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ORIGINAL ARTICLE

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Abstract
We estimate COVID-19-related employment and poverty impacts in South Africa. We observe a 40% decline in active employment between February and April 2020, half of which was composed of job terminations rather than furloughs. Initially, vulnerable groups were disproportionately affected by the labour market shock. Exploiting the dataset's panel dimension and comparing lockdown incomes of job losers to reweighted job retainers, we estimate that approximately 15%–35% of job losers fell into poverty in April. We find evidence of a limited recovery in the labour market and a decrease in poverty by June, in part attributable to expanded emergency social assistance.

KEYWORDS
COVID-19, labour markets, poverty, social protection, unemployment

JEL CLASSIFICATION
J21, J48, J63, J68, I32, I38, H84

1 | INTRODUCTION

The first countries in which detailed labour market data became available during the COVID-19 pandemic—primarily in Western Europe and North America—recorded historic labour market contractions (Adams-Prassl et al., 2020; Alon et al., 2020; Bartik et al., 2020; Chetty et al., 2020; Coibion et al., 2020; Hassan et al., 2020). However, these were also the countries that introduced the most comprehensive set of social protection relief measures. It is widely understood that the poverty impacts of the COVID-19 pandemic were most severe in emerging market economies (Decerf et al., 2021; Mahler et al., 2020; Sumner et al., 2020).

Bassier, Budlender and Zizzamia contributed equally to this work.

We investigate two empirical questions: (Adams-Prassl et al., 2020) What were the initial labour market effects of COVID-19 in South Africa? and (Alon et al., 2020) How did this labour market shock affect welfare? We use the National Income Dynamics Study: Coronavirus Rapid Mobile Survey (NID-SCRAM) to present the first estimates of the impact of COVID-19 on employment and welfare outcomes in South Africa.[1] NIDS-CRAM is a stratified 30% subsample of adults in the 2017 wave of the National Income Dynamics Study (NIDS), which in turn is a nationally representative longitudinal household survey.

We find that after 1 month of intensive lockdown, active employment decreased by 40%. In nearly half of these cases, workers did not expect to return to their jobs. The pattern of job loss severely exacerbated existing inequalities, and we estimate a substantial increase in poverty for job losers. Following the same respondents in a second wave of data collection, we find evidence of a limited recovery in the labour market and a decrease in poverty by June. However, active employment was still 20% lower in June than February, mostly due to job terminations that persisted into June.

To investigate the labour market impacts of COVID-19, we introduce a new employment status typology that distinguishes between the 'not employed', the 'temporarily laid off', those on 'paid leave' and the 'actively employed'. Using this categorisation, we measure the effects of COVID-19 on employment by comparing net changes in employment status from February (before South Africa's first confirmed COVID-19 case on 5 March) to April 2020 (following the imposition of a stringent lockdown on 26 March).

Of the 40% net decline that we observe in active employment between February and April, almost half is accounted for by increases in nonemployment as opposed to temporary layoffs or paid leave. By June, most of this increase in nonemployment had persisted, while approximately 40% of those temporarily laid off in April had fallen into nonemployment in June. Most of those on paid leave in April had returned to active employment by June.

Women, those with lower levels of education, those in manual occupations, informal workers and the poor faced the greatest net employment losses. These employment losses were disproportionately made up of shifts into nonemployment, with the result that the persistence into June of the labour market shock disproportionately affected these groups. For individuals who remained actively employed in April, we observe no statistically detectable change in earnings, while for those who transitioned from actively employed to paid leave, we see a 5% decrease in earnings on average.

To estimate the poverty effect of February to April job loss, we compare April incomes of job losers to the incomes of those who remain actively employed over this period, since we do not observe household income prelockdown. To address selection issues, we reweight the household income distribution of job retainers as per DiNardo et al. (1996) or DiNardo-Fortin-Lemieux (DFL), after using a logit Least Absolute Shrinkage and Selection Operator (LASSO) regression to construct a job loss propensity score from a rich set of 2017 individual-level characteristics. We undertake a comprehensive set of diagnostic and robustness tests which suggest that our reweighting procedure works sufficiently well to provide approximate estimates.

While noting that this inferential exercise is unavoidably approximate, we estimate that by April, between 15% and 35% of job losers—approximately 0.8 to 1.8 million individuals—fell into poverty as a result of COVID-19-related job loss, depending on the poverty line used.[2] Using a rough estimate of the dependency ratio of job losers, we speculate that this job loss is associated with an increase in overall poverty of between 2.6 and 5.8 million people including dependents between February and April.[3]

[1]This paper is a consolidation of two earlier working papers, which were based on the first and second waves of NIDS-CRAM data, respectively. These earlier versions provided the first labour market and welfare estimates of the COVID-19 shock in South Africa. Since the publication of the working paper versions, a number of studies have replicated and extended our labour market results (see, e.g., Köhler, Bhorat et al., 2021), while Barnes et al. (2021) have presented poverty estimates using microsimulation methods.

[2]This is 5% to 11% of the total actively employed in February. Accounting for job finders (18% of gross job loss), we estimate an increase in poverty due to *net* job transitions of approximately 0.6 to 1.5 million people or 4% to 9% of the actively employed in February. See Section 6.

[3]This is 4% to 10% of the total population in South Africa. Accounting for job finders as above, the comparable figure is approximately 1.9 to 4.8 million people or 3% to 8% of the total population.

Using the second wave of NIDS-CRAM data, we can directly compare April incomes to June incomes for the same individuals. Doing so, we estimate that overall poverty rates decreased by approximately 3 to 6 percentage points over this period, depending on the poverty line used. Since direct comparisons of February to April incomes were not possible given the lack of household income data collected retrospectively for February, we cannot say how much this overall April to June decrease in poverty compensated for the February to April increase.

Part of the explanation for the large poverty impacts between February and April was the relatively low rate of social protection coverage at this point. The South African government had introduced only emergency social *insurance* relief measures by April but not yet the social *assistance* measures which were later introduced in May. The implementation of emergency social insurance was also imperfect: We find that only 20% of those workers who moved from being actively employed in February into paid leave or temporary unemployment in April received payouts from the Temporary Employee/Employer Relief Scheme (TERS), the emergency social insurance scheme intended to support them.[4] While pre-existing social assistance measures progressively provide broader coverage, 30% of those who transitioned into nonemployment reported no household-level grant protection.

Based on a counterfactual exercise, we suggest that part of the poverty recovery which we observe in the second wave of NIDS-CRAM data can be attributed to the expansion of social assistance measures which were implemented from May onward, with an important role being played by the introduction of the new social relief of distress (SRD) grant.

Our paper contributes to the emerging cross-country literature on the economic impact of COVID-19. Our paper relates to two strands of this literature in particular. The first is the burgeoning literature which uses new data to provide evidence on the labour market impacts of the COVID-19 shock. This evidence has pointed to large decreases in active employment, with those worst affected being women, low-income workers, the self-employed and those working variable hours (Adams-Prassl et al., 2020; Alon et al., 2020; Barrero et al., 2020, 2021; Bartik et al., 2020; Béland et al., 2023; Bui et al., 2020; Chetty et al., 2020; Coibion et al., 2020; Cowan, 2020). This literature has largely focused on labour market impacts in rich countries. The second strand of this literature has concentrated on the poverty impacts of the COVID-19 pandemic, but because of the lack of real-time data on welfare indicators, this literature has primarily consisted of poverty *projections* (Bassier, Budlender, Zizzamia, et al., 2021; Bhalla et al., 2022; Decerf et al., 2021; Kartseva & Kuznetsova, 2020; Mahler et al., 2020; Parolin & Wimer, 2020; Sumner et al., 2020). Most of these projections have been for developing countries, where the economic consequences of the COVID-19 labour market shock are expected to have been most severe (Gerard et al., 2020; International Labour Organisation, 2020; Kerr & Thornton, 2020b).

We provide some of the first estimates of the labour market impact of COVID-19 in a developing country using data that are representative of the substantial majority of the population (over-15 year olds in 2017). To the best of our knowledge, our paper is also the first to use new data to directly estimate—rather than project—the impact of the COVID-19 labour market shock on poverty in South Africa. In addition, we include an assessment of COVID-19 social protection coverage, contributing to a literature documenting these interventions (Gentilini et al., 2022) and assessing their performance (Rothwell, 2020). An important advantage of our analysis is the longitudinal survey data we use, which links two waves of NIDS-CRAM respondents back to their records in earlier NIDS waves, allowing us to follow individuals from before the pandemic, into the first 'lockdown' phase of the pandemic and finally during a later period of partial economic re-opening. In addition to exploiting this in our estimation strategy, this also allows us to assess the representativity of NIDS-CRAM compared with the nationally representative NIDS (for the 2017 population), unlike other recent rapid surveys.

The paper is structured as follows: Section 2 describes our data. Section 3 reports labour market effects over lockdown. Section 4 presents our estimates of the COVID19 poverty impact and our

[4]TERS is an earnings relief benefit for employers unable to pay their employees due to the COVID-19 lockdown (Department of Labour, Republic of South Africa, 2020).

empirical methodology. Section 5 provides evidence on social protection coverage. Section 6 discusses the implications of our poverty results for the NIDS-CRAM sample population-level poverty impacts, and considers counterfactual policy scenarios. Section 7 concludes.

2 | DATA

This paper uses data from the first two waves of the NIDS-CRAM panel (SALDRU, 2020) and NIDS (SALDRU, 2017). NIDS-CRAM is a computer-assisted telephone interviewing (CATI) survey, with the first wave conducted in May and June 2020 and the second wave conducted in July and August 2020. In Wave 1, respondents were mainly asked retrospective questions about their circumstances in February and April, while in Wave 2, respondents were mainly asked retrospective questions about June.[5]

The NIDS-CRAM sampling frame is the NIDS Wave 5 sample, limited to those aged 18 years or older at the time of NIDS-CRAM data collection.[6] The sample selected (based on age-eligibility criteria) for the NIDS-CRAM mobile phone survey was 17,568 out of approximately 30,000 individuals in NIDS Wave 5. The NIDS-CRAM response rate was approximately 40%, yielding a final sample of 7074 individuals.[7]

The representativity of the NIDS-CRAM sample is discussed at length by Ingle et al. (2020), as it is not straightforward to interpret.[8] NIDS-CRAM was based on a survey which was representative of the South African population in 2017 and therefore may be interpreted as representative of the South Africa population in 2017 who were at that point at least 15 years old. However, demographic changes between 2017 and 2020 means that users cannot claim that NIDS-CRAM is representative of the South African population in 2020. For instance, there may be systematic undersampling of certain demographic groups which have grown since 2017 (possibly migrants, for instance) and oversampling of other groups. Moreover, as discussed above and in the supporting information Appendix A, any differential nonresponse not accounted for by the poststratification weights would further impede even this interpretation of the representativity of the sample of the over-15 year olds in 2017 South African population. When inferences are drawn from the NIDS-CRAM sample in the analysis, they should be interpreted as being representative of the South African population in this qualified sense.

To help assess the correspondence between NIDS-CRAM and NIDS Wave 5 (which is representative of the 2017 South African population), supporting information Appendix Table A1 shows good balance of raw sample statistics across most categories for NIDS-CRAM Wave 1 and NIDS Wave 5. Middle-aged individuals were intentionally oversampled for better precision of labour market statistics—this is corrected by using weights when estimating population-level statistics.[9]

[5]Relying on retrospective data does introduce a source of potential noise and/or recall bias, a problem common to labour force surveys (Paull, 2002). Noise affects the precision of estimates, while bias may be more concerning. While we cannot test for or correct recall bias, we are reassured that data for all periods (February, April and June) are all retrospective. If recall bias has similar effects across the different periods, then the bias would be in the same direction for all three periods; more generally, however, this will be a source of bias in our estimates.

[6]Refer to supporting information for details on sampling, response rates and comparability to previous waves.

[7]The vast majority of nonresponse was from adults who were no longer reachable on the phone number provided in 2017, as opposed to refusals which accounted for approximately 8% of nonresponse (Ingle et al., 2020). In earlier waves of NIDS, which were roughly 2 years apart, between-wave attrition was between 20% and 30% and the refusal rate was around 3% (Branson, 2018).

[8]At the time the NIDS-CRAM survey was conducted, the NIDS Wave 5 sample was the best sampling frame available and had the advantage of allowing researchers to link individuals in the NIDS-CRAM data to the same individuals in the pre-COVID NIDS waves. The NIDS Wave 5 sample was representative of the South African adult population in 2017 (achieved through a top-up to the original 2008 sample). While NIDS-CRAM is not strictly nationally representative in 2020, it is nevertheless the dataset which most closely approximates representativity for this period. Using NIDS Wave 5 as a sampling frame necessitated a trade-off between, on the one hand, these representativity issues and, on the other hand, the advantage of being able to link respondents in NIDS-CRAM back to NIDS and the rich set of NIDS covariates. The analysis of NIDS-CRAM data ought to be interpreted with this trade-off in mind.

[9]While the use of weights is straightforward for many of the descriptive statistics we present in this paper, the question of whether to apply sampling weights in regression analysis is often not straightforward. Solon et al. (2015) provide guidance. They suggest that a leading case for weighted regression analyses is when endogenous sampling is used. Age would be endogenous for all our work- and income-related outcomes of interest, and so because of the NIDS-CRAM oversampling of middle-aged individuals, all of our regressions apply the relevant design weights.

Supporting information Figure A1 also shows that the distributions of 2017 NIDS Wave 5 log household per capita incomes are very similar for those that were and were not successfully interviewed in NIDS-CRAM.

We use the employment, earnings and household-level economic outcomes data from NIDS-CRAM and a broad range of (longitudinally linked) 2017 NIDS variables. We restrict our sample to adults aged between 18 and 64 in 2020.

Unlike Statistics South Africa's Quarterly Labour Force Survey, the other source of data on the pandemic labour market, we observe individual and household-level grant receipt in NIDS-CRAM as well as household income, allowing us to explore the relationship between the labour market, poverty and social protection.

In the social protection module, respondents are asked directly about receipt of the child support grant (CSG), the old age pension and the 'special COVID-19 relief from distress grant' (in those words). These are asked separately by grant, separately for household vs individual receipt and repeatedly to allow for respondents who do not know the number of such grants received. In this paper, we also consider receipt of 'the UIF's Temporary Employer/Employee Relief Scheme (TERS)', which is asked about directly in the labour market module (in those words), for both employers and employees. Various measures were put in place which would have made TERS receipt by employees more transparent, such as that the UIF made public the updated list of employers receiving TERS and (by August) that the UIF paid the TERS amount into employees' bank accounts directly (Köhler & Hill, 2022). However, insofar as employee-respondents were not aware of receiving the grant, we will underestimate TERS receipt.

3 | LABOUR MARKET IMPACTS

3.1 | Employment status definition

Standard employment definitions divide the working-age population into the 'Not economically active', 'Searching unemployed', 'Discouraged unemployed' and 'Employed' (International Labour Organisation, 2013). This categorisation is used in the construction of internationally comparable labour market statistics. However, a straightforward application of this categorisation is prone to overlooking several important features of the COVID-19 labour market. In a context where in-person economic activity has largely ground to a halt and workers have been sent home from their workplaces, substantive labour market dynamics occur both *across* the standard employment categories (*i.e.* employment to unemployment transitions) and also *within* them (*i.e.* among the employed, transitions from actively working to paid leave or being temporarily laid off). In the COVID-19 context, it is also possible that survey respondents themselves were unsure of their employment status and that their reported workdays and compensation are a more meaningful indicator of their employment status.

We therefore develop and implement a new employment typology for better understanding of the COVID-19 labour market, using the following mutually exclusive employment categories:

1. *Active employment*: Engages in economic activity for profit or pay (reports positive workdays).
2. *Paid leave*: Reports an active employment relationship and earns labour income, but works zero days.
3. *Temporary layoff*: Reports an active employment relationship or job to return to, but works zero days and reports zero earnings.
4. *Not employed*: Not engaging in any economic activity for pay or profit, whether willing to accept work or not.

Ideally, we would have liked to distinguish between the 'not economically active' and the 'unemployed' who desire (and may search for) a job. However, while this is possible in NIDS-CRAM for April employment status, respondents were not asked to retrospectively report on willingness to work and job search activity in February. To maintain comparability between February and April, we collapse the unemployed and not economically active into the broader 'Not employed' group.[10]

3.2 | Employment

In Figure 1, we depict changes (across February, April and June) in the *aggregate* percentage of adults in our four labour market states. The first three groups of bars show the proportion of adults in each labour market category as of February, April and June, while the last three groups show net changes in these proportions between February–April, April–June and February–June.

Between February and April, we find a 21 percentage point net decline in active employment as a share of the NIDS-CRAM sample working-age population, while there is a 9 percentage point net increase in nonemployment, a 7 percentage point net increase in paid leave and a 4 percentage point net increase in temporary layoffs (Figure 1).[11] Just under half of the active employment decline is therefore attributable to increases in severed employment relationships rather than temporary unemployment.[12]

With the second wave of NIDS-CRAM data, we observe that between April and June, there was an 11 percentage point gain in net active employment (Figure 1). This partial recovery came almost entirely from a reduction in the percentage of adults in the 'paid leave' (6 percentage point decline) and 'temporary layoff' (5 percentage point decline) categories. There was little change in levels of nonemployment between April and June (1 percentage point decline). Thus, to the extent that there was a recovery, this appears to have benefited primarily those who maintained a live employment relationship.

Taken together, compared with the prelockdown period in February, by June 2020, net active employment declined by 10 percentage points (equivalently, by 20% of the active employment rate in February), while rates of nonemployment have increased by 8 percentage points among working-age adults. Note that using more conventional employment categories to try to match the labour market concepts used by Statistics South Africa, we find a very similar estimate to the employment decline found in the QLFS between February and June.[13] The employment categories that we employ provide us with the additional insight that the growth of the proportion of working-age adults in 'paid leave' and 'temporary layoff' categories, which was observed in April, has disappeared.

Our main results are not exactly comparable to the existing COVID-19 literature because we present *net* changes in employment status, rather than gross employment losses. We believe this is a preferable specification because COVID-19 is likely a labour reallocation shock leading to job creation as well as job destruction (Barrero et al., 2020, 2021) and also because we wish to account for the generally high rates of labour market churning evident in South Africa (Kerr, 2018), which have remained evident over the pandemic (Espi-Sanchis et al., 2022). Results for gross employment losses, as opposed to net changes, are presented in the supporting information Appendix B. While active employment unsurprisingly

[10]Note that this employment typology was developed to represent what we believed to be the most appropriate framework for understanding the South African labour market during the COVID-19 lockdown, given the particular features of the South African lockdown and policy regime, and the features of the NIDS-CRAM data. Others studying the employment effects of COVID-19 in South Africa, but relying on QLFS data, have used other employment definitions Köhler et al. (2021).

[11]In a context in which COVID-19 social protection (discussed later) was linked to employment status and earnings, there may be some concern that intentional misreporting may lead to an overstatement of the employment effects of the pandemic shock. We expect that systematic misreporting due to these eligibility requirements is a not major concern. NIDS-CRAM is a panel of individuals who have been surveyed for NIDS prior to the pandemic, many of them since 2008. NIDS was an in-person survey, and the repeated visits prior to 2020 built trust in the independence of the survey and the confidentiality with which data were treated. NIDS-CRAM enumerators were also instructed to stress to interviewees that their responses are confidential.

[12]Throughout, we make the implicit assumption that changes over the February–April period were substantially due to the COVID-19 shock. We do not attempt to decompose this 'COVID-19 effect' into the effect due to general pandemic conditions vs that due to the lockdown.

[13]If paid leave and (optionally) temporary layoffs are included in the 'employment' category, we find an approximately 15% decline in employment between February and June—which is very similar to the 14% decline that we observe in Statistics South Africa QLFS data between the first quarter and the second quarter of 2020. See supporting information Appendix Figure D1. While NIDS-CRAM and the QLFS seemed to match fairly well in these first and second waves of NIDS-CRAM, this broke down in the third wave for unknown reasons. See Bassier, Budlender, and Kerr (2021) for discussion.

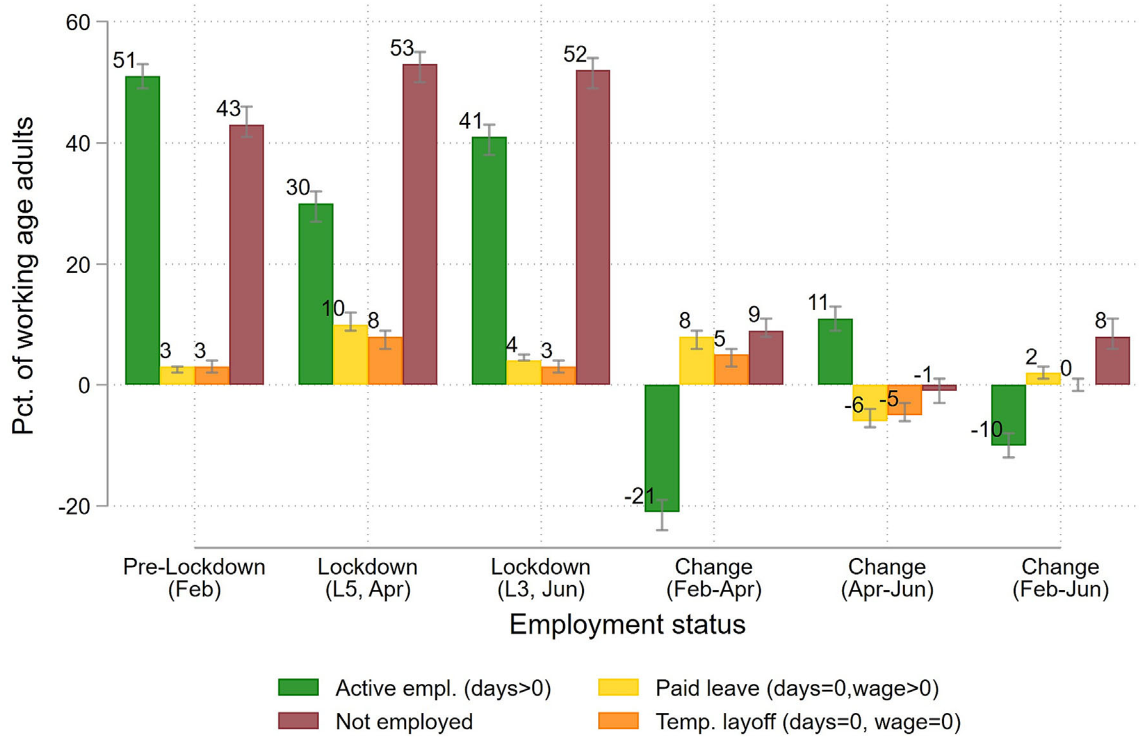

FIGURE 1 Employment status for working-age adults (18–64 years) over three periods and change over these periods. *Notes*: The figure shows the percentage of adults in the following employment status categories: 'active employment', 'PAID leave', 'temporary layoff' and 'not employed'. The three groups of bars on the left indicate the proportion of adults by their employment status in different periods: for February (before the lockdown), for April (during the lockdown) and for June (following a relaxation of lockdown measures). The groups of bars on the right show the corresponding net change (in percentage points) in working-age adults in each category (accounting for both inflows and outflows) for February to April, April to June and February to June. Note that the 'not employed' category includes those not economically active. We use a balanced NIDS-CRAM Wave 1 and Wave 2 panels, and standard errors are clustered and stratified following the survey design.

decreases more dramatically according to this specification (the *net* decrease in active employment is 85% of the *gross* decrease), our main conclusions remain substantively unchanged.

Consistent with evidence in developed-country contexts (Adams-Prassl et al., 2020; Alon et al., 2020; Cowan, 2020), the labour market impact of the COVID-19 shock has disproportionately affected women, manual workers and the poor. Figure 2 reports the heterogeneity in the employment changes between February and April across various categories of workers. The equivalent figure for the April to June period can be found in the supporting information Appendix (Figure B2).[14]

Figure 2 shows that women saw a 49% reduction in active employment over the February–April period. This is 15 percentage points greater than for men, and over half of women's net employment loss is attributable to severed employment relationships, compared with one third for men.[15] The disparities are even starker between occupation categories: Manual workers experienced a 50% net decline in active employment, which is 30 percentage points greater than professionals.[16] Whereas only one sixth of the net employment losses are constituted by job severing for professionals, half of the active employment decreases for manual workers are made up of shifts into the 'nonemployed' category. These results regarding occupation and gender are related: Women are much more likely to be in manual work relative to men.

Those in the NIDS-CRAM sample who were at the bottom half of the income distribution in 2017 have seen a 51% net decrease in active employment over the February–April period, and over half of that is a shift towards nonemployment.[17] Similarly, those with tertiary education fare much better than those without. As expected, there is a much smaller net shift into nonemployment for those who reported having a written contract in 2017 compared with those reporting a verbal contract—reflecting different impacts on formal and informal workers.[18] These patterns are consistent with, for example, the kinds of jobs one may expect are more affected by COVID-related safety restrictions, and the possibilities of working from home, as discussed by Kerr and Thornton (2020a).

In the supporting information Appendix B, we show that the differences in the size of the February–April net active employment loss between these groups (*e.g.* women vs men) are statistically significant at the 95% level, except for the tertiary vs nontertiary comparison (Figure B2a). When examining February–April changes in the proportion of adults with labour income (*i.e.* actively employed or on paid leave) or with any job (*i.e.* actively employed or on paid leave or temporarily laid off), the differences between groups are always statistically significant. Similar to Ranchhod and Daniels (2021), we also find significant racial disparities in active employment loss, with Black workers faring much worse than Whites (Figure B2a). We do not report this racial breakdown in Figure B1 because while the differences between Black and White workers are statistically significant, confidence intervals are large. This is unsurprising because White workers are only 3.7% of our sample.

3.3 | Individual employment transitions, April to June

In Figure 3, we illustrate how *individuals* have transitioned between labour market states between April and June 2020. The four groups of bars are organised according to individuals' labour market status in April, with the differently coloured bars then showing the proportion of this group by labour market status in June. For example, the first group of bars show that 79% of those who were actively employed in April remained actively employed in June, while 15% of the April actively employed were not employed in June.

[14]Heterogeneity in patterns of employment changes between April and June is broadly consistent with those between February and April. For clarity of exposition, we discuss only the February to April impacts here.

[15]Casale and Posel (2021) further discuss the disproportionate effect of the pandemic on women in South Africa.

[16]See supporting information Appendix A for details on these occupational classifications.

[17]We use 2017 household income because household income is not asked for February 2020.

[18]We use 2017 contract status because this question is not asked for February 2020.

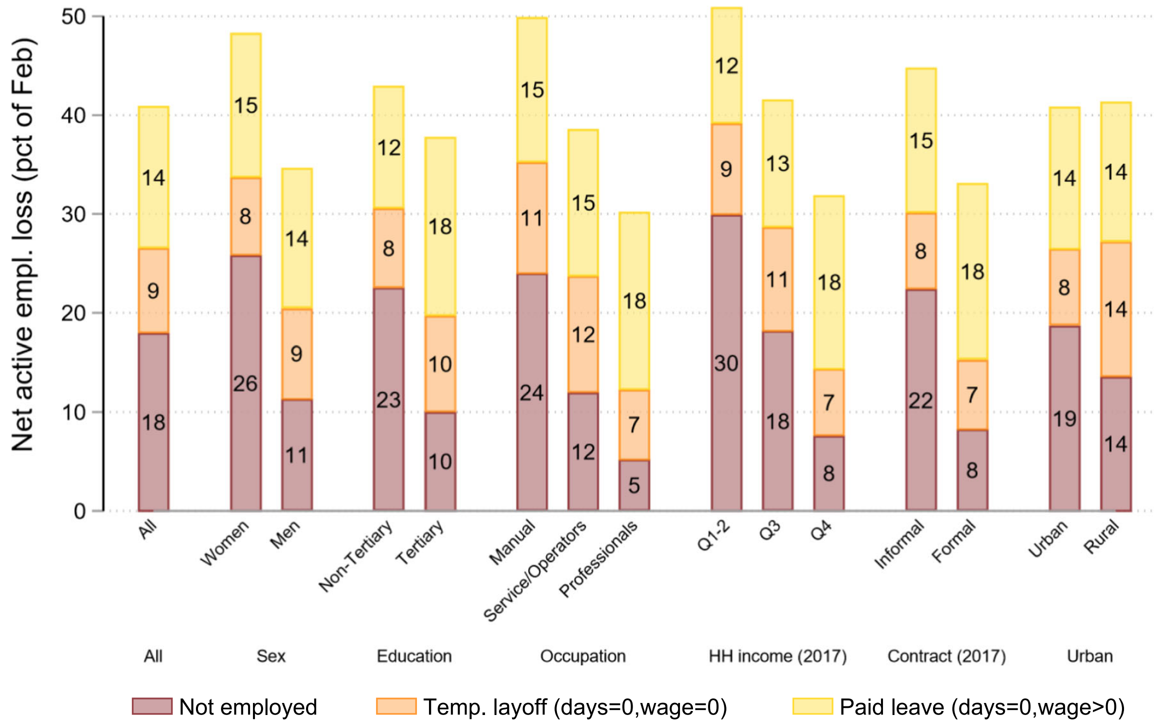

FIGURE 2 Composition of net active employment loss from February to April by worker. Characteristics. *Notes*: Panel (a) shows the percentage of adults in the following employment status categories: 'active employment', 'paid leave', 'temporary layoff' and 'not employed'. The left group of bars indicates the proportion of adults by their employment status for February (before the lockdown), while the middle group of bars shows the same for April (during the lockdown). The right-most group of bars shows the net change (in percentage points) in jobs in each category between February and April (accounting for both inflows and outflows). Note that the 'not employed' category includes those not economically active. Standard errors are clustered and stratified following the survey design. In Panel (b), the sum total in each bar is the net *percent* decrease in the total number of people who were actively employed in February, and each sub-bar decomposes this by shifts into categories of 'paid leave', 'temporary layoff' and 'not employed'. All estimates are weighted using the survey design weights.

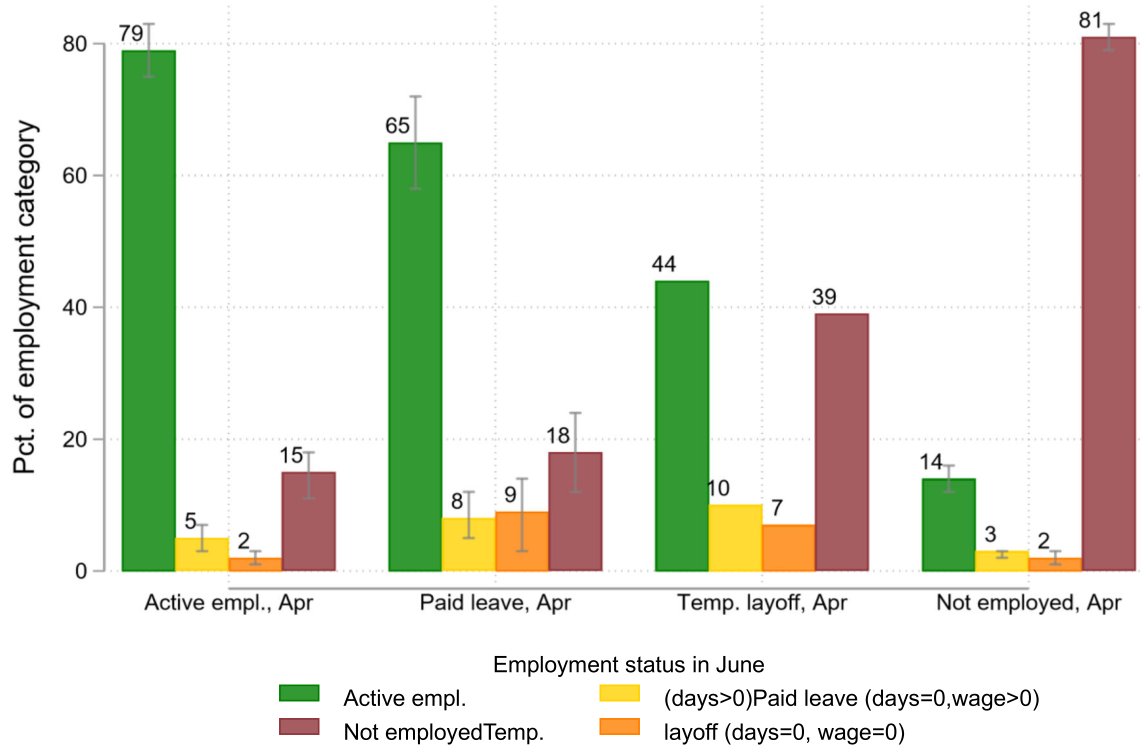

FIGURE 3 Individual employment transitions from April to June. *Notes*: The figure shows the job status transition patterns of individuals who in April were in one of the following employment status categories: 'active employment', 'paid leave', 'temporary layoff' and 'not employed' (left to right groups across the figure). For each group, employment status categories in *June* are represented by the coloured bars. Within each group, coloured bars sum to 100. While Figure 2 shows the *net* change in employment, this figure shows employment transitions for individuals. We use a balanced NIDS-CRAM Wave 1 and Wave 2 panels, and estimates are weighted using the survey design weights.

Looking at these individual job status transitions, we find that the majority of those on paid leave in April moved back into active employment (65 percent), while only 8% remained on paid leave and 18% transitioned into nonemployment. For workers who reported that they were temporarily laid off in April, less than half returned to active employment by June, and almost 40% transitioned into non-employment. This highlights that those who were 'temporarily' laid off without pay during the lockdown were in a precarious position in the labour market and were more than twice as likely to lose their jobs completely compared with those who were put on paid leave.

The left-most and right-most sets of bars show that active employment and nonemployment, respectively, were the least volatile categories between April and June: In both cases, approximately 80% of workers remained in the same category between April and June. Transitions out of these categories likely reflect a combination of usual labour market churn as well as COVID-related causes, such as sectoral labour reallocation and business failures.

3.4 | Earnings

How did average earnings change? With regard to unconditional earnings changes (nonemployment and temporary layoffs are coded as zero wages), we find that, in line with large scale job losses, average earnings declined substantially—by 10%—between February and April.

Looking at changes within individuals who remained actively employed in February and April, Figure 4 shows no statistically detectable change in wages on average.[19] In contrast, we observe a statistically significant decrease in wages of 4.5% on average for individuals who transition from active employment to paid leave. However, given wide confidence intervals, we cannot reject the possibility that those who stayed actively employed had the same earnings change as those who transitioned to paid leave. We also generally do not find statistically significant heterogeneity in earnings changes.

In terms of what we can confidently infer from the NIDS-CRAM data, the substantial changes in *employment* status between February and April (with consequences for *unconditional* earnings) constitute the more salient aspects of the initial COVID-19 labour market shock than the intensive-margin changes in wages. The lack of evidence of wage declines is interesting: If the primary driver of employment loss is a temporary reduction in demand, and there are costs associated with employee hiring, then one may expect it to be more optimal for both employers and employees to deal with the decline in revenue through retaining all employees at a lower wage rather than retaining some employees at the prior wage and dismissing others. Such arrangements would require prior wages to be easily changed; however, the pandemic and associated lockdown meant that large parts of the economy were directly shut down, which perhaps made such arrangements infeasible in any case.

4 | WELFARE IMPACTS

4.1 | Empirical strategy

The poverty impact of COVID-19 and the associated lockdown is a central question of interest. However, because household income in NIDS-CRAM was only asked for April 2020, and not for February, we cannot directly observe income changes from before the lockdown. Additionally, we cannot compare NIDS-CRAM April incomes to incomes in NIDS, as these variables are reported differently.[20]

[19]To reduce noisiness in earnings when taking within-worker changes, we exclude bracket earnings responses and winsorize at the 5% tails of the distribution of percent changes in earnings.

[20]See supporting information Appendix A. As in all surveys, but especially for NIDS-CRAM which was conducted rapidly and under tight resources, the survey team had to make tough trade-offs in which questions to ask. While current household income for April was asked, February household income was not.

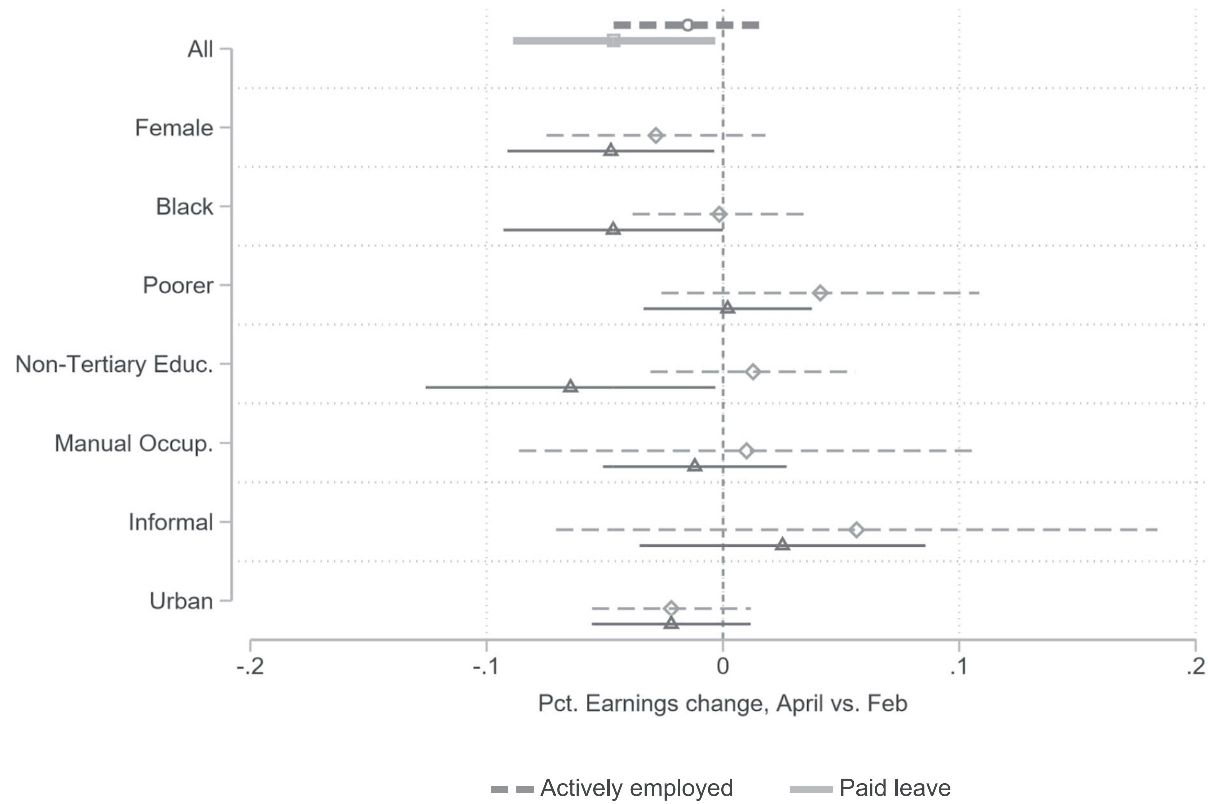

FIGURE 4 Change in earnings for those paid in February and April. *Notes*: The figure shows the percent change in earnings for those workers who were actively working in February and either continued to work actively in April (dashed line) or were on paid leave in April (solid line). The first row shows the average change for all workers in these categories, while the subsequent rows show the earnings loss for different worker characteristics. To reduce noise in the earnings variable, we remove bracket responses and winsorize at the 5% tails. Standard errors are clustered and stratified following the survey design, while estimates are weighted using the survey design weights.

However, we can see which individuals in NIDS-CRAM lost their jobs between February and April. Cross-sectionally comparing the April 2020 household incomes of job losers and job retainers, we can estimate the poverty impact of the COVID-19-induced shock.[21] However, a clear problem with a naive comparison of incomes of these two groups is that job losers are likely to be systematically different from job retainers as low-income workers face disproportionately higher rates of job loss (Section 3).

To estimate a job loss 'treatment effect', we create a counterfactual 'no job loss' income distribution for the job losers, by reweighting the job retainers sample. Intuitively, this allows us to compare household incomes between job losers and job retainers who are observably similar in their characteristics, thus ameliorating the selection effect. Specifically, we use DFL reweighting (DiNardo et al., 1996): We estimate a propensity score for treatment of 'job loss', use these scores to construct inverse probability weights for the job retainers sample and then compare the unadjusted income distribution of the job losers with the reweighted income distribution of the job retainers.

The key assumption needed for our procedure to identify the causal effect of job loss on income is *ignorability* (or *unconfoundedness*) (Fortin et al., 2011).[22] This is a weaker assumption than the conditional independence assumption needed if one were to run a regression of income on job loss in the full sample, which, for example, could be biassed by higher unobserved alternative income sources for those who did not lose their jobs. We do not require that unobservable determinants of income (*e.g.* sources of income) are independent of covariates (*e.g.* job loss) but rather that the conditional distributions of these unobservables, given covariates, are the same for job losers and job retainers. In our example, if we matched on education and household size only, this means that we need to assume that the unobserved variable (sources of income) is similar for job losers and job retainers within the same education and household size. Loosely, this 'selection on observables' allows for selection biases as long as they are the same for job losers and job retainers.

Thus, the key assumption is that the matching variables adequately capture both observed and unobserved correlated with job loss and household income. These matching variables therefore do not need to have a causal interpretation, especially if they are correlated with unobservables. Still, while our assumption of ignorability is weaker than conditional independence, it is not trivial.

For the conditional distribution of unobservables to be plausibly similar across joblosers and job retainers, we need a rich set of pretreatment observable characteristics which predict job loss and which can be controlled for. Additionally, these observable characteristics need to be `pretreatment'—we do not want to control for posttreatment outcomes. The longitudinal nature of NIDS-CRAM means that we have a rich set of 2017 individual-level characteristics to draw from. Combining approximately 1000 characteristics from 2017 along with pretreatment NIDS-CRAM 2020 characteristics (such as demographic characteristics and education), we use an adaptive logit LASSO regression to select variables which predict job loss and then re-estimate a job loss logit using these variables to predict our propensity scores.[23] Supporting information Appendix Tables C1 and C2 show the results from this exercise. Forty-three out of a total possible 2498 combinations of interacted variables are selected as predictors, for example, mother's high school grade, whether someone receives a social grant, and employment 1 year prior. While many of these predictors are intuitive, others are not; recall that the key assumption here is that conditional on these predictors, the distributions of observed and unobserved correlates of job loss and income are similar for job retainers and losers. Thus, for example, whether someone receives a social grant may proxy for poverty and sensitivity to shocks, and this helps satisfy the ignorability identifying

[21]'Job loss' here means a shift from active employment into a temporary layoff or fully severed employment relationship, while 'job retention' means staying actively employed. Those shifting into paid leave are omitted.

[22]We discuss this assumption in detail in the supporting information Appendix C. We also need standard common support and 'no general equilibrium effects' assumptions.

[23]LASSO, or Least Absolute Shrinkage and Selection Operator, is a method to optimise the number of predictors of job loss by selecting the combination of variables that minimises the residual mean squared error plus a penalty term associated with each additional predictor. Note that adding a variable will necessarily reduce the error but increase the summand because of the penalty term. Thus, one avoids simply adding in as many predictors as possible, which may result in overfitting the data, *i.e.* providing a great fit in-sample but worse fit out of sample. Interested readers may refer to Varian (2014) for a review of this and related methods. We use such prediction techniques to construct a control sample (job retainers) in the absence of a clear control group, for example, if there was a compelling quasi-experiment available to leverage in our data.

assumption rather than threaten it. For the same reason, the coefficient values in Table C2 should not be interpreted causally.

Instead, the key tests of whether our procedure works are presented in Figure 5. Empirically, our procedure seems reasonably effective.[24] Estimated propensity scores are well-balanced, and few observations are dropped due to propensity scores below the 1st or above the 99th percentiles of job losers' propensity scores (Figure 5a).[25] This means that we are able to compare each job loser with a control observation that had, according to the prediction model, a very similar probability of job loss.

Of course, the prediction model may be misspecified, so well-balanced propensity score is a necessary but not sufficient condition for the credibility of our exercise. The fundamental problem of causal inference means we cannot directly observe if our procedure works; that is, we cannot observe what the income distribution of 2020 job losers would have been if they had not lost their jobs and how closely our reweighted job retainer income distribution matches this counterfactual. But we can observe 2017 income for 2020 job retainers and job losers, which of course is not causally affected by 2020 job loss, and Figure 5b presents a 'placebo' test for our procedure which uses this observed income. The idea is to use 2017 income as a proxy for 2020 income in the absence of 2020 job loss and check whether our procedure sufficiently corrects for selection bias and adjusts the job retainer (2017) income density such that is sufficiently similar to the job loser (2017) income distribution.

We repeat the LASSO DFL procedure outlined above but do not include NIDS 2017 household income (or its major components) in the donor variables for the LASSO. We then compare the 2017 income distribution of 2020 job losers with the 2017 income distribution of 2020 job retainers. Comparing the unadjusted densities—solid for job losers and dotted for job retainers—the selection issue is clear: 2020 job losers were generally poorer in 2017 than 2020 job retainers. However, after reweighting the job retainers, the reweighted job retainer 2017 income density (dashed) becomes much more similar to the unadjusted job loser 2017 income density. While the densities do not perfectly overlap, we view them as sufficiently similar to suggest that the reweighting procedure is appropriate for approximate estimation of poverty effects. We view this placebo test (and the robustness placebo test in Figure C2; see discussion below) as the key evidence for the plausibility of the ignorability assumption in our context. But we emphasise that we cannot directly test the assumption. A reader who thinks that substantially different dynamics underlie 2017 and 2020 incomes may view this placebo test as unconvincing and be more sceptical about the accuracy of our estimates. We certainly view our results as approximations.

We show two poverty lines in the figure. From left to right, these are the World Bank $1.90-a-day line (R436 per month after PPP conversion) and the Statistics South Africa 'upper bound' line (R1265 per person per month) (Statistics South Africa, 2019). We use these two lines to provide bounds for a range of plausible poverty thresholds, which reflects the approximate nature of the procedure. The leftward excess mass of the reweighted density at low income levels suggests that poverty may be underestimated at low poverty lines, though this is ameliorated somewhat at higher poverty lines where the reweighted density is to the right of the job loser density.

4.2 | Results

Figure 6a shows that the job loser 2020 income density (solid) is substantially to the left of the reweighted job retainer income density (dashed) and that therefore job loss did indeed decrease household income in 2020.

[24]Benchmarks for our LASSO procedure and coefficients from our propensity score estimation logit are shown in the supporting information Appendix C, Tables C1 and C2. The propensity score results also show which covariates are selected in the LASSO procedure. Out of 2498 potential covariates (including counts of the levels of categorical variables), 43 are selected.

[25]By well-balanced, we mean that each bin in the distribution of the probability of job loss for job losers has at least some observations in the comparison sample, as shown in the figure. Otherwise, the reweighting procedure may be less compelling, if, for example, almost all of the control observations had an extremely low propensity score and we had to rely on very few observations to be upweighted.

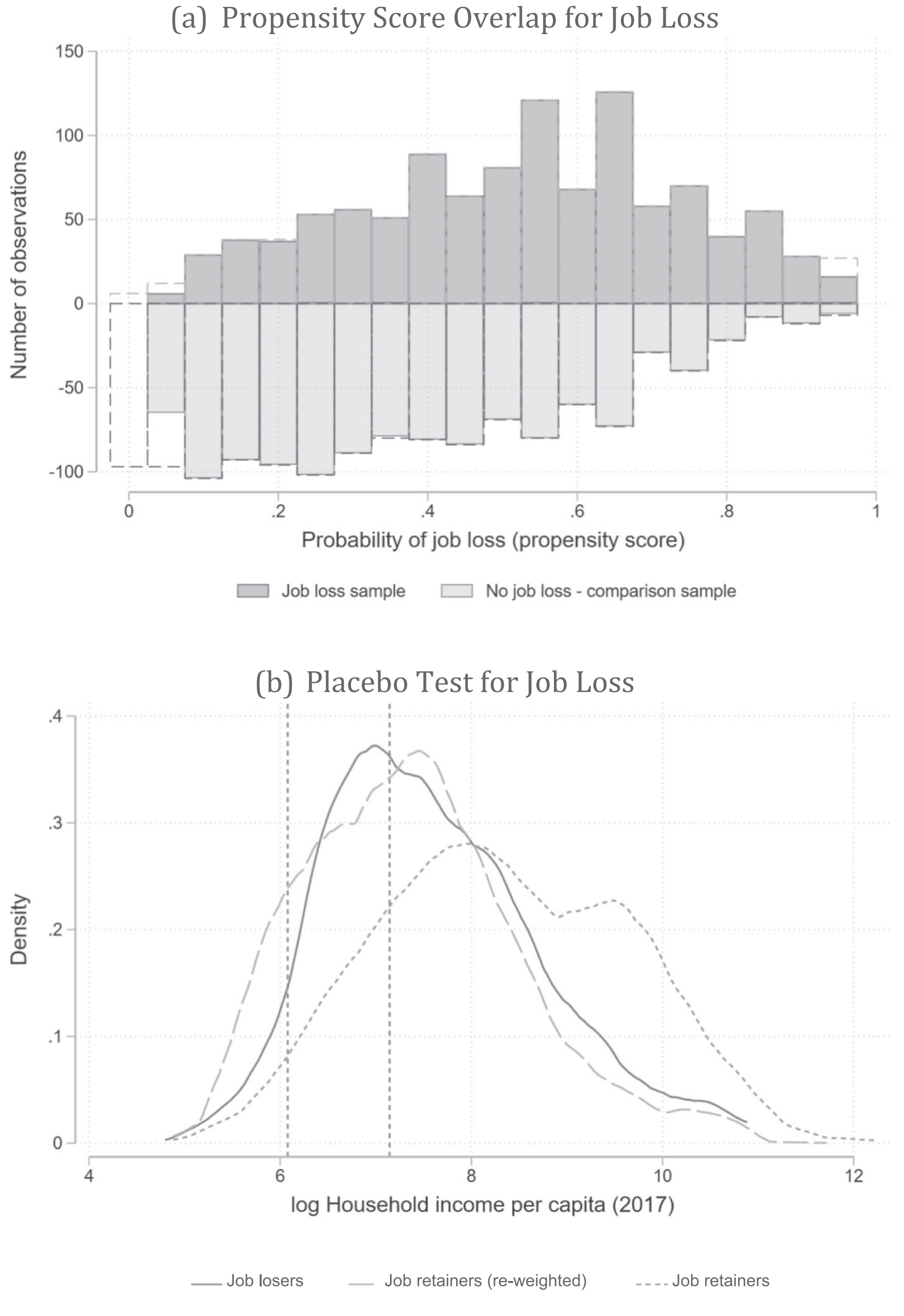

FIGURE 5 DiNardo-Fortin-Lemieux (DFL) reweighting: diagnostics tests. *Notes*: The figure presents evidence on the quality of our reweighting procedures, where we DFL reweight job retainers to match job losers. Panel (a) shows the overlap in job loss propensity scores for job losers and job retainers. Empty bars show observations which are dropped from our matched sample because their propensity scores are below the 1st percentile or above the 99th percentile of propensity scores for job losers. The light-grey bars represent the no-job-loss sample and are shown below the dark grey job loss sample for ease of comparison; the negative *y*-axis values should be read as positive values corresponding to this no-job-loss sample. Panel (b) presents a type of placebo test, showing how our job loss reweighting procedures reweight job retainers (from 2020) such that their reweighted 2017 income distribution approximately matches the 2017 income distributions of 2020 job losers. From left to right, the dashed vertical lines show the World Bank $1.90-a-day poverty line (converted in PPP terms) and the Statistics South Africa upper-bound poverty line. Densities in panel (b) are weighted using the survey design weights.

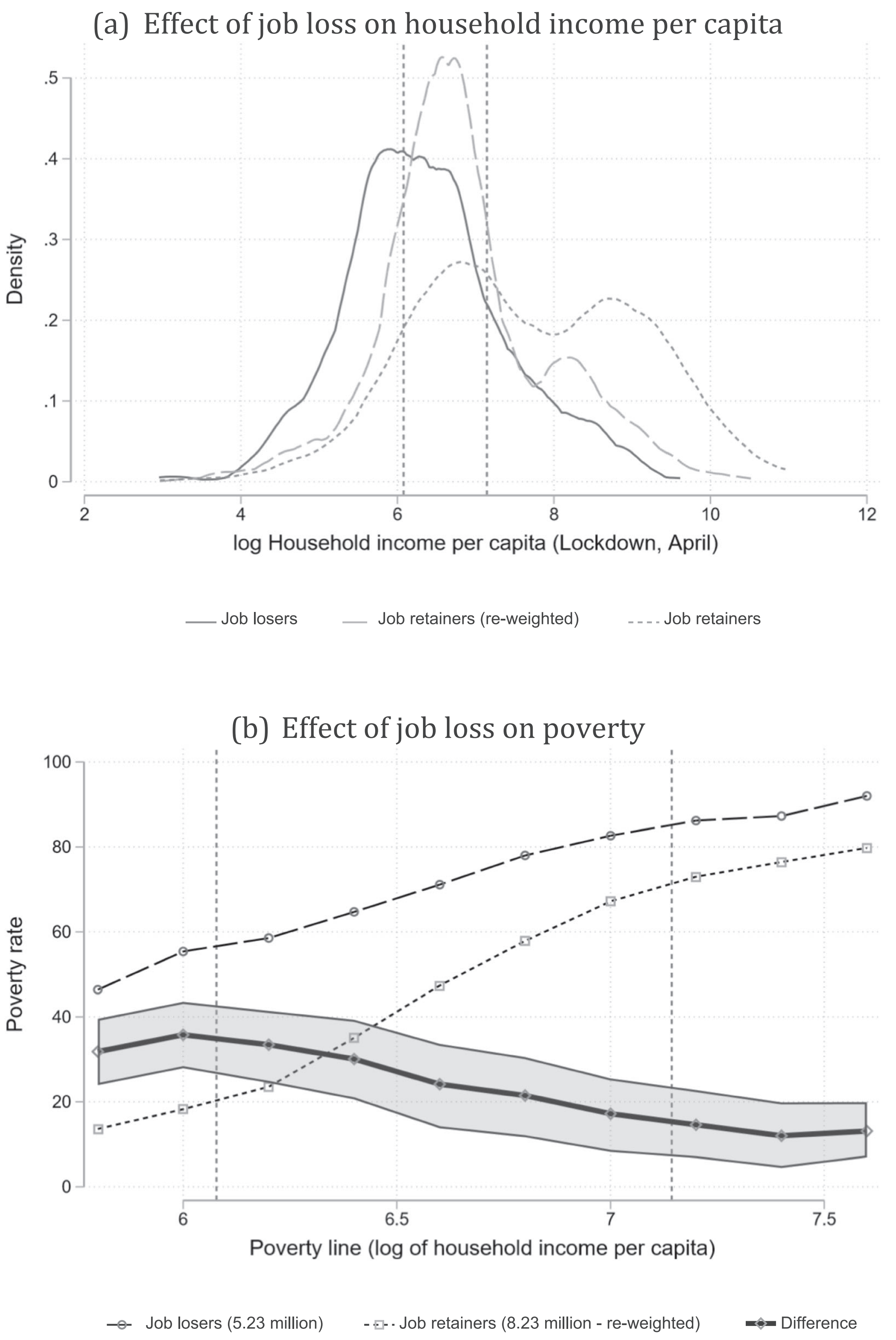

FIGURE 6 Legend on next page.

FIGURE 6 Welfare effects of job loss. *Notes*: Figure shows changes in poverty associated with job loss, defined as the actively employed in February becoming temporarily laid off or not employed in April. From left to right, the dashed vertical lines show the World Bank $1.90-a-day poverty line (converted in PPP terms) and the Statistics South Africa upper-bound poverty line. Panel (a) shows the household per capita income distributions for job losers (solid line) and for job retainers (dotted line). The dashed line is household income of the jobretainers after DFL reweighting. The difference between the solid and dashed lines reflects the treatment effect of job loss. Panel (b) shows the cumulative density of log household income per capita for job losers (dashed line) and job retainers after reweighting (dotted line). Their difference is shown by the solid line shaded with the associated 95% confidence interval (1000 bootstrap repetitions). This line therefore gives the increase in poverty associated with losing a job if the poverty line is defined to be at any point along the *x*-axis. Estimates are weighted using the survey design weights.

The dashed and dotted lines of Figure 6b show April poverty headcount ratios for job losers and reweighted job retainers, respectively, for a poverty line set at any value of household per capita income on the *x*-axis (equivalently, these are the two groups' cumulative density functions). The thick solid line is the difference in poverty rates between the job losers and reweighted job retainers, which as per our DFL procedure is the poverty 'treatment effect' of job loss, for each given poverty line.

We estimate that between 15% and 35% of job losers fell into poverty as a result of job loss, depending on which poverty line is used. At the upper-bound poverty line, the confidence interval suggests an increase in job loser poverty of 7–23 percentage points, while at the $1.90-a-day line, the equivalent increase is 27–43 percentage points.

4.3 | Interpretation and robustness checks

The job loss specification allows an estimate of a specific treatment effect: the number of *workers* who are pushed into poverty *because they lose their job*. An estimate of the total number of *individuals* who fall into poverty due to job loss requires some accounting for workers' dependents, an exercise we undertake in the next section.

While job loss is not the only lockdown-induced factor likely to have a bearing on incomes, it is probably the most important.[26] Our job loss local treatment effect is therefore particularly relevant for understanding the overall poverty effect in contexts where job loss is a more dominant driver of income changes.

One could, in theory, identify the adult poverty increase due to *any* income loss using NIDS-CRAM questions which directly ask if household income decreased over the lockdown period. We report results using this approach in the supporting information Appendix C but prefer the job loss specification because the household income loss questions appear to be of poor quality (see supporting information Appendix C). Results using this method are in any case qualitatively similar to our preferred specification.

We undertake additional robustness tests in the supporting information Appendix C. In Figure C2a, we show the same placebo test for February 2020 earnings as we do for 2017 household income.[27] The reweighting seems to work reasonably well again, substantially shifting the earnings distribution of job retainers to the left. Some divergence remains between jobloser and reweighted job retainer earnings; however, and therefore, it is possible that our estimates of job loss-induced poverty could be slightly upward-biassed, contrary to what Figure 5b would seem to suggest, at least for lower poverty lines. Given concerns about possible underreporting of household income and overreporting of household size in NIDS-CRAM (see Supporting information Appendix A), we also check for increases in reported food-insecurity using the same DFL reweighting procedure—we find that job losers do report greater food-insecurity than the reweighted job retainers (Figure C2b). For our baseline poverty estimates, we also

[26]We discuss other factors in the supporting information Appendix C.

[27]For this figure, we use 'earnings per capita' (individual earnings divided by household size) rather than total individual earnings. While this is not a substantively meaningful welfare measure itself, it is appropriate here because it removes otherwise distorting round-number bunching in reported wages and creates a smooth distribution comparable with that of April per capita incomes, which is the distribution of interest.

implement a version of matched difference-in-differences, across NIDS 2017 and NIDS-CRAM, which compares the change in income between 2017 and 2020 for job losers vs reweighted job retainers. While the poverty increase according to this approach is fairly similar to our main specification at the upper-bound poverty line, it is substantially higher at the food poverty line (Figure C2b), consistent with our interpretation of Figure 5b which suggests that the poverty increase may be underestimated at lower poverty lines.

5 | SOCIAL PROTECTION

Realised changes in household income are jointly determined by the initial COVID-19 earnings shock, by the subsequent and partial labour market recovery, as well as the roll-out of compensatory social protection benefits. These three forces did not occur simultaneously but unfolded sequentially following the imposition of South Africa's lockdown.

In the first wave of NIDS-CRAM data, in which retrospective questions were asked for April, household income would have been affected by the initial COVID-19 earnings shock and by some limited social protection measures. The main form of emergency social insurance implemented for workers in April was the TERS. For the period of NIDS-CRAM Wave 1 data collection, social assistance remained unchanged relative to February. Nevertheless, the existing social grant system would have provided some cushioning of the COVID-19 labour market shock.

In the second wave of NIDS-CRAM, which asked retrospective questions for June, household income would have been affected by the initial COVID-19 earnings shock, by the partial labour market recovery and by a more comprehensive roll-out of compensatory social protection measures. The expansions to existing social grants and the (gradual) roll-out of the new SRD grant were introduced from May and would therefore co-determine household income observed in June.

5.1 | Social protection in April

We find that 37% of those who were 'temporarily laid off' or put on 'paid leave' in April were not covered by any kind of social protection measure (Figure 7a).

Only 20% of these workers received TERS.[28] The upper-middle parts of the distribution—the Service/Operators occupational group and the third quartile of February 2020 earnings—seem to have the greatest coverage by TERS.[29] Grants are, however, consistently progressively targeted. Household grants reach a substantial share of the temporarily unemployed, with over 50% of these workers receiving a grant in their household.

TERS was not applicable in cases where the employment relation was completely severed. For job losers who shifted into nonemployment, we therefore examine social protection coverage by looking at co-residency with different types of household grant receipt, distinguishing between CSG receipt and other grant receipt[30] (Figure 7b). We find that close to a third of these workers were not covered by any kind of social protection measure, 39% received only the CSG in their household, 9% received other household-level grants but no CSG and 20% of these workers received both the CSG and some other grant in their household. Across different groups of job losers, household grant receipt was clearly progressive, with women, those at the bottom of the February earnings distribution, and informal

[28]While TERS was initially intended to cover only formal sector workers, it was successfully challenged in court and officially amended on 25 May to also cover workers not already registered for the Unemployment Insurance Fund (UIF). Therefore, while our estimates do apply to April, our discussion on coverage pertains to the ideal implementation of TERS, that is, including informal workers. In any case, we are unable to restrict to formal workers since we do not observe informality directly in our data. We do observe informality status in 2017, and we provide heterogeneity in TERS coverage using this in Figure 7, with higher but still low TERS coverage among formal sector workers (25% compared with 20%).

[29]We follow Ranchhod and Daniels (2021) in disaggregating by February earnings quartiles.

[30]This latter category includes the (few) Social Relief of Distress Grants we observe.

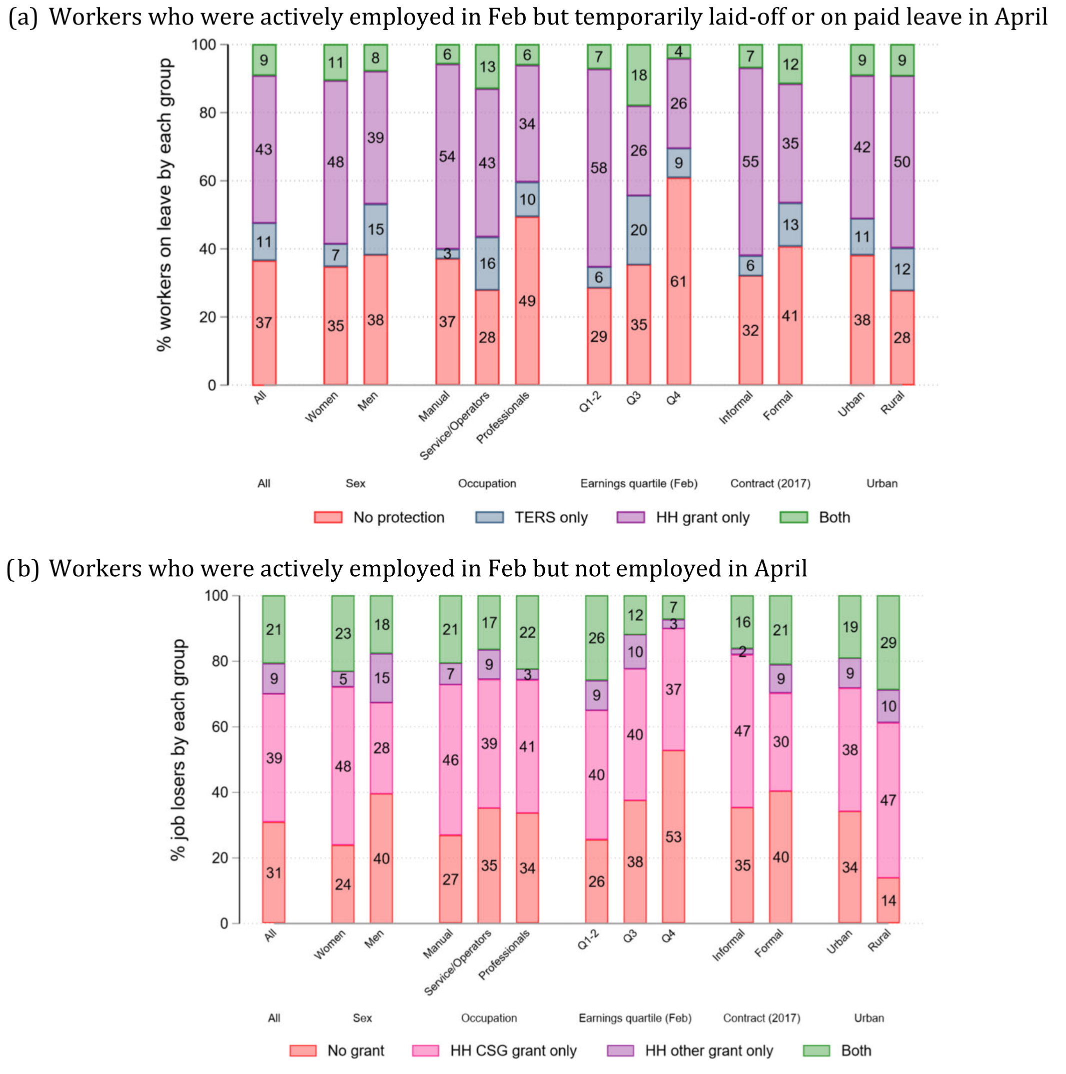

FIGURE 7 Social protection coverage for workers no longer actively employed. *Notes*: Panel (a) shows coverage of social protection for those who were employed in February and are 'temporarily laid off' or on 'paid leave' in April. Grant receipt refers to receiving a grant in their household. Red sub-bars show the percentage in each category who are not reached by any type of grant and do not receive TERS, blue shows those who receive TERS but no grant, purple shows those who receive a grant but not TERS and green shows individuals who receive TERS and a grant. TERS is the social insurance scheme implemented for workers in response to COVID-19. Panel (b) shows coverage of social protection for those who were employed in February and are no longer in employment in April. The sub-bars distinguish between social assistance from having a child support grant recipient in the household vs any other social grant. Estimates are weighted using the survey design weights.

workers being much more likely to have a grant recipient in the household. This was mainly driven by the progressively targeted CSG (Bassier, Budlender, Zizzamia, et al., 2021).

That two thirds of nonemployed job losers had a grant in their household reflects both the progressivity of the grant system and the regressivity of the labour market shock (as reflected by larger net job loss for lower household income quartiles in Figure 2). However, it is important to bear in mind that being 'covered' by household grant receipt would frequently not preclude descent into poverty. This is because the vast majority of these grant recipients live in multimember households (Bassier, Budlender, Zizzamia, et al., 2021), and the monetary value of each grant was small compared with the magnitude of the labour market shock, especially the CSG.[31] Grant coverage is also unevenly distributed: As Figure 7b shows, rural job losers were substantially more likely to be covered by household grants than those in urban areas.

5.2 | Social protection in June

South Africa's existing grant infrastructure was supplemented from May onward. In addition, the new SRD grant was gradually introduced, with implementation gaining momentum in June and July. The SRD grant invited applications from individuals where eligibility was defined as any adult not in employment and not receiving any of South Africa's existing grants. Job losers would in many cases have been eligible for the SRD, valued at R350 per month. The SRD is a form of noncontributory social assistance, much like South Africa's other grants, but is designed to support those nonemployed individuals not covered by the existing grants during the COVID shock.

In Figure 8, we show the extent to which job losers who remained not employed in June were covered by various forms of social protection, with a focus on coverage through the SRD grant, distinguishing this from coverage by any other household or individual social protection, including UIF. We find that, overall, around 17% of workers who were actively employed in February but not employed by June did not receive social protection in June. On the other hand, 33% of these job losers were part of a household which received at least one SRD grant. In terms of reach on the extensive margin, South Africa's existing grant system remains crucial and accounts for the vast majority of those covered by social protection.[32]

We find that coverage of SRD grant was much higher among men (38%) compared with women (28%). However, we do observe that it was otherwise progressively targeted—the receipt of SRD was over 20 percentage points higher in the bottom two quartiles of the February earnings distribution than in the upper two quartiles. Similarly, coverage was almost twice as high for those workers who in 2017 were classified as employed in the informal economy (37% vs 22%, respectively). Coverage was also approximately 10 percentage points higher in rural areas than in urban areas.

6 | DISCUSSION

Here, we present a more speculative extension of the poverty analysis in two directions: accounting for dependents in workers' households and assessing the poverty-mitigating effects of expanded social protection by counterfactually simulating a scenario of no additional social protection measures.

6.1 | Accounting for dependents

With 5.23 million individuals having lost their jobs between February and April, our estimate that 15% to 35% of job losers fell into poverty is equivalent to an additional 800 000 to 1.8 million individuals falling below reasonable poverty thresholds. However, this poverty statistic is associated with the *gross* increase in job loss between February and April. As noted in Section 3, there is significant churning in the South Africa labour market, and 0.94 million February nonemployed or temporarily laid off individuals found active employment in April; the *net* increase in job loss over the period was therefore 4.29 million rather than 5.23 million individuals. If one makes the additional assumption that the income increase from a job *gain* is symmetric to the income decrease we identify from a job *loss*, then the poverty

[31]The value of the Child Support Grant was R440 ($66 PPP) per month per eligible child in April. For reference, the monthly national minimum wage in 2019 was R3,500 ($524 PPP).

[32]Note that while grant coverage is at the household level, we only observe UIF receipt for individuals.

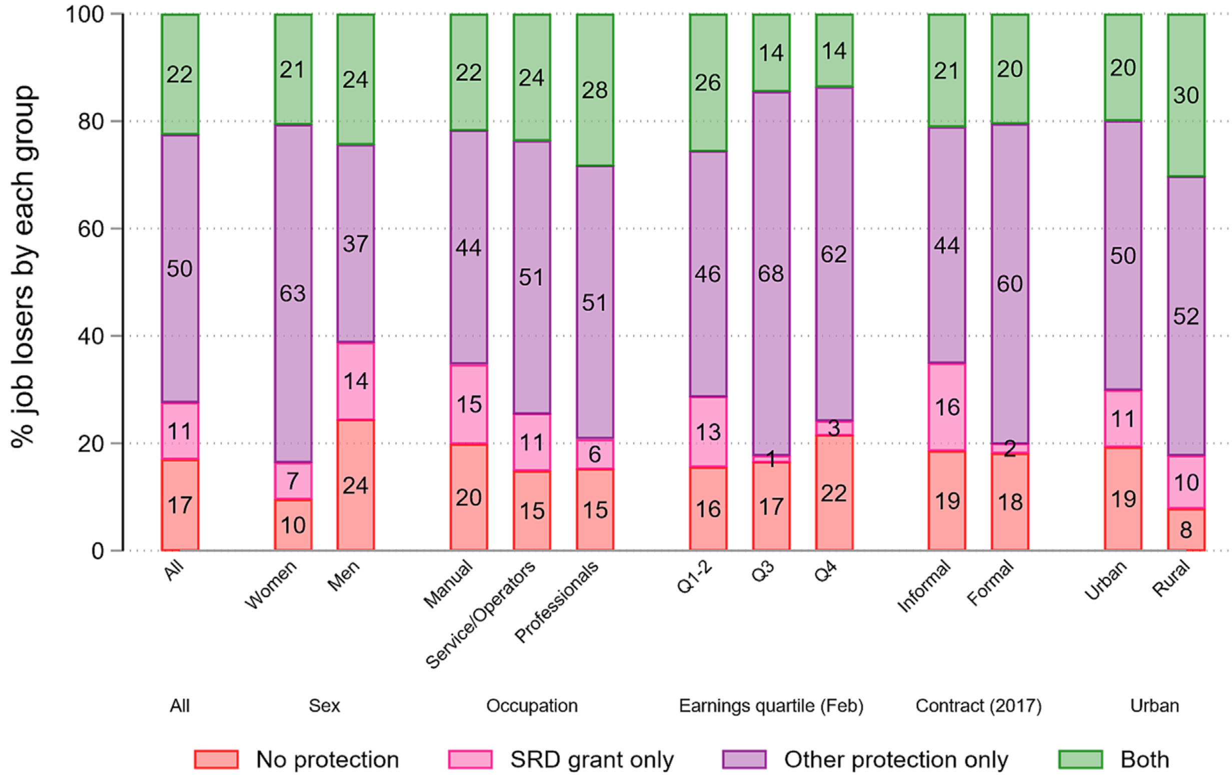

FIGURE 8 Social relief of distress grant coverage in June for job losers. *Notes*: The figure shows the June social protection received by individuals who were in active employment in February and were not employed in June, distinguishing *household* coverage by the social relief of distress (SRD) grant and coverage by any other form of social protection. SRD coverage means at least one household member receives the SRD in June. 'Other protection' means receipt of a non-SRD social grant within the household or individual receipt of an unemployment insurance fund (UIF) payout. We use a balanced NIDS-CRAM Wave 1 and Wave 2 panels, and estimates are weighted using the survey design weights.

effect of net job *transitions* between February and April will be between 0.6 and 1.5 million additional job losers in poverty. We prefer the *gross* job loss figure to avoid making this additional assumption but highlight here that we are identifying a specific treatment effect with our job loss poverty estimates[33]

Regardless of whether gross or net job loss is used, however, the poverty effects reported above understate the total poverty impact of COVID-19 job losses. In particular, workers typically support many dependents who will also be affected by job loss. We cannot directly identify dependents in the NIDS-CRAM data because there is no household roster and the sample does not include children. To approximate the broader impact of job loss-induced poverty, we estimate the average number of dependents each job loser supported in NIDS 2017.[34] Averaging across all job losers in NIDS-CRAM, we find a dependency ratio of 3.2.

Thus, for our estimate of COVID-19-related job loss pushing 0.8 to 1.8 million job losers into poverty by April, we conjecture a broader job loss effect of approximately 2.6 million to 5.8 million individuals having fallen into poverty, when accounting for the dependents of job losers.

This exercise comes with important caveats. In particular, household structure may have changed for job losers between 2017 and 2020, and we also estimate the dependency ratio across all job losers, while the relevant group is those shifted into poverty.

6.2 | Poverty impact of expanded social protection

In Figure 9a, we compare poverty rates in April to poverty rates in June. In Figure 9b, we compare observed poverty rates in April to a counterfactual scenario in which no COVID-19-related expansions to social protection were implemented, allowing us to roughly back-out the effectiveness of these social protection measures in mitigating the COVID-19 poverty impact.

First, in order to undertake a poverty comparison between April and June, we use household income as reported in NIDS-CRAM, converting this to a per capita measure by dividing household income by household size. In June, household income can be reported in brackets, and where individuals report income in a bracket, we assign them the median household income of nonbracket households whose incomes fall in the bracket bounds.[35] Household income is not always reported by respondents in April and June, and in particular, there is substantially more income nonresponse in April. We therefore model the probability of reporting income in April using a logit specification and the same predictors as are used for constructing the Wave 2 attrition weights in Daniels et al. (2022) and create inverse probability weights which are used for our April–June income analysis.[36]

Figure 9 plots the adult poverty headcount ratio in South Africa across a range of possible poverty lines for April and for June. We use this range of poverty lines because it is not clear how income reported in NIDS-CRAM corresponds to income in other South African household surveys—including those used to calibrate poverty lines. We thus prefer to avoid attaching too much significance to any one line in particular. The World Bank $1.90-a-day poverty line and the Stats SA upper poverty line (respectively the two dotted vertical lines) are, respectively, used as lower and upper bounds for 'poverty'. We emphasise that the poverty rates and changes shown are for the adult NIDS-CRAM sample population aged 18-64. NIDS-CRAM does not include children in the sample.

June poverty rates (red dotted line) were, for all plausible poverty lines, lower than their April equivalents (red dashed line). This suggests a decrease in poverty between April and June, in line with the partial recovery we see in the labour market. The poverty rate decreased between 3 and 6 percentage points

[33]We check the wage aspect of the relative incomes of these groups. We find that the distribution of April-wages of job gainers is higher than the distribution of February-wages of job losers; this may relate to differential job loss among lower-paid jobs, such that the replacement rate was higher in higher-paid jobs, and this shifted up the wage distribution of the job gainers (noting that the absolute number of job gainers is much lower). Insofar as the job gainers' wages are indeed higher, and this shifts more incomes above each poverty line, the net poverty effect will be smaller.

[34]We divide each job loser's 2017 household size by the total number of workers in the household.

[35]While this rather simple bracket imputation does create bunching in the household income distribution, the household per capita income distribution is suitably smooth.

[36]Specifically, these new weights are the product of the inverse of the propensity score estimated in the logit model and the Wave 2 panel weights.

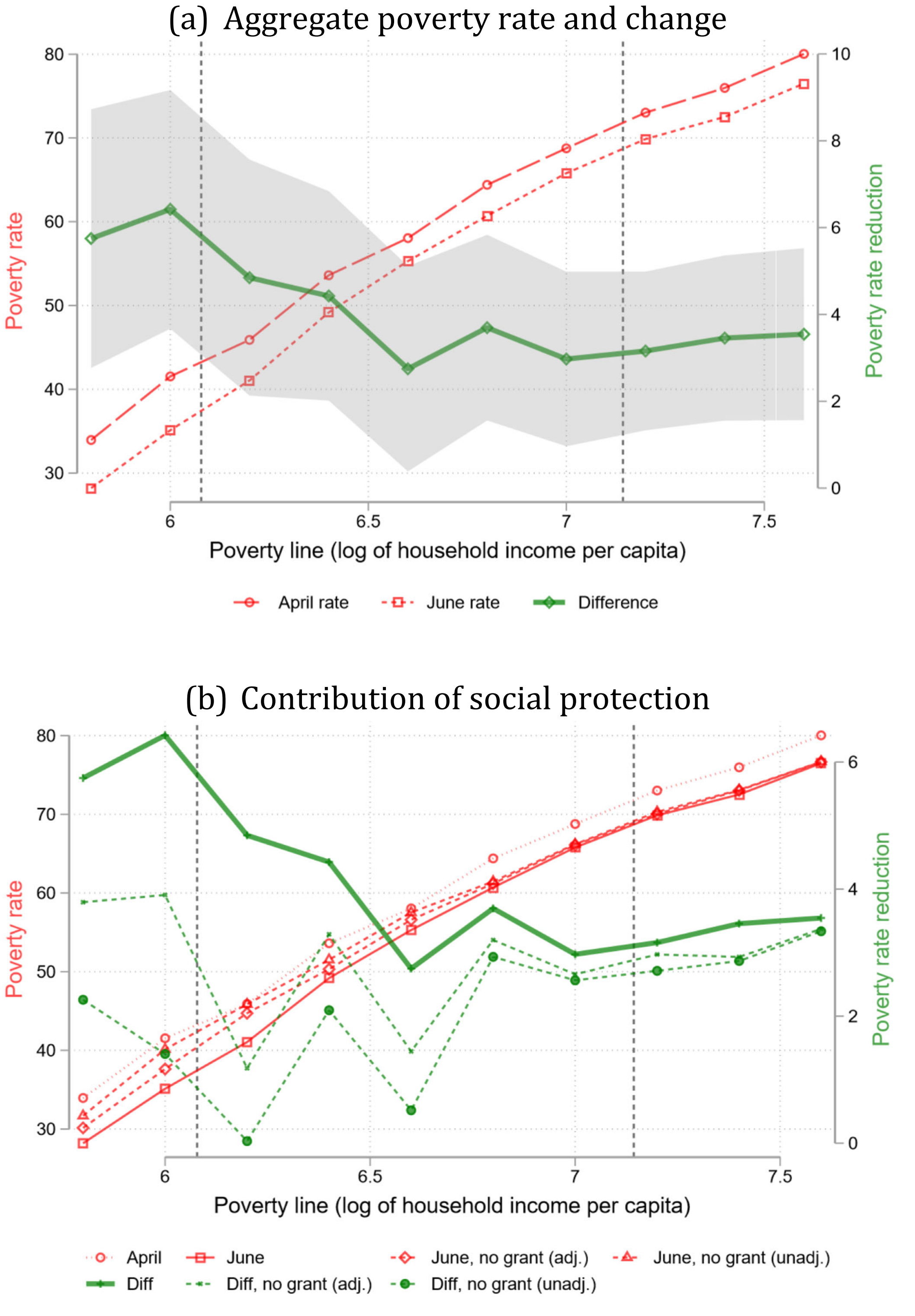

FIGURE 9 Legend on next page.

FIGURE 9 Poverty rates in April and June. *Notes*: (a) The figure plots South African adult poverty headcount ratios in April and June for a continuum of poverty lines on the horizontal axis. From left to right, the dashed vertical lines indicate the World Bank \$1.90-a-day poverty line (converted in PPP terms) and the Statistics South Africa upperbound poverty line (respectively R436 and R1265 per person per month in march 2020 Rands). The dashed lines in red show the April and June poverty rates for each poverty line (left vertical axis). The difference in the poverty rate between April and June for each poverty line (*i.e.* the poverty reduction) is shown by the solid green line, shaded with the associated 95% confidence interval (right vertical axis). Panel (b) adds counterfactual poverty results when the expected income associated with receiving COVID-19 expanded social protection (social relief of distress, caregiver and top-up to old age grants) is removed from June income. This subtracted amount is calculated as the number of recipients of each grant times by the official additional amount associated with each grant. Since household income is likely underestimated, the 'adjusted' line shows household income minus 0.5 times the estimated additional grant income. For both figures, we use a balanced NIDS-CRAM Wave 1 and Wave 2 panels, and estimates are weighted using the survey design weights.

(green solid line), depending on the poverty line used; this corresponds to 1-2 million out of an adult population of 34 million.[37] The poverty decrease is larger at lower poverty lines, suggesting that the poverty recovery was strongest at the bottom of the income distribution. This difference in incidence along the distribution is magnified when considering the change in poverty rate as a percentage of April poverty—approximately a 13% reduction in poverty at the \$1.90-a-day poverty line and a 4% reduction at the Stats SA upper bound.

The confidence intervals of the poverty decrease (grey shaded area) show that the poverty decrease point estimate is generally statistically different from 0, but there is a nonnegligible margin of error around these estimates.

In Figure 9b, we simulate changes in poverty headcount ratios between April and June under the counterfactual scenario of *no COVID-19 social protection*. This is achieved by subtracting expected income associated with receiving COVID-19 expanded social protection (SRD, Caregiver and top-up to Old Age grants) from the observed June income. This subtracted amount is calculated as the number of recipients of each grant times by the official additional amount associated with each grant. Since household income is likely underestimated, we also include a line which is adjusted to roughly account for this potential underestimation. This 'adjusted' line shows household income minus 0.5 times the estimated additional grant income.

In this counterfactual exercise, we find that the COVID-19-related expansion in social protection accounted for most of the observed reduction in the extreme poverty headcount ratio (as measured by the WB \$1.90/day line) between April and June. At this poverty line, the unadjusted 'no poverty' counterfactual scenario suggests that in the absence of social protection, the change in poverty between April and June would have been negligible. The 'adjusted' line suggests some reduction in poverty (approximately three percentage points). Nevertheless, both adjusted and unadjusted counterfactual scenarios suggest that the observed poverty reduction can be attributed primarily to the expansion in social protection, with the labour market recovery likely playing a limited role.

However, as one moves across the spectrum of poverty lines, the observed and counterfactual scenarios converge at a reduction in the poverty headcount of between three and four percentage points. This convergence suggests that, at higher poverty lines, the observed poverty reduction between April and June is likely accounted for by the limited labour market recovery rather than the expansion in social protection. This pattern is consistent with the small amount of the SRD grant compared with relatively large income from employment.

[37]Since we do not observe household income in February, we cannot estimate the direct changes in poverty between February and June. However, we can make some back-of-the-envelope calculations, subject to several caveats. In the previous section, we estimated that poverty increased by 2.6–5.8 million people based on the disemployment effects between February and April. If this is well-estimated and captures the main change in poverty between February and April (there may be many other reasons for poverty changes in this period, such as household composition changes), then we can combine this with the observed decrease in poverty based on household income between April and June (*i.e.* 1.7 to 3.4 million people). This suggests that poverty increased between February and June by 0.9–2.4 million people.

Taken together, Figure 9 provides evidence for the important role played by expanded social protection in reducing levels of extreme deprivation in the months following the initial COVID-19 shock.

7 | CONCLUSION

The COVID-19 crisis resulted in a large negative labour market shock globally. We use contemporaneous survey data which are representative of the substantial majority of the population (over-15 year olds in 2017) to provide some of the first evidence on the initial impact of COVID-19 on employment and poverty in South Africa.

We observe a 40% decline in net active employment between February and April of 2020, with approximately half of this being composed of shifts into nonemployment. These movements into nonemployment proved durable, with the limited labour market recovery evident in June–June employment was 20% lower than February employment—mostly coming from those who maintained some kind of employment relationship in April. The incidence of employment losses was much greater among female, manual and informal workers and the poor, thereby exacerbating existing inequalities.

While recognising that the inferential exercise we use here is unavoidably inexact, we estimate that approximately 15%–35% of those who lost jobs over the severe lockdown period fell into poverty, which translates to between 0.8 and 1.8 million job losers. Accounting for dependents, we tentatively estimate that between 2.6 and 5.8 million individuals fell into poverty as a result of this job loss.

We document that in April, only 20% those who lost active employment received relief through new COVID-19 social insurance mechanisms, and approximately one third of job losers did not receive any household-level social protection at all.

However, by June, various emergency social assistance programmes had been implemented. We document a decline in poverty between April and June, and our counterfactual simulations suggest extreme poverty was significantly reduced by these policies, such as the new SRD grant.

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CONFLICT OF INTEREST STATEMENT

The authors declare that there is no conflict of interest beyond what is stated in this acknowledgment. Any errors remain our own.

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