.[26] We allow for a one-year difference in age within individuals over time in either direction to account for ageing or possible measurement error. We omit all observations that exhibit an inconsistency in any of these characteristics, affecting 7.5 percent (n=4,244) of observations. Furthermore, for identification reasons outlined in the section to follow, we restrict the sample to adults aged 18 to 59 years who did not receive any unemployment benefits or other cash transfers apart from the SRD grant. Table A1 in the appendix presents the varying sample sizes before and after these adjustments by wave. From a pooled cross-sectional sample of about 255,000 observations prior to these adjustments, this procedure results in an unbalanced panel sample of approximately 52,000 observations, comprising 20,536 unique individuals of whom 60 percent are observed at least twice and up to five times. Attrition across the panel is not related to treatment status.[27]

Despite the magnitude of the sample size reduction, these restrictions do not significantly affect the composition of our sample. Table A2 in the appendix presents weighted mean estimates of several observable covariates at baseline (2020Q1) for SRD recipients and non-recipients, identified as per our approach described in the section to follow, before and after the sample restrictions. Before the restrictions and relative to non-recipients, recipients were slightly more likely to be older and African/Black[28] and less likely to be women, married, live in an urban area, and have a tertiary education. After the restrictions, the signs of all covariate-specific between-group differences are unchanged and their magnitudes are largely similar. It is important to note, however, that these between-group differences in levels do not themselves threaten the validity of our identification strategy given that, in a DiD design, groups are not required to have similar baseline means in covariates, but rather these differences should be stable or evolve in 'parallel' from before to after treatment (Daw & Hatfield, 2018; Wing et al., 2018; Roth et al., 2023). This is analogous to the parallel trends assumption, which we investigate later.

3.2 Identification strategy

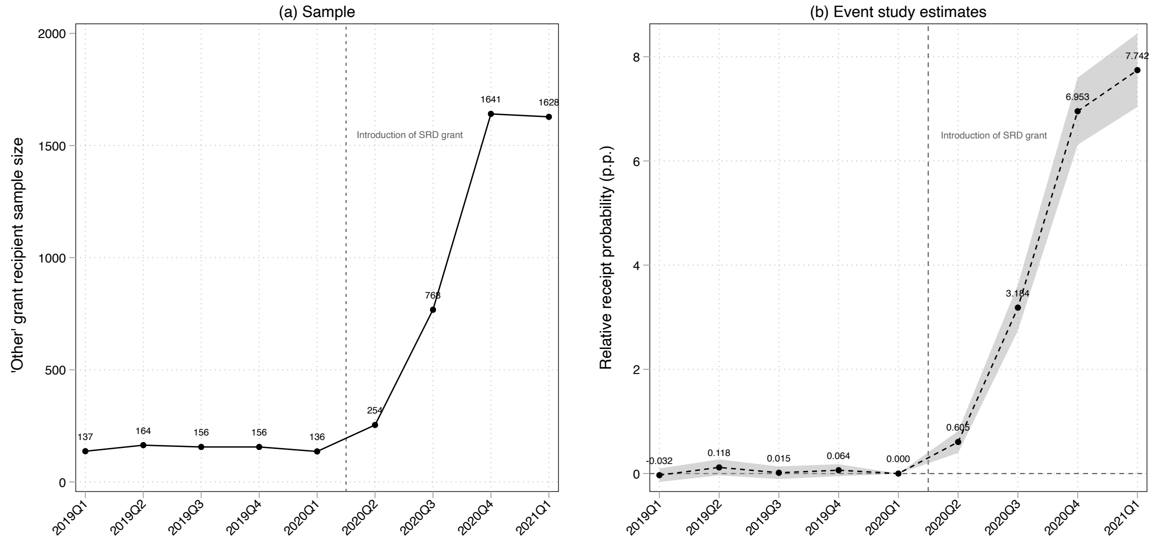
3.2.1 Treatment assignment

We adopt a DiD design to estimate the causal effects of SRD receipt on labour market outcomes. Importantly, a new theoretical and empirical literature on the econometrics of DiD has developed over the last few years which highlights how, in practice, many typical applications do not meet all requirements of the canonical DiD setup. This is primarily due to the presence of heterogenous or 'staggered' treatment timing; that is, when units are treated at different points in time during the treatment period (Athey & Imbens, 2018; de Chaisemartin & d'Haultfoeuille, 2020, 2023, 2024; Borusyak et al., 2021; Callaway & Sant'Anna, 2021; Goodman-Bacon, 2021;

Imai et al., 2021; Sun & Abraham, 2021; Roth et al., 2023). A canonical DiD design with such staggered treatment yields DiD estimates which comprise both 'clean' comparisons (between treated and not-yet- or never-treated units) as well as 'forbidden' comparisons (between units who are both already-treated but in varying periods). The consequence is that such estimates can be severely biased and not correspond with interpretable causal parameters. This is highly relevant in the case of the SRD grant, where individuals receive the transfer at different points in time as opposed to all at once when it was introduced (see Figure A1 in the appendix which illustrates the staggered timing of SRD grant receipt in the period, explained in more detail later). Fortunately, a variety of heterogeneity-robust estimators have been proposed which all strictly only use 'clean' comparisons to avoid these issues. As such, we avoid the canonical setup and adopt a staggered DiD design using Callaway & Sant'Anna's (2021) semi-parametric and doubly robust DiD estimator. By doing so, we can obtain a credible estimate of the average treatment effect on the treated (ATT) after accounting for variation in treatment timing. We describe this estimator in detail later, but first discuss how we identify SRD recipients in the QLFS data.

Unfortunately, the QLFS survey instrument does not include an item which specifically asks about SRD grant receipt.[29] It only includes items which ask about receipt of the CSG, FCG, OAP, and DG, and these items are posed only to the non-employed.[30] However, the survey does include an item which asks about receipt of any of the 'other' cash transfers. By process of elimination, this refers to the WVG, CDG, GIA, and from the end of May 2020, the SRD grant. Figure 2 presents trends in the sample size of respondents who responded affirmatively to this item from 2019Q1 to 2021Q1, accompanied by event study estimates. It is clear that in the year prior to the introduction of the SRD grant, the probability of reporting receipt of an 'other' transfer was unchanged, as indicated by the statistically insignificant event study estimates with magnitudes close to zero. However thereafter, the sample size and probability grew significantly, from $n = 150$ in the average pre-SRD wave to $n = 1,073$ thereafter; equivalent to an eight percentage point higher probability at the end of the series. Given that, individually and collectively, the number of WVG, CDG, and GIA recipients remained constant during both the pre- and post-SRD periods according to administrative data (as shown in Figure 1 in the previous section), this increase in 'other' transfer recipients is arguably due to the variable capturing SRD grant recipients. Hence, we use this variable to indirectly identify these recipients.[31]

Figure 2: 'Other' cash transfer recipients covered in the QLFS, 2019Q1 - 2021Q1



[a] Author's own calculations. Source: QLFS 2019Q1 - 2021Q1 (Statistics South Africa, 2019a,b,c,d, 2020a,b,c,d,e).
[b] Notes: Panel (a) presents the number of observations of 'Other' cash transfer recipients as covered in the data over time. Panel (b) presents event study estimates of the probability of being an 'Other' transfer recipient over time relative to the immediate pre-treatment period (2020Q1). Event study estimates obtained from a linear regression using Ordinary Least Squares of a binary 'Other' transfer indicator on time fixed effects, weighted using sampling weights with standard errors clustered at the primary sampling unit level. By process of elimination, 'Other' cash transfers include the War Veterans' Grant, the Care Dependency Grant, the Grant in Aid, and from the last month of 2020Q2 the SRD grant. Shaded areas represent 95 percent confidence intervals.

This variable may, however, be biased by contamination. That is, it would include SRD recipients in addition to any WVG, CDG, and GIA recipients present in the data. While we are not very concerned about this given the very small collective magnitude of recipients of these transfers,[32] we address this by dropping observations who are eligible for them as far as we can identify in the data in all periods.[33] Thereafter, we still observe a very small amount of such recipients in the data: $n = 148$ observations (or 0.3 percent of all observations) who report receipt of the 'other' transfer in the pre-SRD grant period. To further avoid contamination, we drop this subset of individuals including any subsequent observations of them in the panel from the study sample.[34] Furthermore, recall that individuals were only eligible for the SRD grant if they were between 18 and 59 years old, unemployed, and were not receiving any other form of government support (i.e. any other cash transfer or unemployment insurance benefits). Additionally and as discussed in the previous section, at the same time as the introduction of the SRD grant the values of all other pre-existing transfers were increased for six months. To avoid possibly confounding our estimated effects, we restrict our sample to non-employed[35] individuals aged 18 to 59 years who were not receiving any alternative transfer or unemployment insurance benefits. This ensures that our results are not driven by the aforementioned change to pre-existing transfers. Overall then, our approach compares the temporal outcomes of (i) non-employed adults aged 18 to 59 years who neither receive unemployment insurance benefits nor any cash transfer (comprising $n = 43,444$ observations who never report receipt in the treatment period) versus (ii) non-employed adults aged 18 to 59 years who neither receive unemployment insurance benefits nor any cash transfer with the exception of the 'other' transfer which captures the SRD grant (comprising $n = 8,442$ observations who ever report receipt in the treatment period).

As previously noted, differences in the composition of recipients versus non-recipients within a period does not necessarily threaten the validity of our identification strategy, but instead that any differences should be stable or evolve in 'parallel' from before to after the introduction of the transfer. To examine this, we estimate the mean levels of covariates by receipt status and period and the corresponding between-group differences both within and between periods. These estimates are presented in Table 1. This is equivalent to a placebo falsification test where the DiD model is estimated separately on covariates which theoretically should not be affected by transfer receipt. We find that prior to the transfer's introduction and relative to non-recipients, recipients are of a statistically similar age but are significantly more likely to be self-reported African/Black and male - which is in line with analyses which uses administrative data and alternative survey data (Casale & Shepherd, 2022; Gronbach et al., 2022) - and less likely to be married, reside in an urban area, and have a tertiary-level education. All differences are significant at the 1 percent level. Following the transfer's introduction, the magnitude, sign, and statistical significance of all differences remained similar with one exception: Recipients were significantly older than non-recipients on average, however only marginally so (about half a year). It is therefore unsurprising that the between-period differences for all covariates, apart from age, are all close to zero in magnitude and are statistically insignificant, as shown in column (7).[36] These estimates are in support of the parallel trends assumption and hence the validity of our DiD design. In our results section to follow, we also show event study estimates which provide further support. Although the significant estimate on age may be of concern, in the next section we describe our adoption of Callaway & Sant'Anna's (2021) estimator which accounts for such inter-group differences.

Table 1: Covariate balance by SRD receipt status and period

	Pre-SRD period			Post-SRD period			DiD
	Never received SRD (1)	Ever received SRD (2)	Diff. (3)= (2)-(1)	Never received SRD (4)	Ever received SRD (5)	Diff. (6)= (5)-(4)	(7)= (6)-(3)
Age (years)	31.541	31.832	0.291	31.414	32.290	0.877***	0.586***
	(0.117)	(0.286)	(0.309)	(0.134)	(0.253)	(0.287)	(0.208)
Female	0.481	0.303	-0.178***	0.470	0.308	-0.162***	0.016*
	(0.005)	(0.012)	(0.013)	(0.006)	(0.011)	(0.012)	(0.009)
African/	0.834	0.933	0.099***	0.828	0.930	0.102***	0.003
Black	(0.004)	(0.007)	(0.009)	(0.005)	(0.008)	(0.009)	(0.006)
Married	0.260	0.161	-0.098***	0.254	0.151	-0.103***	-0.004
	(0.004)	(0.010)	(0.011)	(0.005)	(0.008)	(0.010)	(0.008)
Urban	0.681	0.532	-0.148***	0.690	0.548	-0.142***	0.006
	(0.005)	(0.014)	(0.014)	(0.005)	(0.012)	(0.013)	(0.009)
Tertiary	0.083	0.052	-0.031***	0.078	0.053	-0.025***	0.006
education	(0.003)	(0.006)	(0.006)	(0.003)	(0.005)	(0.006)	(0.005)

[a] Author's own calculations. Source: QLFS 2020Q1 - 2021Q1 (Statistics South Africa, 2020a,b,c,d,e).
[b] Notes: This table presents estimates of mean values of observable covariates for individuals who ever or never receipt the SRD grant accompanied by difference estimates in the periods before and after the transfer was introduced. Estimates weighted using sampling weights and standard errors, presented in parentheses, are clustered at the panel (individual) level. The magnitude and statistical significance of a given difference is estimated using a t-test. *** p<0.01; ** p<0.05; * p<0.10.

3.2.2 Model specification

As described earlier, our justification for using a heterogeneity-robust DiD estimator in this context is that while the SRD grant was introduced at a single point in time, its receipt was staggered, with individuals receiving the transfer not all at once but at different points in time. Figure A1 in the appendix illustrates the staggered timing of SRD grant receipt during the study period. We observe several unique treatment histories among individuals who either received the transfer in a certain period, not-yet received the transfer by a certain period, or never receive the transfer during the whole period. The intuition behind Callaway & Sant'Anna's (2021) semi-parametric estimator is that, when SRD grant receipt is staggered, only individuals who had never or not-yet received the SRD grant by a given period are included in the control group. 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 recipients first received the transfer and t refers to a given time period. This is formally calculated as follows:

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

where $EY(g)_t$ is the mean value of a given outcome for group g at time t, and $E(C)_t$ is the mean value of the same outcome for the control group C at time t. The control group here comprises individuals who either never received the SRD or had not-yet received it by time t. As a robustness test, we alternatively restrict this group to those who never received it only. The first term then calculates the between-group difference in outcomes at time t while the second terms does the same but at time $g-1$; that is, the period immediately before the first treatment period for group g. Hence, the estimator accounts for variation in treatment timing by separately calculating the ATT for all groups across all time periods. Importantly, because estimation for each group g requires pre-treatment data at time $g-t$, individuals who are 'always-treated' are automatically omitted from the sample. In addition to removing bias from the canonical design, another key advantage of this approach is that it allows us to obtain up to one year's worth of pre-treatment estimates, despite our data only including one survey wave prior to the transfer's introduction.

As opposed to the singular ATT in canonical DiD designs, this process can, however, result in a potentially large number of individual $ATT(g,t)$'s to consider which may be cumbersome to report. Our study here contains four g's and four post-treatment periods resulting in 16 individual $ATT(g,t)$'s. A particularly attractive feature of the estimator, however, 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 group-specific aggregation which reveals how effects vary depending on when the transfer was first received, the calendar period-specific aggregation which reveals how effects vary across calendar periods, and the event study aggregation to study effect dynamics or how effects vary by the number of times the transfer was received. This latter aggregation also allows us to obtain pre-treatment estimates which can be used to gauge the plausibility of the parallel trends assumption. In doing so, we estimate "long differences" for both the pre- and post-treatment coefficients to ensure that they are numerically equivalent to the dynamic two-way fixed effects (TWFE) specifications in a non-staggered setting (Roth, 2024).[37] To provide a nuanced account of effects, we make use of all of these aggregations in our analysis, the formal derivations of which are described in Callaway & Sant'Anna (2021).[38]

We estimate effects on three main 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.[39] Job search is measured through the item "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 StatsSA's 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, which is consistent with international best practice. We also analyse effect heterogeneity by employment type (wage employment, employer, self-employment, or persons helping unpaid in their household business) and employment formality conditional on gaining employment.[40] For each variable, responses were coded as one for individuals who responded affirmatively and zero if negatively.

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, which are both relevant for our analysis here. In the latter case, researchers can flexibly incorporate covariates to obtain more comparable treatment and control groups through 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 & Zhao (2020). While we report both unconditional and conditional estimates, we employ the DR estimand for the latter which is particularly attractive because it relies on less stringent modelling conditions and enjoys additional robustness against model misspecification.[41] Doing so also reduces residual variance and improves the precision of our estimates. Following the literature, we incorporate only time-varying covariates which include 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. We do however re-estimate all conditional models using the other two alternative estimands as a robustness test.

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 with 1,000 replications.

4 Results

We first present the results for our three primary outcomes and thereafter examine effect heterogeneity by employment type and formality. For each outcome, we estimate several aggregations described above to examine the average, heterogenous, and dynamic effects of SRD grant receipt.

4.1 Main effect estimates

Table 2 presents the aggregated average treatment effect estimates for each of the three primary outcomes. While we report unconditional and conditional results, our preferred estimates are the latter based in columns (2), (4), and (6). Overall, as shown in the top panel, we find evidence of statistically significant, positive, but small effects of SRD grant receipt on job search, trying to start a business, and employment. Specifically, the results suggest that SRD grant receipt increases the likelihood of job search by 4.4 percentage points, trying to start a business by 4.0 percentage points, and employment by 6.0 percentage points, all significant by at least the 5 percent level. The larger employment effects relative to job search and trying to start a business are also suggestive of effects at the intensive margin of each outcome; that is, behaviour conditional on already searching or trying to start a business, which we investigate later. Importantly, these effects are estimated based on an aggregation of average effects, either across all g's and/or t's. Later we reveal important heterogeneity across these dimensions.

The aggregated group-specific estimates in panel (a) show that the positive employment effects were driven by effects on individuals who first received the transfer towards the end of the series at the end of 2020 and beginning of 2021.[42] This coincides with a period characterised by fewer and less stringent pandemic-related restrictions and greater physical mobility, and hence when the labour market participation of job-seekers and job-losers was less constrained (Köhler et al., 2023). In fact, during the hard lockdown in 2020Q2, we do not find any evidence of any job search or employment effects. Alternatively, this effect heterogeneity may also be due to an improvement in the efficiency of the State's administration system of the new transfer, resulting in improved consistency of payments. The effects of such gains in the stability and predictability of transfers are well-documented (de Janvry & Sadoulet, 2006; Bastagli et al., 2016; Gennetian et al., 2021). This pattern is largely consistent with the calendar period-specific estimates, presented in panel (b), which are all – with the exception of the null estimate for 2020Q2, consistent with the group-specific estimates – positive, significant, and varying marginally in magnitude. A similar pattern exists for the effect estimates on job search. In contrast, the estimates on trying to start a business are relatively constant across treatment groups and calendar periods, apart from larger, positive effects for those who first received the transfer in 2020Q3, or for the 2020Q3 period overall.

Table 2: Aggregated average treatment effect estimates of SRD grant receipt

	Pr(Job search)		Pr(Try to start business)		Pr(Employment)	
	Uncond.	Cond. (doubly robust)	Uncond.	Cond. (doubly robust)	Uncond.	Cond. (doubly robust)
	(1)	(2)	(3)	(4)	(5)	(6)
ATT	0.070***	0.044**	0.042***	0.040***	0.068***	0.060***
	(0.019)	(0.018)	(0.007)	(0.007)	(0.010)	(0.010)
Panel (a): First treatment group-specific effects						
Mean	0.075***	0.055***	0.035***	0.034***	0.080***	0.075***
	(0.017)	(0.017)	(0.007)	(0.007)	(0.010)	(0.009)
2020Q2	-0.163	-0.536***	0.011***	0.012***	0.014	-0.382***
	(0.219)	(0.139)	(0.002)	(0.004)	(0.108)	(0.104)
2020Q3	0.068**	0.030	0.080***	0.074***	0.049**	0.044**
	(0.032)	(0.029)	(0.011)	(0.011)	(0.019)	(0.020)
2020Q4	0.068***	0.052**	0.019*	0.019*	0.063***	0.059***
	(0.024)	(0.025)	(0.010)	(0.011)	(0.012)	(0.013)
2021Q1	0.094***	0.085**	0.026*	0.026*	0.127***	0.125***
	(0.032)	(0.034)	(0.014)	(0.014)	(0.020)	(0.020)
Panel (b): Calendar period-specific effects						
Mean	-0.008	-0.176***	0.043***	0.041***	0.080**	-0.085*
	(0.056)	(0.059)	(0.005)	(0.005)	(0.040)	(0.047)
2020Q2	-0.233	-0.814***	0.023***	0.025***	0.103	-0.539***
	(0.212)	(0.218)	(0.002)	(0.007)	(0.152)	(0.183)
2020Q3	0.057*	0.013	0.074***	0.069***	0.082***	0.077***
	(0.034)	(0.035)	(0.015)	(0.014)	(0.024)	(0.023)
2020Q4	0.067***	0.029	0.038***	0.036***	0.074***	0.066***
	(0.021)	(0.023)	(0.010)	(0.010)	(0.012)	(0.012)
2021Q1	0.078***	0.069***	0.037***	0.035***	0.060***	0.056***
	(0.022)	(0.021)	(0.009)	(0.009)	(0.010)	(0.010)
Observations	51,886	51,560	51,886	51,560	51,886	51,560

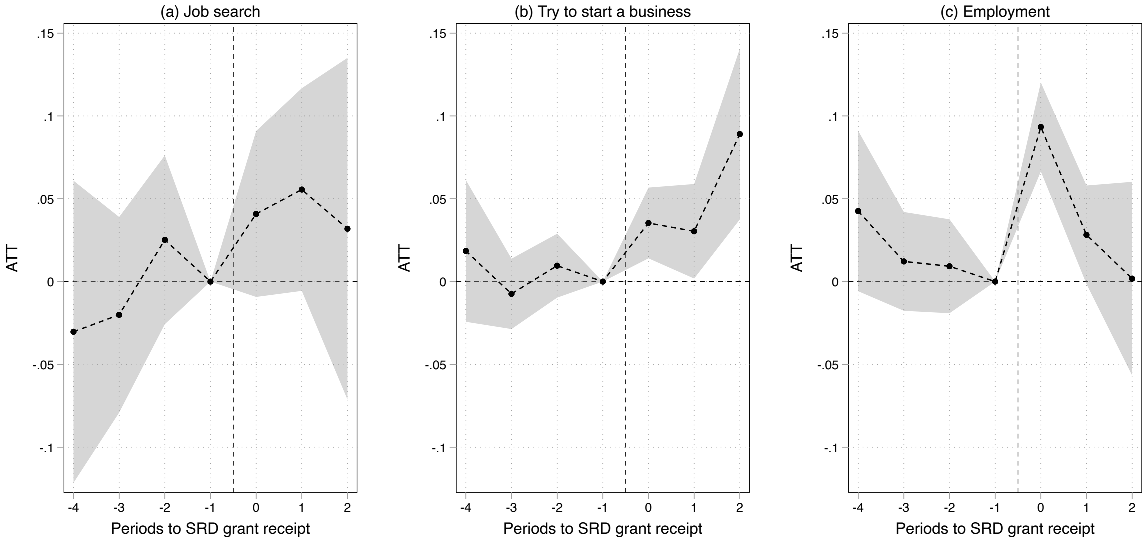
[a] Author's own calculations. Source: QLFS 2020Q1 - 2021Q1 (Statistics South Africa, 2020a,b,c,d,e).
[b] Notes: This table presents difference-in-differences estimates of the effect of receipt of the SRD grant on the three outcomes of interest. Uncond. = unconditional; Cond. = conditional. All models are estimated using Callaway & Sant'Anna's (2021) heterogeneity-robust estimator, while conditional models are estimated by additionally incorporating Sant'Anna & Zhao's (2020) doubly robust estimand. ATT = average treatment effect on the treated. Panel (a) presents the aggregated ATT's across all first-treatment cohorts for each calendar period. Panel (b) presents the aggregated ATT's across all first-treatment cohorts for each calendar period. 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 with 1,000 replications. *** p<0.01; ** p<0.05; * p<0.10.

We next examine the dynamics of these estimated effects using Callaway & Sant'Anna's (2021) event study aggregation. Specifically, we examine effect heterogeneity by length of exposure or duration of receipt of the SRD grant. Effects are estimated and scaled for each period relative to the first period of transfer receipt across all treatment cohorts; that is, regardless of when individuals first received the transfer. This approach is useful for at least two reasons. First, by allowing us to examine pre-treatment outcome dynamics between recipients and non-recipients, we are able to gauge the plausibility of the parallel trends assumption. Second, it allows us to analyse variation in the presence and magnitude of effects beyond the transfer's immediate impact, and hence, whether any short-term gains eventually translate into sustained longer-term benefits (Aizer et al., 2016; Millán et al., 2019) or if transfer income is experienced as a transitory or more permanent income shock (Eyal & Woolard, 2011a).

Our event study estimates are presented in Figure 3.[43] For all outcomes, all pre-treatment estimates are statistically insignificantly different from zero, supporting the plausibility of the parallel trends assumption. Additionally, we find no evidence of any anticipation, further supporting the credibility of our design. First considering panel (c), we find that the SRD grant's positive average employment effects, as observed previously, are driven by effects in the short-term and, importantly, do not persist with longer periods of receipt. The "on impact" (at $t = 0$, or the first quarter) average effect on the probability of employment is positive and relatively large at 9.3 percentage points, significant at the 1 percent level. With additional quarters of receipt, however, the effect quickly dissipates and becomes null. Two and three quarters of receipt yield effects of 2.8 and 0.2 percentage points, respectively, but both are statistically insignificant. Figure A2 in the appendix, which includes a fourth post-treatment period, shows that this null result holds after one full year of receipt.

Complementing the employment effect estimates with those of our other two primary outcomes adds some nuance into understanding the transfer's labour market effects. As shown in panel (a), we do not estimate any significant heterogeneity in effects on the probability of job search across varied durations of receipt. This does not, however, contradict our earlier finding as per Table 2, which reports a positive, average effect. In contrast, the estimates in panel (b) suggest that receipt of the transfer increases recipients' probability of trying to start a business both initially but also increasingly with additional periods of receipt. While we estimate the first quarter of receipt increases this probability by 3.5 percentage points, similar in magnitude to the average

Figure 3: Event study estimates of the dynamic labour market effects of SRD grant receipt



[a] Author's own calculations. Source: QLFS 2020Q1 - 2021Q1 (Statistics South Africa, 2020a,b,c,d,e).
[b] Notes: Models estimated using Callaway & Sant'Anna's (2021) heterogeneity-robust difference-in-differences estimator and Sant'Anna & Zhao's (2020) doubly robust estimand. ATT's are estimated for each period relative to the first-treatment period, across all first-treatment cohorts. Periods to SRD grant receipt indexes the length of receipt. Estimates are weighted using sampling weights. Standard errors are clustered at the panel (individual) level and are estimated using a multiplicative wild bootstrap procedure with 1,000 replications. Shaded areas represent 95 percent confidence intervals.

effect, three quarters of receipt yields an effect of over 8 percentage points. The estimates in Figure A2 in the appendix, however, reveals a null effect – statistically insignificant and close to zero in magnitude – after one full year of receipt, similar to the employment results. Taken together, while we find some variation within the period, this analysis imply that the short-term labour market benefits of the SRD grant do not translate into sustained longer-term benefits over one year, at least during the transfer's initial design.

We posit that this null, longer-term employment effect is likely explained by the extent and nature of unemployment in South Africa which limits the extent gains in a recipient's ability to search for work or start a business translate into employment. First, considering job search, as referenced earlier the literature suggests that cash transfers can alleviate liquidity constraints which limit an individual's ability to search for work (Rose, 2001; Gertler et al., 2012; Ervin et al., 2017; Baird et al., 2018; Daidone et al., 2019), thus lowering job search costs and addressing spatial mismatches between the location of jobseekers' households and job opportunities. This mismatch tends to be particularly large in South Africa's context as an enduring consequence of apartheid-era policy (Shah, 2022; Asmal et al., 2024). Additionally, cash transfers can alleviate insurance constraints which reduce recipients' willingness to engage in productive but risky activities, such as job search, with high potential rewards, such as employment (Hennessy, 1998; Serra et al., 2006; Baird et al., 2018; Bastagli et al., 2019). While this is consistent with our estimated job search effects and appears to translate into some recipients gaining jobs initially, it does not persist into the longer-term. This is plausibly due to both a combination of a severe scarcity of jobs as well as a largely structural nature of unemployment in the country. Job-seekers, characterised by relatively low skill levels,[44] are confronted with a labour market experiencing increasing demand for high-skilled workers due to structural shifts in the economy (Banerjee et al., 2008; Pauw et al., 2008). The consequence is a significant skills mismatch and a persistently high rate of unemployment. This is strongly consistent with recent experimental evidence in the country by Banerjee & Sequeira (2023) who show that, while reductions in search costs address spatial mismatch and increase job search in initially, the failure of jobs to materialise immediately leads job-seekers to become more impatient, adjust their expectations, and look for work closer to home, which also does not improve their employment odds since nearby jobs are also scarce.

Second, considering starting a business, the literature on liquidity and insurance constraints is again relevant given that cash transfers can alleviate the constraints which limit an individual's ability to invest in capital to start or expand a business or compensate for the risk that doing so entails. However, in South Africa, unusually high barriers to entry into the informal sector serve as an additional and significant constraint which the SRD grant may not be able to address, at least in its initial design. It is well-documented that the country is an international outlier regarding its relatively small informal sector (Banerjee et al., 2008; Davies & Thurlow, 2010; Shah, 2022; Köhler et al., 2023; Asmal et al., 2024). Just one third of workers are informally employed compared to an average of 70 percent in low- and middle-income countries and 86 percent in Africa (International Labour Office, 2018). It has been argued that the size of the informal sector is not unrelated to the country's extremely high rate of unemployment. Existing studies provide evidence that informal work is indeed preferred over unemployment, however numerous barriers to entry exist such as a high prevalence of crime and associated insecurity, inadequate government support, unavailability of infrastructure, spatial mismatch between the location of informal traders' households and their consumers, high transport and licensing costs, and punitive regulations which restrict where informal traders can operate (Kingdon & Knight, 2004; Banerjee et al., 2008; Davies & Thurlow, 2010; Hausmann et al., 2023; Asmal et al., 2024). Many of these latter constraints are not common in other developing countries, leading to the argument that informal sector regulation in South Africa leans more towards urban management than small business development, which is more akin to such regulation in advanced economies (Asmal et al., 2024). Overall, our estimates suggest that the SRD grant in its initial design does address some of the aforementioned constraints to job search and starting a business – which we explore in more detail later – leading some recipients to gain jobs initially. However, the existence of other constraints means that such gains do not translate into better employment prospects in the longer-term.

4.2 Effect heterogeneity by employment type

We now further analyse the employment effects of the transfer by considering effect heterogeneity by employment type. Specifically, we estimate effects on the probabilities of becoming an employee or in wage employment, an employer, self-employed, or working unpaid un a household business (henceforth referred to as an unpaid household worker), all conditional on becoming employed. Table 3 presents the relevant treatment effect estimates for each outcome and aggregation.

We find that the positive employment effect observed previously is driven by positive effects on the probabilities of wage and self employment. As shown in columns (2) and (6), SRD grant receipt increases the probability of wage employment by 4.2 percentage points and self-employment by 2.1 percentage points, both significant at the 1 percent level. The group- and calendar period-specific estimates for these outcomes in panels (a) and (b) are analogous to those in Table 2, which again highlights them as the drivers of the overall employment results. These results may at least partially be explained by two key characteristics of South Africa's labour market. First, it's plausible that the SRD grant increases recipients' ability to access wage jobs simply because these account for the majority of jobs in the country, regardless of sector (Kohler et al., 2022; Yu et al., 2023). Second and relatedly, large-scale unemployment reflects a scarcity of these jobs, and hence, the SRD grant may induce some recipients to opt for self-employment as an alternative which is typically characterised by lower barriers to entry (Falco & Haywood, 2016; Asmal et al., 2024). In contrast to the effects on wage and self-employment, we do not find compelling evidence of an average effect on the probabilities of becoming an employer or unpaid household worker, as shown in columns (4) and (8). The estimate for the former is statistically insignificant, and while that of the latter holds more significance, the estimates for both outcomes are very close to zero in magnitude. Moreover, these null average effects largely hold across treatment groups and calendar periods, as shown by most of the group- and calendar period-specific estimates which are close to zero in magnitude or are statistically insignificant.

Figure 4 presents the event study estimates for these outcomes.[45] As in Figure 3, the absence of significant pre-treatment coefficients for all outcomes yields support for the parallel trends assumption and, hence, our research design. Like the overall employment results in Figure 3, panel (a) shows that receipt of the SRD grant increases the probability of wage employment, but only in the short-term. The "on impact" (at $t = 0$) average effect here is again positive and relatively large at 6.3 percentage points, significant at the 1 percent level. However, as before, with additional quarters of receipt this effect does not persist but instead quickly dissipates and becomes null. With the exception of the initial effect, the estimate is no longer significant from two quarters and up to one full year of receipt. The effect dynamics for self-employment, as shown in panel (c), are qualitatively similar, however the "on impact" effect is only half the magnitude (3.1 percentage points) of the effect on wage employment. This is consistent with the differences in average effect estimates in Table 3. Again in line with the average effect estimates, the effects on the probabilities of becoming an employer or unpaid household worker are all insignificant and close to zero in magnitude, regardless of duration of receipt.

4.3 Effect heterogeneity by employment formality

As before, we again further analyse the employment effects of the transfer but now consider effect heterogeneity by employment formality; that is, effects on the probabilities of becoming employed in the formal or informal sector. Table 4 presents the relevant treatment effect estimates. The estimates in columns (2) and (4) reveal significant effects of SRD grant receipt on both formal and informal sector employment, however the magnitude of the latter is about double the size of the former (3.6 versus 1.9 percentage points, respectively). This suggests that the positive, average employment effect observed previously is primarily, but not completely, explained by effects on informal sector employment.[46] These results may at least partially be explained by the typically lower barriers to entry into informal work (Mattos & Ogura, 2009; Davies & Thurlow, 2010; Asmal et al., 2024). The distributions of group- and calendar period-specific estimates in panels (a) and (b) are again very similar to those in Table 2, again primarily highlighting larger effects for those who first received the transfer later in the period.

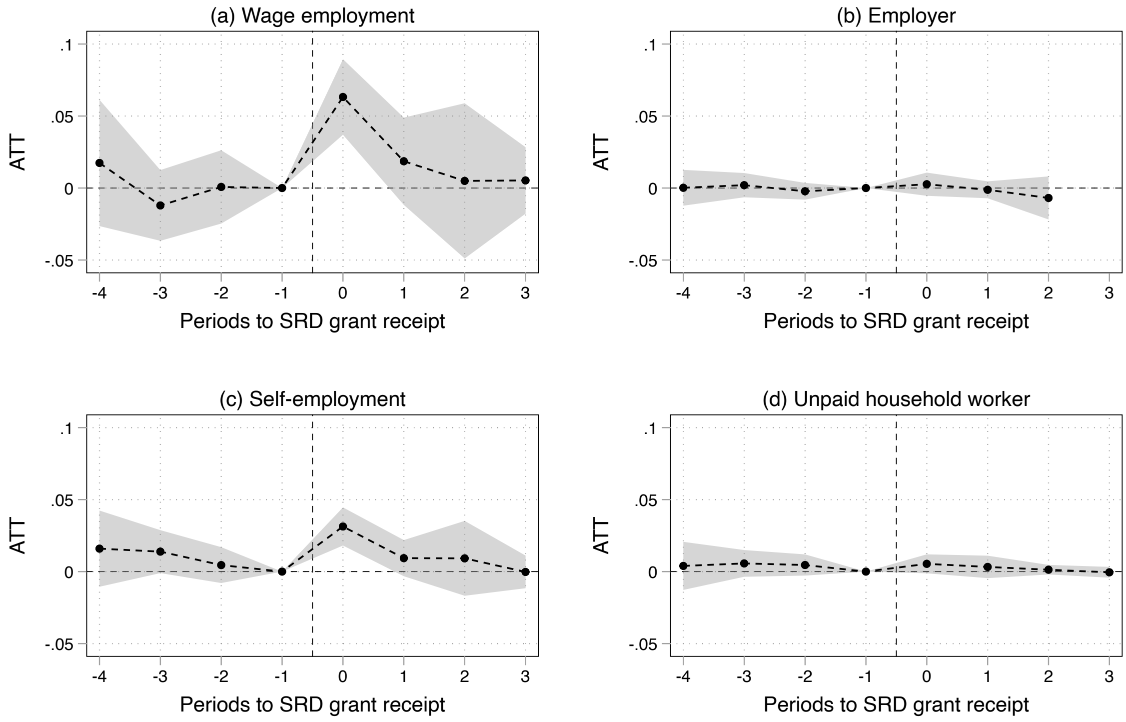
Table 3: Aggregated average treatment effect estimates of SRD grant receipt, by employment type

	Pr(Wage employment)		Pr(Employer)		Pr(Self-employment)		Pr(Unpaid HH employment)	
	Uncond.	Cond. (doubly robust)	Uncond.	Cond. (doubly robust)	Uncond.	Cond. (doubly robust)	Uncond.	Cond. (doubly robust)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	0.044***	0.042***	0.004	0.000	0.024***	0.021***	0.004**	0.004*
	(0.009)	(0.008)	(0.002)	(0.002)	(0.004)	(0.004)	(0.002)	(0.002)
Panel (a): First treatment group-specific effects								
Mean	0.051***	0.049***	0.004	0.002	0.029***	0.026***	0.004**	0.004*
	(0.009)	(0.008)	(0.003)	(0.003)	(0.005)	(0.004)	(0.002)	(0.002)
2020Q2	0.091*	0.065*	-0.024	-0.406	0.004	0.005	0.009	0.008
	(0.049)	(0.037)	(0.132)	(0.106)	(0.013)	(0.013)	(0.014)	(0.013)
2020Q3	0.027	0.027	0.006*	0.005	0.025***	0.020**	0.002**	0.003**
	(0.017)	(0.018)	(0.004)	(0.003)	(0.009)	(0.010)	(0.001)	(0.001)
2020Q4	0.043***	0.040***	0.001	0.001	0.015***	0.013***	0.007*	0.006*
	(0.011)	(0.011)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
2021Q1	0.076***	0.075***	0.007	0.007	0.050***	0.049***	0.001	0.001
	(0.017)	(0.018)	(0.006)	(0.006)	(0.012)	(0.011)	(0.003)	(0.003)
Panel (b): Calendar period-specific effects								
Mean	0.079***	0.059**	0.003	0.002	0.023***	0.021***	0.010	0.009
	(0.028)	(0.025)	(0.034)	(0.003)	(0.009)	(0.008)	(0.010)	(0.009)
2020Q2	0.179*	0.102	-0.003	-0.406	0.010	0.011	0.026	0.024
	(0.105)	(0.093)	(0.134)	(0.106)	(0.040)	(0.035)	(0.040)	(0.038)
2020Q3	0.050**	0.050**	0.009	0.005	0.039***	0.036***	0.004*	0.004
	(0.021)	(0.020)	(0.006)	(0.003)	(0.011)	(0.012)	(0.002)	(0.003)
2020Q4	0.052***	0.050***	0.005*	0.001	0.018***	0.015***	0.007	0.007**
	(0.011)	(0.011)	(0.003)	(0.003)	(0.005)	(0.005)	(0.003)	(0.003)
2021Q1	0.035***	0.034***	0.001	0.007	0.025***	0.023***	0.002	0.002
	(0.009)	(0.010)	(0.003)	(0.006)	(0.005)	(0.005)	(0.002)	(0.002)
Observations	51,886	51,560	51,886	51,560	51,886	51,560	51,886	51,560

[a] Author's own calculations. Source: QLFS 2020Q1 - 2021Q1 (Statistics South Africa, 2020a,b,c,d,e).

[b] Notes: This table presents difference-in-differences estimates of the effect of receipt of the SRD grant on employment by type. Uncond. = unconditional; Cond. = conditional. All models are estimated using Callaway & Sant'Anna's (2021) heterogeneity-robust estimator, while conditional models are estimated by additionally incorporating Sant'Anna & Zhao's (2020) doubly robust estimand. ATT = average treatment effect on the treated. Panel (a) presents the aggregated ATT's across all periods for each first-treatment cohort. Panel (b) presents the aggregated ATT's across all first-treatment cohorts for each calendar period. 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 with 1,000 replications. *** p<0.01; ** p<0.05; * p<0.10.

Figure 4: Event study estimates of the dynamic labour market effects of SRD grant receipt, by employment type



[a] Author's own calculations. Source: QLFS 2020Q1 - 2021Q1 (Statistics South Africa, 2020a,b,c,d,e).
[b] Notes: Models estimated using Callaway & Sant'Anna's (2021) heterogeneity-robust difference-in-differences estimator and Sant'Anna & Zhao's (2020) doubly robust estimand. ATT's are estimated for each period relative to the first-treatment period, across all first-treatment cohorts. Periods to SRD grant receipt indexes the length of receipt. Estimates are weighted using sampling weights. Standard errors are clustered at the panel (individual) level and are estimated using a multiplicative wild bootstrap procedure with 1,000 replications. Shaded areas represent 95 percent confidence intervals.

The larger informal sector employment effects suggest that the transfer may be able to play a role in diminishing the aforementioned barriers to entry into the sector (Kingdon & Knight, 2004; Banerjee et al., 2008; Heintz & Posel, 2008; Davies & Thurlow, 2010; Hausmann et al., 2023; Asmal et al., 2024). However, as before, we do not find evidence that the positive effects on both formal and informal sector employment persist over time. As before, Figure 5 presents the event study estimates for these outcomes. The absence of significant pre-treatment coefficients again lend support for the parallel trends assumption.[47] Similar to the effects on overall employment in Figure 3 and wage and self-employment in Figure 4, both panels (a) and (b) show that SRD grant receipt increases the probabilities of both formal and informal sector employment in the short-term, but not thereafter. The magnitude of the "on impact" effect on formal sector employment is again smaller that of the effect on informal sector employment (3.5 versus 5.4 percentage points, respectively). As before, with additional quarters of receipt these effects disappear. The estimates again become null from two quarters and up to one full year of receipt.

Table 4: Aggregated average treatment effect estimates of SRD grant receipt, by employment formality

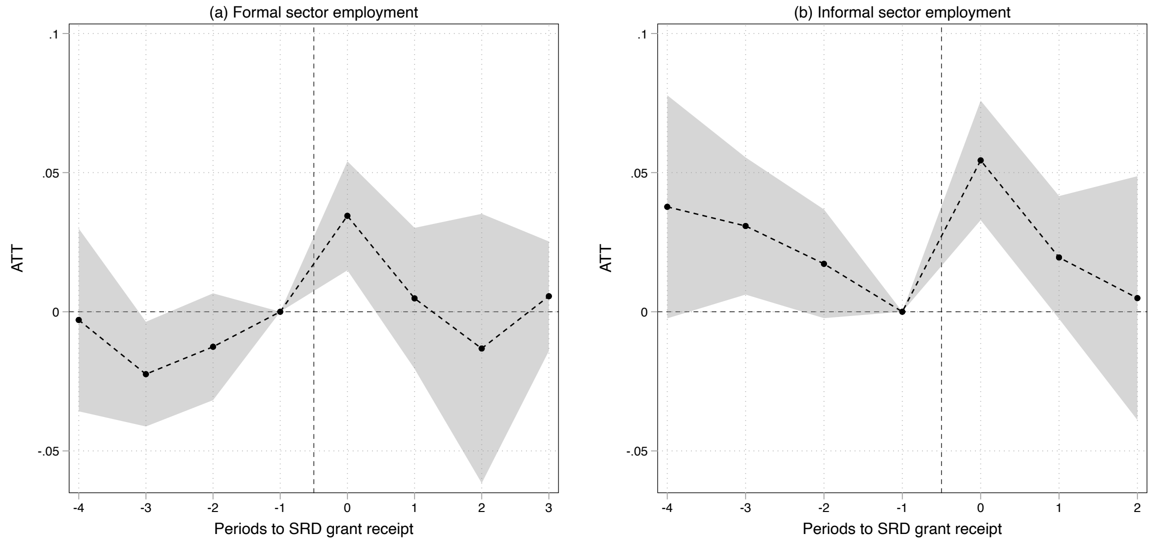
	Pr(Formal sector employment)		Pr(Informal sector employment)	
	Uncond. (1)	Cond. (doubly robust) (2)	Uncond. (3)	Cond. (doubly robust) (4)
ATT	0.020***	0.019**	0.043***	0.036***
	(0.007)	(0.008)	(0.006)	(0.007)
Panel (a): First treatment group-specific effects				
Mean	0.024***	0.025***	0.050***	0.045***
	(0.007)	(0.007)	(0.007)	(0.007)
2020Q2	-0.001	-0.008	-0.039	-0.415
	(0.009)	(0.008)	(0.129)	(0.109)
2020Q3	0.010	0.007	0.034***	0.033***
	(0.016)	(0.017)	(0.013)	(0.012)
2020Q4	0.020**	0.021**	0.038***	0.035***
	(0.009)	(0.009)	(0.008)	(0.009)
2021Q1	0.040***	0.043***	0.077***	0.075***
	(0.015)	(0.015)	(0.015)	(0.015)
Panel (b): Calendar period-specific effects				
Mean	0.014*	0.008	0.043	-0.105
	(0.008)	(0.009)	(0.037)	(0.047)
2020Q2	-0.013	-0.032	0.038	-0.543
	(0.026)	(0.019)	(0.146)	(0.189)
2020Q3	0.029	0.025	0.052***	0.051***
	(0.019)	(0.019)	(0.016)	(0.016)
2020Q4	0.028***	0.025***	0.042***	0.037***
	(0.009)	(0.010)	(0.009)	(0.009)
2021Q1	0.012	0.014*	0.041***	0.036***
	(0.008)	(0.008)	(0.008)	(0.008)
Observations	51,886	51,560	51,886	51,560

[a] Author's own calculations. Source: QLFS 2020Q1 - 2021Q1 (Statistics South Africa, 2020a,b,c,d,e).
[b] Notes: This table presents difference-in-differences estimates of the effect of receipt of the SRD grant on employment by formality. Uncond. = unconditional; Cond. = conditional. All models are estimated using Callaway & Sant'Anna's (2021) heterogeneity-robust estimator, while conditional models are estimated by additionally incorporating Sant'Anna & Zhao's (2020) doubly robust estimand. ATT = average treatment effect on the treated. Panel (a) presents the aggregated ATT's across all periods for each first-treatment cohort. Panel (b) presents the aggregated ATT's across all first-treatment cohorts for each calendar period. 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 with 1,000 replications. *** p<0.01; ** p<0.05; * p<0.10.

4.4 Mechanisms

As noted previously, the transfer's short-term employment effects can at least partially be explained by its effects on job search and trying to start a business at the extensive margin. However, the larger employment estimates are suggestive of additional mechanisms at the intensive margin; that is, behaviour related to job search or trying to start a business conditional on already doing so. Identifying such effects aids one's understanding of the underlying mechanisms of the transfer's labour market effects. To investigate this, we first stratify the sample and re-estimate the model to examine how these employment effects vary between those who were

Figure 5: Event study estimates of the dynamic labour market effects of SRD grant receipt, by employment formality



[a] Author's own calculations. Source: QLFS 2020Q1 - 2021Q1 (Statistics South Africa, 2020a,b,c,d,e).
[b] Notes: Models estimated using Callaway & Sant'Anna's (2021) heterogeneity-robust difference-in-differences estimator and Sant'Anna & Zhao's (2020) doubly robust estimand. ATT's are estimated for each period relative to the first-treatment period, across all first-treatment cohorts. Periods to SRD grant receipt indexes the length of receipt. Estimates are weighted using sampling weights. Standard errors are clustered at the panel (individual) level and are estimated using a multiplicative wild bootstrap procedure with 1,000 replications. Shaded areas represent 95 percent confidence intervals.

already searching or trying to start a business and those who were not. The relevant treatment effect estimates are presented in Table 5 and the event study estimates in Figure 6. The estimates in the top panel of the table are indicative of positive, significant effects for all groups of the unemployed. However, effects are significantly larger for those already engaged in job search or trying to start a business. On average, SRD grant receipt increases the likelihood of employment by 9.5 percentage points for searchers but just 3.8 percentage points for non-searchers, and 17.3 percentage points for those trying to start a business compared to 5.4 percentage points for those who are not. This disparity is also largely evident for the estimates in both panels (a) and (b) again, with the former again suggesting that effects tend to be larger for those who first received the transfer towards the end of the series.[48] This event study estimates in Figure 6 also reveal approximately twice as large effects for those already engaged in job search and trying to start a business as their counterparts.[49] Importantly, and as before, for all groups these effects are positive and significant in the first quarter of receipt but approach zero and become null thereafter. Overall, these results suggest that the transfer has larger positive, short-term employment effects on the intensive margin; that is, among the unemployed who are already engaged in the labour market.

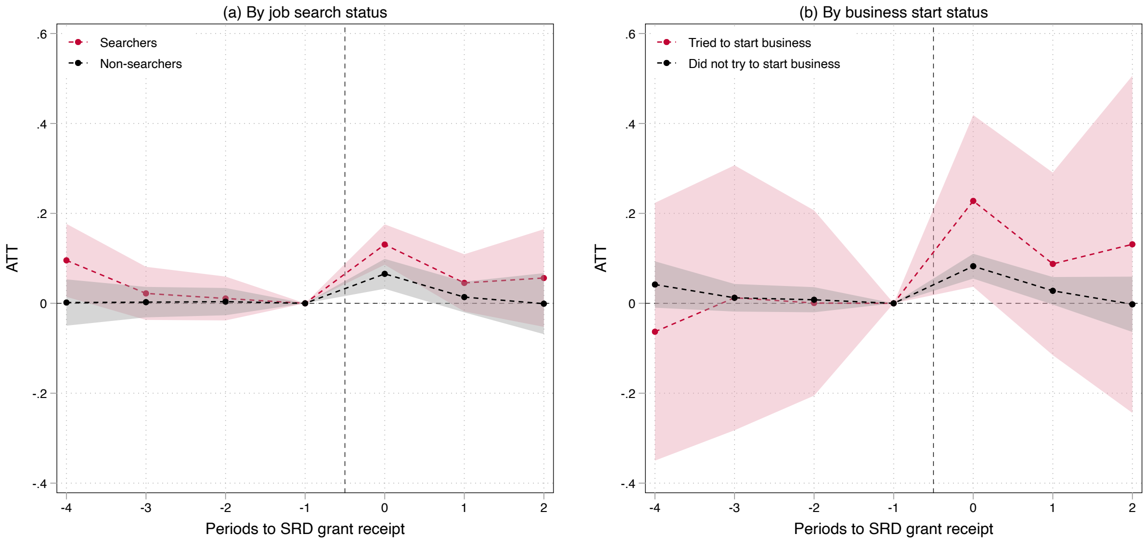
Table 5: Aggregated average treatment effect estimates of SRD grant receipt on employment, by job search and trying to start a business status

Sample:	(a) By job search status		(b) By business start status	
	Searchers	Non-searchers	Tried to start business	Did not try to start business
	(1)	(2)	(3)	(4)
ATT	0.095***	0.038***	0.173**	0.054***
	(0.017)	(0.012)	(0.068)	(0.010)
Panel (a): First treatment group-specific effects				
Mean	0.110***	0.047***	0.167***	0.068***
	(0.017)	(0.011)	(0.062)	(0.010)
2020Q2	-0.154	-0.077	.	-0.342***
	(0.167)	(0.203)	.	(0.102)
2020Q3	0.083**	0.024	0.225**	0.038*
	(0.037)	(0.023)	(0.102)	(0.020)
2020Q4	0.076***	0.039***	0.121	0.052***
	(0.022)	(0.013)	(0.100)	(0.011)
2021Q1	0.167***	0.083***	0.180	0.117***
	(0.032)	(0.027)	(0.122)	(0.021)
Panel (b): Calendar period-specific effects				
Mean	-0.060	0.064**	0.214***	-0.091**
	(0.070)	(0.029)	(0.070)	(0.044)
2020Q2	-0.541*	0.126	.	-0.539***
	(0.296)	(0.105)	.	(0.173)
2020Q3	0.111***	0.059**	0.349***	0.065**
	(0.042)	(0.028)	(0.135)	(0.026)
2020Q4	0.102***	0.039***	0.180**	0.059***
	(0.023)	(0.013)	(0.080)	(0.013)
2021Q1	0.087***	0.030**	0.112	0.052***
	(0.019)	(0.014)	(0.075)	(0.011)
Observations	16,660	34,900	1,819	49,741

[a] Author's own calculations. Source: QLFS 2020Q1 - 2021Q1 (Statistics South Africa, 2020a,b,c,d,e).
[b] Notes: This table presents difference-in-differences estimates of the effect of receipt of the SRD grant on employment using four separate samples of the unemployed. All models are conditional and are estimated using Callaway & Sant'Anna's (2021) heterogeneity-robust estimator and Sant'Anna & Zhao's (2020) doubly robust estimand. ATT = average treatment effect on the treated. 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 with 1,000 replications. *** p<0.01; ** p<0.05; * p<0.10.

We further interrogate these larger employment effects among those already searching or trying to start a business by examining effects at the intensive margin; that is, effects on outcomes related to job search or trying to start a business conditional on already doing so. We do so by exploiting eight items in the survey instrument which asked those respondents who responded affirmatively to either searching for work or trying to start a business about their related behaviour in the four weeks preceding the survey. These included waiting or registering at an employment agency or trade union; enquiring at workplaces; placing or answering advertisements;

Figure 6: Event study estimates of the dynamic effects of SRD grant receipt on employment, by job search and trying to start a business status



[a] Author's own calculations. Source: QLFS 2020Q1 - 2021Q1 (Statistics South Africa, 2020a,b,c,d,e).
[b] Notes: Models estimated using Callaway & Sant'Anna's (2021) heterogeneity-robust difference-in-differences estimator and Sant'Anna & Zhao's (2020) doubly robust estimand. ATT's are estimated for each period relative to the first-treatment period, across all first-treatment cohorts. Periods to SRD grant receipt indexes the length of receipt. Estimates are weighted using sampling weights. Standard errors are clustered at the panel (individual) level and are estimated using a multiplicative wild bootstrap procedure with 1,000 replications. Shaded areas represent 95 percent confidence intervals.

searching through advertisements or the internet; seeking assistance from relatives or friends; looking for assets or applying for a permit; waiting at the street side where casual workers are found; and seeking any financial assistance. We use this data to construct eight outcomes and, as before, code responses to each of these items as one for individuals who responded affirmatively and zero if negatively. The aggregated average effect estimates of SRD grant receipt on each of these outcomes are presented in Table 6. We find statistically significant effects for just two outcomes: searching job advertisements and looking for assets or applying for a permit. The estimated effect on the former is, however, twice the size of that on the latter: SRD grant receipt increases the probabilities of searching job advertisements by 5.5 percentage points and looking for assets or applying for a permit to start a business by just under 3 percentage points. Figure A5 in the appendix presents the event study estimates for these two outcomes. While the effect on the probability of looking for assets or applying for a permit remains smaller than that on the probability of searching job advertisements, both appear to grow with a longer duration of receipt. However, most of these estimates are insignificant. The estimates for all other outcomes are insignificant and much closer to zero in magnitude.

Table 6: Aggregated average treatment effect estimates of SRD grant receipt on intensive margin behaviour

	Registered at employment agency (1)	Enquired at workplaces (2)	Placed/answered advertisements (3)	Searched advertisements (4)
ATT	-0.013	-0.011	-0.006	0.055**
	(0.023)	(0.027)	(0.024)	(0.025)
Observations	12,356	14,165	13,946	13,535
	Sought assistance from network (5)	**Looked for assets or applied for business permit (6)**	**Waited at street side as casual worker (7)**	**Sought financial assistance (8)**
ATT	-0.012	0.028***	0.010	-0.005
	(0.024)	(0.007)	(0.017)	(0.019)
Observations	14,390	11,789	11,863	11,723

[a] Author's own calculations. Source: QLFS 2020Q1 - 2021Q1 (Statistics South Africa, 2020a,b,c,d,e).
[b] Notes: This table presents difference-in-differences estimates of the effect of receipt of the SRD grant on intensive margin behaviour, conditional on job search or trying to start a business. All models are conditional and are estimated using Callaway & Sant'Anna's (2021) heterogeneity-robust estimator and Sant'Anna & Zhao's (2020) doubly robust estimand. ATT = average treatment effect on the treated. 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 with 1,000 replications. *** p<0.01; ** p<0.05; * p<0.10.

Taken together, the results above suggest that the transfer's positive, short-term employment effects can be explained by its effects on both extensive and intensive margin behaviour, but they appear driven by the latter. At the extensive margin, receipt of the transfer increases the likelihood of both job search and trying to start a business and, hence, enables a subset of the unemployed who are doing neither to transition into these activities, thus improving their employment prospects. At the intensive margin – that is, among those already engaged in labour market by actively searching or trying to start a business – receipt improves the 'efficiency' of these two outcomes, resulting in even greater gains in employment prospects. Our analysis suggests that this is done by increasing the likelihood of at least two behaviours: searching through job advertisements or the internet, and looking for assets or applying for a permit to start a business. This is consistent with the literature on two particular mechanisms through which labour supply responds to cash transfers; namely, liquidity and insurance constraints, as referenced earlier. First, the transfer may help recipients overcome a liquidity constraint which prevents them from searching for work or undertaking certain investments – like acquiring a permit or an asset – that would enable them to work (Rose, 2001; Banerjee et al., 2017; Baird et al., 2018; Salehi-Isfahani & Mostafavi-Dehzooei, 2018; Daidone et al., 2019). Second, the transfer may serve as a form of insurance against the risk of failure and enable recipients to undertake risky activities with high potential rewards, such as starting a business or acquiring an asset (Hennessy, 1998; Serra et al., 2006; Baird et al., 2018; Bastagli et al., 2019). Both of these mechanisms are strongly consistent with qualitative evidence in South Africa that the SRD grant alleviates recipients' job search costs and allows some to acquire work-related assets (Plagerson et al., 2023; Venter et al., 2024).

5 Robustness tests

We conduct several robustness tests to examine the sensitivity of our results to specification-related decisions. In our analysis, our estimates of effects use of a control group who comprise individuals who had not-yet received the transfer by time t as well as those who never received the transfer in the period. This approach then includes individuals who may anticipate receiving the transfer in later periods, which may introduce of source of bias in our estimates. Fortunately, Callaway & Sant'Anna's (2021) estimator allows one to control for treatment anticipation behaviour through a control group restriction. As such, we re-estimate our overall and sub-group models but restrict the control group to only those who never received the transfer in the period. The aggregated treatment effect estimates for all outcomes are presented 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 0.3 percentage points. This similarly holds for the standard errors, resulting in largely identical levels of statistical significance across all estimates. Overall, these estimates suggest that our main results are not sensitive to the inclusion of 'not-yet treated' individuals in our control group.

We next explore the sensitivity of our results to our choice of estimand to incorporate covariates into the modelling to obtain more comparable groups of transfer recipients and non-recipients. As described previously, our preferred approach adopts Sant'Anna & 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

Table 7: Aggregated average treatment effect estimates of SRD grant receipt, by control group composition

	Main 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)
Control group:									
NT + NYT	0.044**	0.040***	0.060***	0.042***	0.000	0.021***	0.004*	0.019**	0.036***
	(0.018)	(0.007)	(0.010)	(0.008)	(0.002)	(0.004)	(0.002)	(0.008)	(0.007)
NT only	0.046***	0.038***	0.063***	0.042***	0.003	0.021***	0.004	0.021***	0.039***
	(0.016)	(0.007)	(0.010)	(0.009)	(0.002)	(0.004)	(0.002)	(0.007)	(0.006)

[a] Author's own calculations. Source: QLFS 2020Q1 - 2021Q1 (Statistics South Africa, 2020a,b,c,d,e).

[b] Notes: This table presents difference-in-differences estimates of the effects of receipt of the SRD grant by varied control group compositions. All models are conditional and are estimated using Callaway & Sant'Anna's (2021) heterogeneity-robust estimator and Sant'Anna & Zhao's (2020) doubly robust estimand. NT = observations never treated in the period; NYT = observations 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 with 1,000 replications. *** p<0.01; ** p<0.05; * p<0.10.

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. For all outcomes, we find that our effect estimates are largely insensitive to the choice of estimand. Qualitatively, our main results hold, but in some instances the magnitude and statistical significance of a given estimate increases. For instance, the coefficient for job search effects rises from 0.044 to 0.057 and 0.061 using the OR and IPW estimands, respectively. In all cases the estimate remains highly statistically significant. Similarly but to a lesser extent, the coefficient for trying to start a business rises marginally to 0.041 using these estimands. For all other outcomes, the magnitudes of the coefficients and levels of statistical significance are also relatively relatively constant. Overall, these results suggest that our main estimates are largely insensitive to the choice of estimand.

Finally, we consider the fact that Callaway & Sant'Anna's (2021) estimator assumes that treatment is an 'absorbing state'. That is, once an individual receives the SRD grant, they remain a recipient of it for the remainder of the panel such that treatment exposure is 'weakly increasing' (it either remains the same or increases). While we believe this to be an appropriate assumption given that it implies individuals do not "forget about their treatment experience", as previously noted, a small subset of individuals in our sample experience non-absorbing treatment: 4 percent of all observations, or 22 percent of unique individuals who ever report receipt. This is evident when examining the patterns of treatment rollout in Figure A1 in the appendix, referenced earlier. To examine the sensitivity of our results to this assumption, we adjust the specification of our main models to include an additional binary control variable which identifies this subset of individuals. As an alternative, we also re-estimate the models while excluding these individuals from the sample entirely. The relevant aggregated treatment effect estimates are presented in Table 9. Comparing our main estimates in column (1) to those of the alternative models in columns (2) and (3), the largest difference in coefficients is very small at just 0.006, while the largest difference in standard errors is 0.003. Due to the smaller sample, the latter tend to be marginally inflated in column (3). Hence, the levels of significance for effect estimates on given outcome are relatively constant across all models. This suggests that our main results are largely insensitive to our 'absorbing state' treatment assumption.

Table 8: Aggregated average treatment effect estimates of SRD grant receipt across varied estimands

	Main estimand	Alternative estimands	
	Doubly robust (Sant'Anna & Zhao, 2020) (1)	Outcome regression adjustment (2)	IPW with stablised weights (3)
Panel (a): Main outcomes			
Pr(Job search)	0.044**	0.057***	0.061***
	(0.018)	(0.017)	(0.018)
Pr(Try to start business)	0.040***	0.041***	0.041***
	(0.007)	(0.007)	(0.007)
Pr(Employment)	0.060***	0.066***	0.067***
	(0.010)	(0.010)	(0.010)
Panel (b): By employment type			
Pr(Wage employment)	0.042***	0.042***	0.043***
	(0.008)	(0.009)	(0.009)
Pr(Employer)	0.000	0.003	0.004
	(0.002)	(0.002)	(0.002)
Pr(Self-employment)	0.021***	0.022***	0.023***
	(0.004)	(0.005)	(0.005)
Pr(Unpaid household employment)	0.004*	0.004*	0.004*
	(0.002)	(0.002)	(0.002)
Panel (c): By employment formality			
Pr(Formal sector employment)	0.019**	0.019**	0.020**
	(0.008)	(0.008)	(0.008)
Pr(Informal sector employment)	0.036***	0.041***	0.042***
	(0.007)	(0.007)	(0.007)

[a] Author's own calculations. Source: QLFS 2020Q1 - 2021Q1 (Statistics South Africa, 2020a,b,c,d,e).
[b] Notes: This table presents difference-in-differences estimates of the effect of receipt of the SRD grant on the outcomes of interest across varying estimands to incorporate covariates. All models are estimated using Callaway & Sant'Anna's (2021) heterogeneity-robust estimator. 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 with 1,000 replications. *** p<0.01; ** p<0.05; * p<0.10.

Table 9: Aggregated average treatment effect estimates of SRD grant receipt, adjusted for non-absorbing treatment

	Main results	Adjustment for non-absorbing treatment	
		Regression adjustment	Stratified sample
	(1)	(2)	(3)
Panel (a): Main outcomes			
Pr(Job search)	0.044**	0.050***	0.046**
	(0.018)	(0.016)	(0.019)
Pr(Try to start business)	0.040***	0.041***	0.040***
	(0.007)	(0.007)	(0.007)
Pr(Employment)	0.060***	0.063***	0.063***
	(0.010)	(0.010)	(0.012)
Panel (b): By employment type			
Pr(Wage employment)	0.042***	0.043***	0.040***
	(0.008)	(0.009)	(0.012)
Pr(Employer)	0.000	0.000	0.004
	(0.002)	(0.002)	(0.003)
Pr(Self-employment)	0.021***	0.024***	0.024***
	(0.004)	(0.004)	(0.006)
Pr(Unpaid household employment)	0.004*	0.004*	0.004*
	(0.002)	(0.002)	(0.002)
Panel (c): By employment formality			
Pr(Formal sector employment)	0.019**	0.019**	0.018*
	(0.008)	(0.008)	(0.010)
Pr(Informal sector employment)	0.036***	0.040***	0.041***
	(0.007)	(0.007)	(0.008)

[a] Author's own calculations. Source: QLFS 2020Q1 - 2021Q1 (Statistics South Africa, 2020a,b,c,d,e).
[b] Notes: This table presents difference-in-differences estimates of the effect of receipt of the SRD grant on the outcomes of interest after accounting for a subset of the treated sample who experience non-absorbing treatment in the panel. All models are estimated using Callaway & Sant'Anna's (2021) heterogeneity-robust estimator. 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 with 1,000 replications. *** p<0.01; ** p<0.05; * p<0.10.

6 Conclusion

This paper provides evidence of the labour market effects of an unconditional cash transfer – South Africa's Social Relief of Distress grant – in a context where both the design of the transfer and the underlying economic conditions are explicitly characterised by unemployment. The transfer is distinct in South Africa in that it is the first in the country's history to make explicit use of a labour market eligibility criterion for targeting, and the first intended for working-age adults for their own consumption, thus addressing a long-lasting gap in the social safety net. We apply a staggered, heterogeneity-robust, and semi-parametric difference-in-differences design on nationally representative, panel labour force survey data conducted during 2020 and 2021 to estimate contemporaneous and dynamic effects on three primary outcomes: the probabilities of job search, trying to start a business, and employment. To our knowledge, this study is the first to provide causal evidence of the transfer's effects on any outcome.

We show that receipt of the SRD grant has positive but heterogenous effects on labour market outcomes. On average, receipt increases the probability of job search by 4.4 percentage points, trying to start a business by 4.0 percentage points, and employment by 6.0 percentage points. The latter effect is driven by effects on wage and informal sector employment, though smaller effects on self and formal sector employment are also evident. Our analysis of mechanisms suggests that these employment effects are explained by the transfer's effects on labour market constraints at both the extensive and intensive margins, but more so the latter. Specifically, employment effects are positive for both the unemployed who are already searching for work or trying to start a business, as well as for those who are not. The transfer appears to enable this latter subset of the unemployed to transition into these activities, while also improving the efficiency of these activities among those already doing so, both resulting in gains in employment prospects. However, effects are substantially (at least 2.5 times) larger for the latter group, which highlights the importance of active labour market engagement for improving employment prospects through the transfer. These findings are consistent with the presence of liquidity and insurance constraints well-documented in other contexts.

However, and importantly, the aforementioned employment effects are only evident in the short-term and quickly become and remain null for longer durations of receipt. This suggests that the transfer's short-term labour market gains do not translate into sustained longer-term benefits, at least with respect to its initial design. We argue that this is due to both the extent and nature of unemployment in South Africa. Specifically, the combination of a general severe scarcity of jobs and a largely structural nature of unemployment means that gains in a recipient's ability to search for work or start a business need not translate into better longer-term employment prospects.

Our findings are consistent with most studies which do not support a negative labour supply effect, and provide evidence of effect dynamics in a high-unemployment context and hence hold relevance for other low- and middle-income countries. In particular, they have important policy implications for the future of the SRD grant in South Africa. Our finding of a short-term, positive employment effect that does not persist into the longer-term suggests that the transfer may not be an appropriate policy tool to reduce the country's extreme level of unemployment, at least in its initial design. While this does not negate the transfer's important simulated effects on other outcomes, such as poverty, alternative or additional approaches are required if improving the functioning of the labour market is the goal. Additionally, our analysis is not without its limitations, and hence much scope exists for further research. In particular, future research ought to consider estimating the effects of later phases of the transfer. Our study is limited to the transfer's 'first phase' during a period characterised by macroeconomic and policy uncertainty and prior to the transfer's expansion in eligibility, which remained in place at the time of writing. Effects during later periods, when the transfer is considered more permanent and payments are more dependable, may differ.

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Notes

1. In addition to this income effect, such disincentives may also result from the belief that higher labour income disqualifies them from receiving benefits, hence acting as a "tax" on labour income (Banerjee et al., 2017; Salehi-Isfahani & Mostafavi-Dehzooei, 2018).

2. Unemployment defined under the broad definition. Working-age population refers to those aged 15 to 64 years.

3. The one exception is the Disability Grant which targets a small subset of low-income working-age adults: those with a temporary or permanent physical or mental disability.

4. The larger effects on both wage employment and informal sector employment is not contradictory, as we explain in the analysis to follow. While wage employment in South Africa is more common in the formal sector, and self-employment in the informal sector, wage employment is prevalent in both. As of 2020Q1, microdata from Statistics South Africa's Quarterly Labour Force Survey for the first quarter of 2020 shows that wage jobs represented 94 percent of formal sector jobs and 41 percent of informal sector jobs.

5. Throughout this paper, the terms 'cash transfers' and 'social grants' are used interchangeably.

6. The OPG used to be called the Old Age Pension.

7. In 2022, the extended CSG was additionally introduced to provide income support to primary caregivers of orphans, and was distributed to approximately 50,000 recipients as of July 2023 (South African Social Security Agency, 2023).

8. The OPG is ZAR2,090 (US$300 PPP) for individuals aged 60 to 75 years but ZAR2,110 (US$304 PPP) for those older than 75 years.

9. The discrepancy between the number of transfers distributed and number of recipients is because (i) primary caregivers receive the CSG not for their own benefit but on behalf of their eligible child(ren) and (ii) some individuals receive multiple transfers simultaneously.

10. These numbers include 7.1 million SRD grant recipients in the month. This number can, however, vary significantly from month-to-month.

11. Own calculations using microdata from Statistics South Africa's (StatsSA) 2022 General Household Survey.

12. The reader is referred to Köhler et al. (2023) for a more detailed description of the evolution of the government's lockdown policy.

13. Except for the GIA, which is a supplementary cash transfer offered to recipients of other cash transfers (either a DG, WVG, or OPG) who cannot care for themselves to pay the person who takes care of them.

14. Own calculations using microdata from StatsSA's Quarterly Labour Force Survey for the first quarter of 2020.

15. Initially, the SRD grant was conceptualised to target the informally employed, but this was not followed through due to concerns surrounding inclusion errors (Bassier et al., 2021).

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

17. Own calculations using microdata from StatsSA's Quarterly Labour Force Survey for the first quarter of 2020.

18. Own calculations using microdata from StatsSA's Quarterly Labour Force Survey for the fourth quarter of 2020.

19. The amount of the SRD grant has remained unchanged in nominal terms at ZAR350 (US$50 PPP) since its inception in 2020. Adjusted for inflation using StatsSA's Consumer Price Index data (Statistics South Africa, 2022a), this amounts to approximately ZAR280 (US$40 PPP) in February 2024 Rands, equivalent to a 20 percent reduction in purchasing power.

20. At the time of their analysis, the SRD grant had yet to be introduced. The authors define eligibility for this transfer as being aged 18 to 59 years, not being formally employed, and not receiving any other cash transfer. Additionally, they set the transfer's value at 50 percent higher than the realised policy: ZAR526 (US$75 PPP) per person per month.

21. Given that the value of the transfer used in this study is substantially higher than the realised policy, these simulated effects may be overestimated.

22. Using StatsSA"s upper-bound poverty line of ZAR1,335 (US$192 PPP) per person per month.

23. Using StatsSA's food poverty line of ZAR600 (US$86 PPP) per person per month.

24. Because of this, 621 sampled dwelling units (or 2 percent of the intended sample) were not interviewed in the 2020Q1 dataset (Statistics South Africa, 2020f). To adjust for this missing data, StatsSA used the rotational panel component of the survey and made imputations where possible using data for respondents from the previous quarter.

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

26. The anonymisation of the data prohibits one from making use of more sensitive identifiers, such as names and birth dates.

27. Using a linear probability model, we estimate a coefficient of -0.004 and standard error of 0.007 for treatment status regressed on a binary attrition variable conditional on wave fixed effects, yielding a large p-value of 0.560.

28. Race as a form of classification in South Africa is still widely used in the literature with the four largest race

groups being African/Black, Indian/Asian, Coloured (mixed-race), and White. It is important to note that this serves a functional rather than normative purpose.

29. As an alternative dataset, the NIDS-CRAM survey does include items on receipt of the SRD grant. However, use of the data for causal identification is inadequate given that the all survey waves were conducted during the post-treatment period, and Callaway & Sant'Anna's (2021) estimator requires at least one pre-treatment period for each group. Moreover, the NIDS-CRAM sample is substantially smaller, equivalent to about 15 percent of the 2020Q2 QLFS sample.

30. The term 'non-employed' here refers to the unemployed as ell as the economically inactive.

31. It should be noted that because respondents were only surveyed once per quarter, we are only able to observe receipt once per quarter. It is of course possible that recipients received the transfer multiple (up to three) times per quarter, however due to data availability we are unable to observe this.

32. Administrative data, as used in Figure 2, shows that WVG, CDG, and GIA transfers collectively represent approximately 3 percent of all transfers distributed in the post-SRD period.

33. As mentioned in the discussion to follow, we restrict our sample to the unemployed aged 18 to 59 years who do not receive any other cash transfer, apart from the SRD grant. Doing so automatically largely addresses this contamination issue. WVG recipients are excluded because eligibility is conditional on being at least 60 years old. GIA recipients are excluded because eligibility is conditional on receiving the DG or OPG, which we can observe in the data. Regarding the CDG, unfortunately we are unable to exclude potential recipients because we do not observe any data relevant to the transfer's eligibility criteria (for instance, that relating to parental status, household income, and child disability status).

34. In total, this affected just $n = 348$ observations or 0.7 percent of all observations.

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

36. The coefficient of the difference on the binary female variable is also statistically significant, but only marginally so at the 10 percent level.

37. In other words, we always use the period immediately before treatment as the baseline. As shown by Roth (2024), the default Callaway & Sant'Anna (2021) event study estimates are constructed asymmetrically for the pre- and post-treatment periods, such that the former comprise comparisons of consecutive periods (referred to as "short differences") while the latter are "long differences". This asymmetry can result in a spurious jump in the plot even if there is no treatment effect.

38. Additionally, this estimator 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 (specifically, just 4 percent of observations), we believe this to be an appropriate assumption given that it implies individuals do not "forget about their treatment experience" (transfer receipt in this case). An alternative estimator by de Chaisemartin & d'Haultfoeuille (2020) does allow for treatment to turn on and off, however only subject to a strict 'no carryover' assumption which imposes that potential outcomes only depend on current treatment status and not on full treatment histories (Roth et al., 2023). Given the possibility of dynamic and cumulative effects of transfer receipt, we believe this assumption is inappropriate in this context and thus proceed with Callaway & Sant'Anna's (2021) approach. As a robustness test in Section 5, we do however examine the sensitivity of our results to this decision.

39. Recall that because the surveys asks only the non-employed about transfer receipt, it is not possible to use a conventional binary employment variable. Instead, we obtain this measure of employment by exploiting the panel nature of the data to observe recipients' future employment outcomes.

40. Employment formality is defined as per StatsSA's 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, as well as all others who are not registered for any tax.

41. 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.

42. These groups of recipients do not significantly differ from one another across a wide range of observable demographic characteristics.

43. While we are able to obtain estimates for four post-treatment periods, for visualisation we omit estimates for the fourth post-treatment period in these plots due to a large amount of imprecision and hence large confidence intervals for the period, which affects the plot scales. For completeness, however, the plots which include these estimates are presented in Figure A2 in the appendix.

44. Indeed, job-seekers and SRD grant recipients are relatively low-skilled as measured by their highest levels of education, as shown by Bhorat & Köhler (2021) and the estimates in Table 1.

45. As in Figure 3, we are able to obtain estimates for four post-treatment periods, but unlike before, here we retain all periods due to an absence of excessively large confidence intervals which affect the plot scales. The exception is for panel (b), which again omits the fourth post-treatment period due to a large amount of imprecision. For completeness, the plot which includes the estimate for this period is presented in Figure A3 in the appendix.

46. The larger effects on both wage employment and informal sector employment is not contradictory. While wage

employment in South Africa is more common in the formal sector, and self-employment in the informal sector, wage employment is prevalent in both. As of 2020Q1, microdata from StatsSA's QLFS for 2020Q1 shows that wage jobs represented 94 percent of formal sector jobs and 41 percent of informal sector jobs.

47. As previously stated, we are able to obtain estimates for four post-treatment periods, but here we retain all periods for informal sector employment due to an absence of excessively large confidence intervals which affect the plot scales, but not informal sector employment. For completeness, the plot which includes the estimate for this period and outcome is presented in Figure A4 in the appendix.

48. It was not possible to produce estimates for the respective 2020Q2 groups in panels (a) and (b) due to an inadequate sample size ($n = 182$), which may reflect the difficulty in trying to start a business during a period characterised by stringent pandemic-related restrictions.

49. The large confidence intervals for the sample of unemployed who tried to start a business is a consequence of the group's relatively small sample size as indicated in Table 5.

7 Appendix

Table A1: Sample sizes before and after sample restrictions

Wave	Complete QLFS sample (n)	After primary sample restrictions: 18-59 years, no UI benefits, no other cash transfers (n)	After secondary sample restriction: consistent unbalanced panel (n)
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

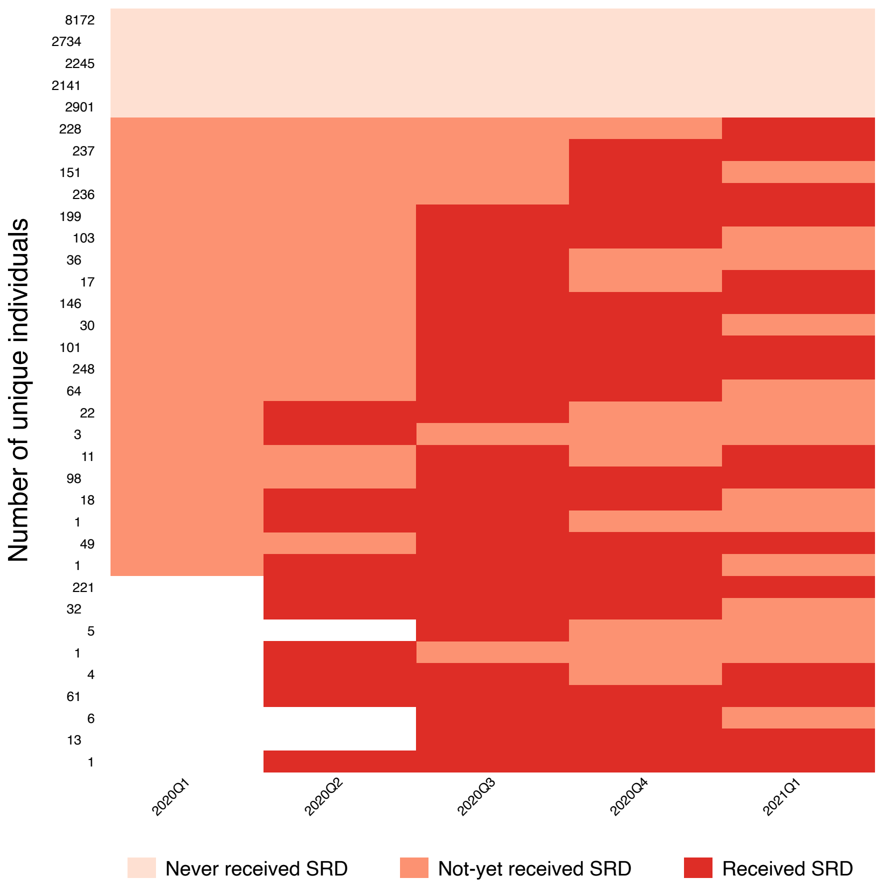
[a] Author's own calculations. Source: QLFS 2020Q1 - 2021Q1 (Statistics South Africa, 2020a,b,c,d,e).
[b] Notes: UI = Unemployment Insurance.

Table A2: Covariate balance before and after sample restrictions at baseline

	Before sample restrictions			After sample restrictions		
	Never received SRD (1)	Ever received SRD (2)	Diff. (3)=(2)-(1)	Never received SRD (4)	Ever received SRD (5)	Diff. (6)=(5)-(4)
Age (years)	29.695	33.196	3.501***	31.143	31.659	0.516
	(0.086)	(0.263)	(0.277)	(0.116)	(0.293)	(0.315)
Female	0.515	0.406	-0.109***	0.486	0.309	-0.177***
	(0.002)	(0.010)	(0.011)	(0.005)	(0.013)	(0.014)
African/Black	0.805	0.939	0.134***	0.838	0.938	0.100***
	(0.002)	(0.005)	(0.006)	(0.004)	(0.007)	(0.008)
Married	0.264	0.184	-0.080***	0.250	0.153	-0.097***
	(0.002)	(0.008)	(0.008)	(0.004)	(0.010)	(0.011)
Urban	0.657	0.533	-0.124***	0.681	0.528	-0.153***
	(0.002)	(0.011)	(0.011)	(0.005)	(0.014)	(0.015)
Tertiary education	0.092	0.056	-0.036***	0.078	0.051	-0.027***
	(0.001)	(0.005)	(0.005)	(0.003)	(0.006)	(0.007)

[a] Author's own calculations. Source: QLFS 2020Q1 (Statistics South Africa, 2020a).
[b] Notes: Treatment defined as receipt of the SRD grant as identified through the use of the 'other' transfer variable 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 panel (individual) level. The magnitude and statistical significance of a given difference (Diff.) is estimated using a t-test. *** p<0.01, ** p<0.05, * p<0.10.

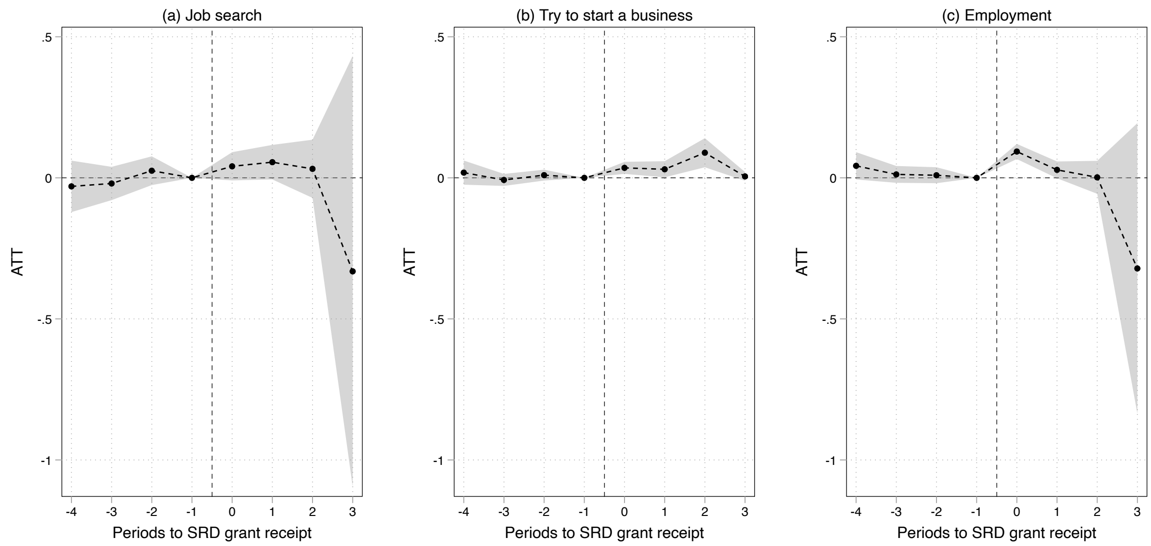
Figure A1: Heatmap of staggered SRD grant receipt rollout across the study period



[a] Author's own calculations. Source: QLFS 2020Q1 - 2021Q1 (Statistics South Africa, 2020a,b,c,d,e).

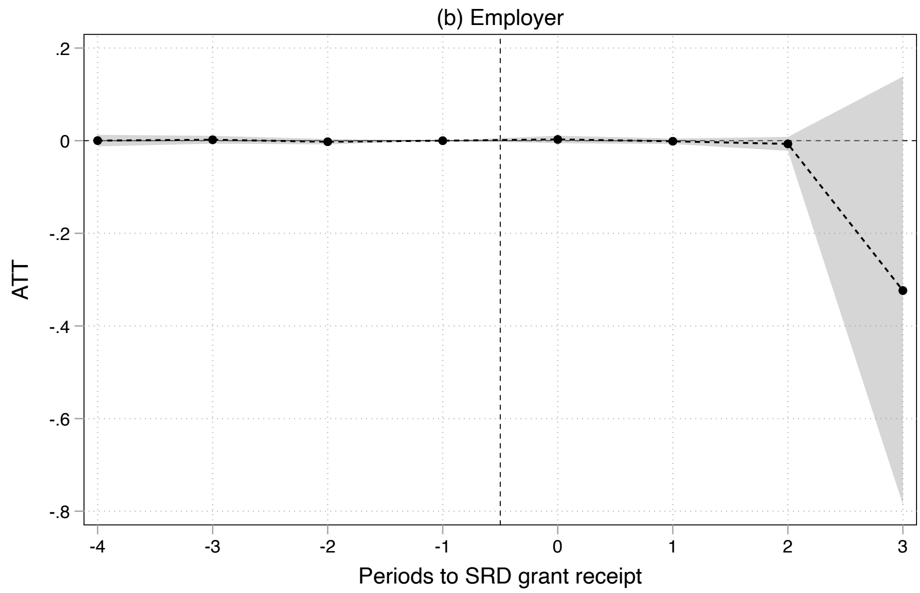
[b] Notes: SRD grant receipt (treatment) ordered by treatment timing. Only unique treatment histories plotted, with the number of individuals experiencing each treatment history presented on the y-axis. White spaces indicate missing data.

Figure A2: Event study estimates of the dynamic labour market effects of SRD grant receipt, retaining all post-treatment periods



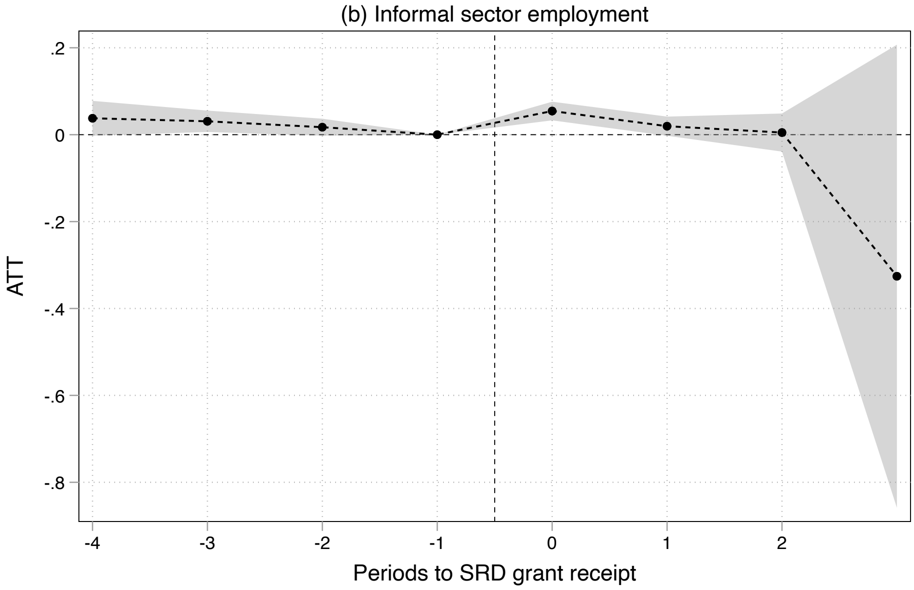
[a] Author's own calculations. Source: QLFS 2020Q1 - 2021Q1 (Statistics South Africa, 2020a,b,c,d,e).
[b] Notes: Models estimated using Callaway & Sant'Anna's (2021) heterogeneity-robust difference-in-differences estimator and Sant'Anna & Zhao's (2020) doubly robust estimand. ATT's are estimated for each period relative to the first-treatment period, across all first-treatment cohorts. Periods to SRD grant receipt indexes the length of receipt. Estimates are weighted using sampling weights. Standard errors are clustered at the panel (individual) level and are estimated using a multiplicative wild bootstrap procedure with 1,000 replications. Shaded areas represent 95 percent confidence intervals.

Figure A3: Event study estimates of the dynamic effects of SRD grant receipt on the probability of being an employer, retaining all post-treatment periods



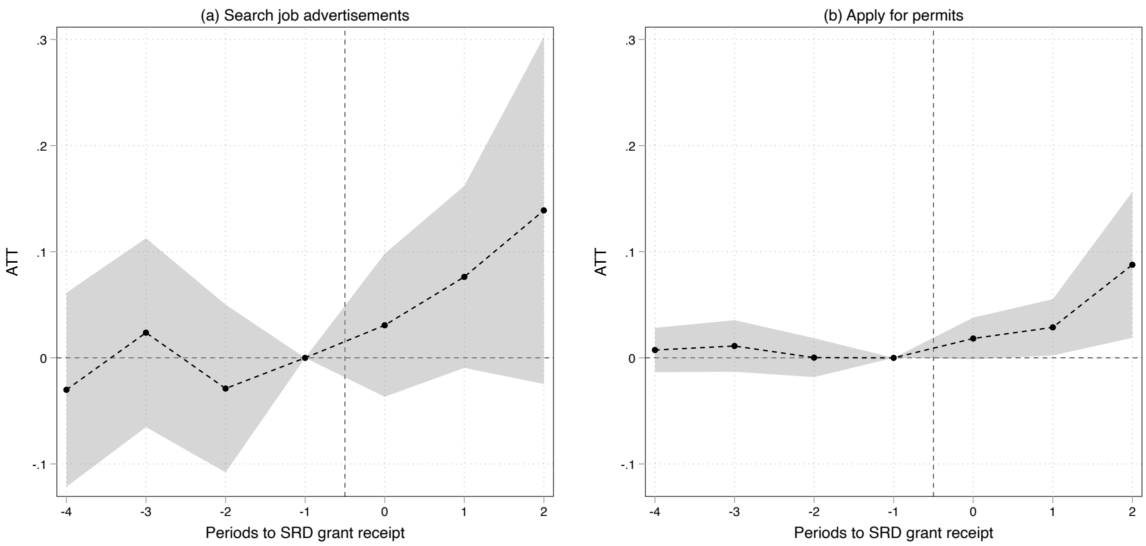
[a] Author's own calculations. Source: QLFS 2020Q1 - 2021Q1 (Statistics South Africa, 2020a,b,c,d,e).
[b] Notes: Models estimated using Callaway & Sant'Anna's (2021) heterogeneity-robust difference-in-differences estimator and Sant'Anna & Zhao's (2020) doubly robust estimand. ATT's are estimated for each period relative to the first-treatment period, across all first-treatment cohorts. Periods to SRD grant receipt indexes the length of receipt. Estimates are weighted using sampling weights. Standard errors are clustered at the panel (individual) level and are estimated using a multiplicative wild bootstrap procedure with 1,000 replications. Shaded areas represent 95 percent confidence intervals.

Figure A4: Event study estimates of the dynamic effects of SRD grant receipt on the probability of informal sector employment, retaining all post-treatment periods



[a] Author's own calculations. Source: QLFS 2020Q1 - 2021Q1 (Statistics South Africa, 2020a,b,c,d,e).
[b] Notes: Models estimated using Callaway & Sant'Anna's (2021) heterogeneity-robust difference-in-differences estimator and Sant'Anna & Zhao's (2020) doubly robust estimand. ATT's are estimated for each period relative to the first-treatment period, across all first-treatment cohorts. Periods to SRD grant receipt indexes the length of receipt. Estimates are weighted using sampling weights. Standard errors are clustered at the panel (individual) level and are estimated using a multiplicative wild bootstrap procedure with 1,000 replications. Shaded areas represent 95 percent confidence intervals.

Figure A5: Event study estimates of the dynamic effects of SRD grant receipt on intensive margin behaviour



[a] Author's own calculations. Source: QLFS 2020Q1 - 2021Q1 (Statistics South Africa, 2020a,b,c,d,e).
[b] Notes: Models estimated using Callaway & Sant'Anna's (2021) heterogeneity-robust difference-in-differences estimator and Sant'Anna & Zhao's (2020) doubly robust estimand. ATT's are estimated for each period relative to the first-treatment period, across all first-treatment cohorts. Periods to SRD grant receipt indexes the length of receipt. Estimates are weighted using sampling weights. Standard errors are clustered at the panel (individual) level and are estimated using a multiplicative wild bootstrap procedure with 1,000 replications. Shaded areas represent 95 percent confidence intervals.

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