

The labor market and poverty impacts of COVID-19 in South Africa

Ronak Jain*
rjain@g.harvard.edu

Joshua Budlender†
jbudlender@umass.edu

Rocco Zizzamia‡
rocco.zizzamia@qeh.ox.ac.uk

Ihsaan Bassier†
ibassier@umass.edu

July 19, 2020

CSAE Working Paper WPS/2020-14

Abstract

We use newly-released South African data to present the first estimates of COVID-19-related employment and poverty impacts in a developing country. We observe a 40% decline in active employment. Half of this comprises job terminations, suggesting persistent labor market effects. Initially vulnerable groups are disproportionately affected. Exploiting the dataset's panel dimension and comparing lockdown incomes of job losers to re-weighted job retainers, we estimate that 20-33% of job losers fall into poverty. Only 20% of those temporarily not working received the intended relief, while a third of job losers had no access to any major form of social protection.

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

Keywords: Labor markets, poverty, unemployment, COVID-19, social protection

The authors contributed equally to this work. Author order is randomized. We are very grateful to Debbie Budlender, Reza Daniels, Arindrajit Dube, Leila Gautham, Edward Glaeser, Claudia Goldin, Alyssa Huberts, Lawrence Katz, Asim Ijaz Khwaja, Gabriel Kriendler, Pramila Krishnan, Murray Leibbrandt, Aarti Malik, Suresh Naidu, Vimal Ranchhod, and participants at the Development Urban Public workshop at Harvard Economics Department and NIDS-CRAM Labor workshop, for helpful comments and discussions. The authors gratefully acknowledge funding for the project through NIDS-CRAM. Any errors remain our own.

*Harvard University

†University of Massachusetts Amherst

‡University of Oxford and SALDRU, University of Cape Town

1 Introduction

Little is known about the labor market effects of COVID-19 in developing countries and its poverty impact in these less-protected labor markets. The first countries in which detailed labor market data has become available – primarily in Western Europe and North America – have recorded historic labor market contractions (Adams-Prassl et al., 2020; Alon et al., 2020; Bartik et al., 2020; Chetty et al., 2020; Coibion, Gorodnichenko, and Weber, 2020; Hassan et al., 2020). However, these have also been the countries which introduced the most comprehensive set of social protection relief measures. It is widely expected that the poverty impact of the COVID-19 pandemic will be most severe in emerging market economies (Mahler et al., 2020; Sumner, Hoy, and Ortiz-Juarez, 2020).

In this paper, we investigate two empirical questions: (1) What are the labor market effects of COVID-19 in South Africa? and (2) How has this labor market shock affected welfare? We use a newly-released dataset – the National Income Dynamics Study: Coronavirus Rapid Mobile Survey (NIDS-CRAM) – to present some of the first estimates of the impact of COVID-19 on employment and welfare outcomes in the developing world. NIDS-CRAM is a stratified 30% subsample of adults in the 2017 wave of the National Income Dynamics Study (NIDS), a nationally representative longitudinal household survey.

We find that after one month of intensive lockdown, active employment decreased by 40%. In nearly half of these cases, workers did not expect to return to their jobs, suggesting potentially long-lasting effects in the labor market. The pattern of job-loss severely exacerbated existing inequalities, and we estimate a substantial increase in poverty for job-losers.

To investigate the labor 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 categorization, 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 March 5th) to April 2020 (following the imposition of a stringent lockdown on March 26th).

Of the 40% net decline that we observe in active employment, almost half is accounted for by increases in non-employment as opposed to temporary layoffs or paid leave. Women, those with lower levels of education, those in manual occupations, informal workers, and the poor face the greatest net employment losses. These employment losses are disproportionately made-up of shifts into non-employment. For individuals who remained actively employed, 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.

It is challenging to identify the effect of job losses on poverty because we only observe household income in April 2020 (post-shock) but *not* February 2020 (pre-shock), preventing us from directly observing the change in this variable. To estimate the poverty effect of February to April job-loss we instead compare April incomes of job-losers to the incomes of those who remain actively employed over this period. To address selection issues, we re-weight the household income distribution of job-retainers as per DiNardo, Fortin, and Lemieux (1996) (DFL), after using a logit 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 re-weighting procedure works as intended.

We estimate that between 20% and 33% of job-losers – approximately 1 to 1.7 million individuals – fall into poverty as a result of COVID-19-related job loss, depending on the poverty line used.¹ 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 3

¹This is equivalent to 6% to 10% 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 0.8 to 1.4 million people, or 5% to 9% of the actively employed in February. We discuss the gross versus net specifications in Section 6.

to 5.5 million people including dependents.²

Part of the explanation for these large poverty impacts is the relatively low rate of social protection coverage in April. 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.³ While pre-existing social assistance measures progressively provide broader coverage, 30% of those who transitioned into non-employment reported no household-level grant protection. Based on a counterfactual exercise, we tentatively estimate that the April poverty increase associated with job-loss would have been mitigated by about 35-50% if TERS had been implemented as intended and social grant top-ups had been implemented in April.

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 labor 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, Bloom, and Davis, 2020; Bartik et al., 2020; Béland, Brodeur, and Wright, 2020; Bui, Button, and Picciotti, 2020; Chetty et al., 2020; Coibion, Gorodnichenko, and Weber, 2020; Cowan, 2020). This literature has almost exclusively focused on labor market impacts in rich countries. The second strand of this literature has concentrated on

²This is equivalent to 5% to 10% of the total population in South Africa. Accounting for job finders as above, the comparable figure with dependents is approximately 2.5 to 4.5 million people, or 4% to 8% of the total population.

³TERS 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).

the poverty impacts of the COVID-19 pandemic, but because of the lack of real-time data on welfare indicators, this literature has until now only consisted of poverty *projections* (Bassier et al., 2020; Kartseva and Kuznetsova, 2020; Mahler et al., 2020; Parolin and Wimer, 2020; Sumner, Hoy, and Ortiz-Juarez, 2020). Most of these projections have been for developing countries, where the economic consequences of the COVID-19 labour market shock are expected to be most severe (Gerard, Imbert, and Orkin, 2020; International Labour Organisation, 2020; Kerr and Thornton, 2020).

We provide some of the first estimates of the labour market impact of COVID-19 in a developing country using ex-post, broadly representative data. 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, for any country. In addition, we include an assessment of COVID-19 social protection coverage, contributing to a literature documenting these interventions (Gentilini et al., 2020) and assessing their performance (Rothwell, 2020). Lastly, an important advantage of our analysis is the longitudinal survey data we use, which links NIDS-CRAM respondents back to their records in earlier NIDS waves. In addition to exploiting this in our estimation strategy, this also allows us to assess the representativity of NIDS-CRAM compared to the nationally-representative NIDS, 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 provides evidence on social protection coverage. Section 5 presents our estimates of the COVID-19 poverty impact and our empirical methodology. Section 6 discusses the implications of our poverty results for population-level poverty impacts, and considers counterfactual policy responses. Section 7 concludes.

2 Data

2.1 NIDS-CRAM

This paper uses data from NIDS-CRAM (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. Respondents were mainly asked retrospective questions about their circumstances in February and April. The NIDS-CRAM sample is 7,074 individuals drawn from the adult sub-sample of the fifth wave of NIDS (2017), an existing longitudinal household survey. 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 18-64 in 2020.

NIDS-CRAM should be interpreted as being broadly representative of the NIDS sample of South African adults from 2017, which itself was nationally representative at the time (Ingle, Brophy, and Daniels, 2020).⁴ Appendix Table A1 shows good balance of raw sample statistics across most categories for NIDS-CRAM and NIDS Wave 5. Middle-aged individuals were however intentionally over-sampled for better precision of labor market statistics – this is corrected by using weights when estimating population-level statistics.

3 Labor 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 categorization is used in the construc-

⁴Refer to Appendix A for details on sampling, response rates and comparability to previous waves.

tion of internationally-comparable labor market statistics. However, a straightforward application of this categorization is prone to overlooking several important features of the COVID-19 labor 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 labor 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 current situation it is also possible that survey respondents themselves are 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 labor 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 lay-off*: 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.

3.2 Employment

We find a 21 percentage point net decline in active employment as a share of the working-age population, while there is a 9 percentage point net increase in non-employment, a 7 percentage point net increase in paid leave, and a 4 percentage point net increase in temporary layoffs (Figure 1 Panel (a)). Just under half of the active employment decline is therefore attributable to increases in severed employment relationships rather than temporary unemployment, suggesting that the COVID-19 shock will likely have long-run impacts on the South African labor market.⁵

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 labor reallocation shock leading to job creation as well as job destruction (Barrero, Bloom, and Davis, 2020), and also because we wish to account for the generally high rates of labor market churning evident in South Africa (Kerr, 2018). Results for gross employment losses, as opposed to net changes, are presented in Appendix B. While active employment unsurprisingly 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.

Figure 1, Panel (b) shows *percent* changes in the net number of people employed in each category as a percent of February active employment. For example, column one shows

⁵Throughout, we make the implicit assumption that changes over the February-April period were substantially due to the COVID-19 shock. Given the suddenness of these changes and the magnitudes of the labor market effects we observe, we view this as a reasonable assumption. We do not attempt to decompose this “COVID-19 effect” into the effect due to general pandemic conditions versus that due to the lockdown.

that the number of actively employed adults decreased by approximately 40% between February and April 2020 and decomposes this into net increases in the “not employed” (18%), “temporary layoff” (9%), and “paid leave” (14%) categories.

Consistent with evidence in developed-country contexts (Adams-Prassl et al., 2020; Alon et al., 2020; Cowan, 2020), employment decreases have disproportionately affected women, manual workers and the poor. Women have seen 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 to one third for men. The disparities are even starker between occupation categories: manual workers have seen a 50% net decline in active employment, which is 30 percentage points greater than professionals.⁶ 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 “non-employed” category. These results regarding occupation and gender are related: women are much more likely to be in manual work relative to men.

Those 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 non-employment.⁷ A similar pattern is observed with respect to educational attainment: those with tertiary education fare much better than those without. As expected, there is a much smaller net shift into non-employment for those who reported having a written contract in 2017 compared to those reporting a verbal contract – reflecting different impacts on formal and informal workers.⁸

In Appendix B we show that the differences in the size of the February-April net active employment loss between these groups (e.g. women versus men) are statistically

⁶See Appendix A for details on these occupational classifications.

⁷Differences between the first and second quartiles are negligible. We use 2017 household per capita income quartiles because household income is not asked for February 2020.

⁸We use 2017 contract status because in NIDS-CRAM this question is only asked of individuals who remain employed in April.

significant at the 95% level, except for the tertiary versus non-tertiary comparison (Figure B2 Panel (a)). 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. Similarly to Ranchhod and Daniels (2020), we also find significant racial disparities in active employment loss, with Blacks faring much worse than Whites (Figure B2 Panel (a)). We do not report this racial breakdown in Figure 1 because while the differences between Black and Whites are statistically significant, confidence intervals are large. This is unsurprising because Whites are only 3.7% of our sample.

3.3 Earnings

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

Looking at changes within individuals who remained actively employed in February and April, Figure 2 shows no statistically detectable change in wages on average.⁹ 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 that the confidence intervals associated with these estimates are wide, 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

⁹To 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.

changes in *employment* status between February and April (with consequences for *unconditional* earnings) constitute the more salient aspects of the COVID-19 labor market shock than the intensive-margin changes in wages.

4 Social protection

Realized changes in household income will be jointly determined by the earnings shock as well as the compensatory social protection benefit. Therefore before moving to our analysis of poverty impacts, we discuss here the reach of social protection among affected workers. As discussed in Section 1, the main form of emergency social insurance implemented for workers in April was the Temporary Employee/Employer Relief Scheme (TERS). Additional emergency social assistance interventions – that is, the social grants’ expansions – were only introduced in May. Therefore during the period of our data collection, social assistance remained unchanged relative to February. Nevertheless, the existing social grant system would have provided some cushioning of the COVID-19 labor market shock.¹⁰

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 3, Panel (a)). Only 20% of these workers received TERS. 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 in TERS.¹¹ 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 is not applicable in cases where the employment relation is completely severed. For job-losers who shift into non-employment, we therefore examine social protection

¹⁰We discuss this further in Section 6.

¹¹We follow Ranchhod and Daniels (2020) in disaggregating by February earnings quartiles.

coverage by looking at co-residency with different types of household grant receipt, distinguishing between Child Support Grant (CSG) receipt and other grant receipt¹² (Figure 3, Panel (b)). We find that close to a third of these workers are not covered by any kind of social protection measure, 39% receive only the CSG in their household, 9% receive other household-level grants but no CSG, and 20% of these workers receive both the CSG and some other grant in their household. Across different groups of job-losers, household grant receipt is clearly progressive, with women, those at the bottom of the February earnings distribution, and informal workers being much more likely to have a grant recipient in the household. This is mainly driven by the progressively targeted CSG (Bassier et al., 2020).

That two thirds of non-employed job-losers have a grant in their household reflects both the progressivity of the grant system and the regressivity of the labor market shock (as reflected by larger net job loss for lower household income quartiles in Figure 1). However, it is important to bear in mind that being “covered” by household grant receipt will frequently not preclude descent into poverty. This is because the vast majority of these grant recipients live in multi-member households (Bassier et al., 2020), and the monetary value of each grant is small compared to the magnitude of the labor market shock, especially the CSG.¹³

¹²While the Social Relief of Distress (SRD) Grant was introduced explicitly to target otherwise unprotected job-losers, too few respondents report receiving this grant to merit its inclusion as a stand-alone category. Instead, it is included within the “other grants” category.

¹³The value of the Child Support Grant was R440 (\$66 PPP) per month per eligible child before it was increased in May. For reference, the monthly national minimum wage in 2019 was R3,500 (\$524 PPP).

5 Welfare impacts

5.1 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.¹⁴

However, we can see which individuals in NIDS-CRAM have 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.¹⁵ However, a clear problem with a naive comparison of incomes of these two groups of workers is that job-losers are likely to be systematically different from job-retainers. As our analysis in Section 3 shows, low-income workers face disproportionately higher rates of job loss.

In order to estimate a job loss “treatment effect”, we create a counterfactual “no job loss” income distribution for the job-losers, by re-weighting 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 removing the selection effect and isolating the treatment effect of job loss. Specifically, we use DiNardo-Fortin-Lemieux (DFL) re-weighting (DiNardo, Fortin, and Lemieux, 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 re-weighted income distribution of the job-retainers.

¹⁴See Appendix A for discussion of this.

¹⁵In our analysis here, “job loss” means a shift from active employment into either a temporary lay-off or a fully severed employment relationship, while “job retention” means staying actively employed. Those shifting into paid leave are omitted.

The key assumption needed for our procedure to identify the causal effect of job-loss on income is *ignorability*, sometimes also called *unconfoundedness* (Fortin, Lemieux, and Firpo, 2011).¹⁶ This is a weaker assumption than conditional independence: we do not require that unobservable determinants of income be independent of covariates, but that the conditional distributions of these unobservables, given covariates, are the same for job-losers and job-retainers. Loosely, this “selection on observables” allows for selection biases as long as they are the same for job-losers and job-retainers (Fortin, Lemieux, and Firpo, 2011).¹⁷

While ignorability is weaker than conditional independence, it is not a trivial assumption. In order for the conditional distribution of unobservables to be plausibly similar across job-losers and job-retainers, we need a rich set of observable characteristics which predict job loss, and which can be controlled for. Additionally, these observable characteristics need to be “pre-treatment” – we do not want to control for post-treatment outcomes. Fortunately, 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 pre-treatment 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.

Empirically our procedure seems to be effective. Estimated propensity scores are reasonably well-balanced across job-losers and job-retainers, and not many observations are dropped due to propensity scores below the 1st or above the 99th percentiles of job-

¹⁶We also need standard common support and “no general equilibrium effects” assumptions (Fortin, Lemieux, and Firpo, 2011).

¹⁷To take a canonical example, consider unobserved “ability” which is positively correlated with both income and education, and abstract from other determinants of income. While a linear regression of income on education will yield biased estimates of the return to education, comparing incomes of those with similar education will still yield the treatment effect we are interested in so long as the dependence structure between ability and education is the same for job-losers and job-retainers (Fortin, Lemieux, and Firpo, 2011).

losers’ propensity scores (Panel (a) of Figure 4).

Figure 4 Panel (b) presents a type of “placebo” test for our procedure. We repeat the LASSO DFL procedure outlined above, but do not include NIDS 2017 household income (or its major components) in the set of variables from which the LASSO regression selects. 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 we re-weight the job-retainers according to our DFL weights, the re-weighted job-retainer 2017 income density (dashed) is remarkably similar to the unadjusted job-loser 2017 income density, especially in the regions to the left of the two indicated poverty lines. 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.

These results suggest that the re-weighting successfully accounts for selection, giving us confidence that differences between 2020 income densities of job-losers and re-weighted job-retainers will approximate the poverty treatment effects.

5.2 Results

Figure 5 Panel (a) shows that the job-loser 2020 income density (solid) is substantially to the left of the re-weighted job-retainer income density (dashed), and that therefore job loss did indeed decrease household income in 2020.

The dashed and dotted lines of Figure 5 Panel (b) show April poverty headcount ratios, for job-losers and re-weighted 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 re-weighted job-retainers, which as per our DFL procedure is the poverty “treatment effect” of job-loss, for each given poverty line.¹⁸

We estimate that between 20% and 33% 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 14-26 percentage points, while at the \$1.90-a-day line the equivalent increase is 26-40 percentage points.

5.3 Interpretation and robustness checks

The job loss specification allows an estimate of a very 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.

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 Appendix C, but prefer the job-loss specification because the household income loss questions appear to be of poor quality (as we discuss in greater detail in Appendix C). Results using this method are in any case qualitatively similar to our preferred specification.

We undertake additional robustness tests in Appendix C. In Figure C2 Panel (a) we show the same placebo test for February 2020 earnings as we do for 2017 household income. The re-weighting seems to work well for the bottom two-fifths of the earnings distribution – likely the region most relevant for our poverty analysis – though it is less successful

¹⁸95% confidence intervals are shown; these come from bootstrapping the estimation procedure from the estimation of the logit model to construct the DFL weights up to the computation of the difference in headcount ratios. This bootstrapping procedure is analogous to that usually used for DFL re-weighting (Fortin, Lemieux, and Firpo, 2011) when the researcher manually selects which variables to include in the propensity score estimation.

higher up the earnings distribution (partly due to round-number bunching in reported wages). Given concerns about possible under-reporting of household income and over-reporting of household size in NIDS-CRAM (see Section A), we also check for increases in reported food-insecurity using the same DFL re-weighting procedure – we find that job-losers do report greater food-insecurity than the re-weighted job-retainers (Figure C2 Panel (b)). For our baseline poverty estimates we also 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 versus re-weighted job-retainers. We find very similar results using this approach as we do with our baseline DFL re-weighting procedure (Figure C2 Panel (b)).

6 Discussion

In this section we present a more speculative extension of the poverty analysis in two directions: accounting for dependents in workers’ households, and counterfactually simulating a more immediate and comprehensive delivery of additional social protection measures following the imposition of the lockdown.

6.1 Accounting for dependents

With 5.23 million individuals losing their jobs between February and April, our estimate of 20% to 33% of job-losers falling into poverty is equivalent to an additional 1 million to 1.7 million individuals falling below reasonable poverty thresholds. However, it is important to note that 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 non-employed or temporarily laid-off individuals find active employment in April; the *net* increase in job loss over the period is 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 effect of net job *transitions* between February and April will be between 0.8 and 1.4 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.

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 a worker losing her job. Unfortunately, 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 social impact of job-loss-induced poverty estimated in the previous section, we estimate the average number of dependents each job-loser supported in NIDS 2017.¹⁹ 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 1 to 1.7 million job-losers into poverty, we conjecture a broader job-loss effect of approximately 3 million to 5.5 million individuals falling into poverty, when accounting for the dependents of job-losers.

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

6.2 Expanded social protection

South Africa went into lockdown at the end of March 2020, but only announced additional social assistance measures – in the form of social grant expansions – at the end of April. Here we present a counterfactual poverty simulation where we estimate how the grant

¹⁹We divide each job-loser’s 2017 household size by the total number of workers in the household.

increases would have mitigated the poverty of job-losers and income losers if they had been implemented from the beginning of April. Reflecting the grant increase values, we add R250 (\$38.5 PPP) per Old Age Pension and R300 (\$46 PPP) per Child Support Grant to April household monthly income. We then compare headcount ratios for job-losers under this counterfactual scenario to the headcount ratios actually observed. We find that the grant increases would decrease poverty among job-losers by 11 and 1 percentage points for the \$1.90 and upper bound poverty lines respectively.

We perform a similar counterfactual poverty simulation imposing perfect implementation of the COVID-19 social insurance mechanism (TERS). If social insurance was implemented perfectly, all workers who were temporarily laid-off should instead have received TERS and been placed on paid leave. We therefore impose that all temporarily laid off workers receive TERS, and add the projected payout to their household incomes.²⁰ Comparing headcount ratios for job-losers under this counterfactual scenario to the headcount ratios actually observed, we find poverty would have decreased by 7 and 4 percentage points for the \$1.90 and upper bound poverty lines respectively. If we combine the two exercises above, i.e. assume earlier introduction of grant expansion *and* perfect implementation of social insurance, the corresponding decreases in poverty are 16 and 7 percentage points.

While job-loss-induced poverty (33 and 20 percentage points at the two poverty lines respectively) is on net larger than the protection offered, we estimate that this combined additional social protection would have mitigated the poverty increase associated with job-loss by 35% to 50%.

²⁰ As per the TERS regulations, the payout in Rands is calculated as $\min [\max (3500, 0.6 \times w_{\text{feb}}), 6600]$, where w_{feb} is the February wage.

7 Conclusion

The COVID-19 crisis has resulted in a large negative labor market shock globally. We use newly-released South African data to provide some of the first pieces of evidence on the impact of COVID-19 on employment and poverty in the developing world. To the best of our knowledge, our paper is the first to directly estimate the impact of the COVID-19 labour market shock on poverty in any country.

We observe a 40% decline in net active employment between February (pre-lockdown) and April (during lockdown) of 2020, with approximately half of this being comprised of shifts into non-employment rather than paid leave or temporary layoffs. The incidence of employment losses has been much greater among female, manual and informal workers, and the poor, thereby exacerbating existing inequalities. The already-existing social grant system accounted for the bulk of social protection reaching job-losers. We document that only 20% those who lost active employment by April received relief through new COVID-19 social insurance mechanisms. Approximately one third of job-losers did not receive any household-level social protection at all.

Finally, we estimate that 20-33% of those who have lost jobs over the lockdown period fell into poverty, which translates to between 1 and 1.7 million job-losers. Accounting for dependents, we tentatively estimate that between 3 and 5.5 million individuals fell into poverty as a result of this job-loss. In our counterfactual simulations, we estimate that poverty among job-losers could have been substantially reduced by better implementation of COVID-19 social protection policies.

Figure 1: Employment Status for Working-Age Adults (18-64 years)

(a) Feb., Apr. and Net Change Over the Lockdown

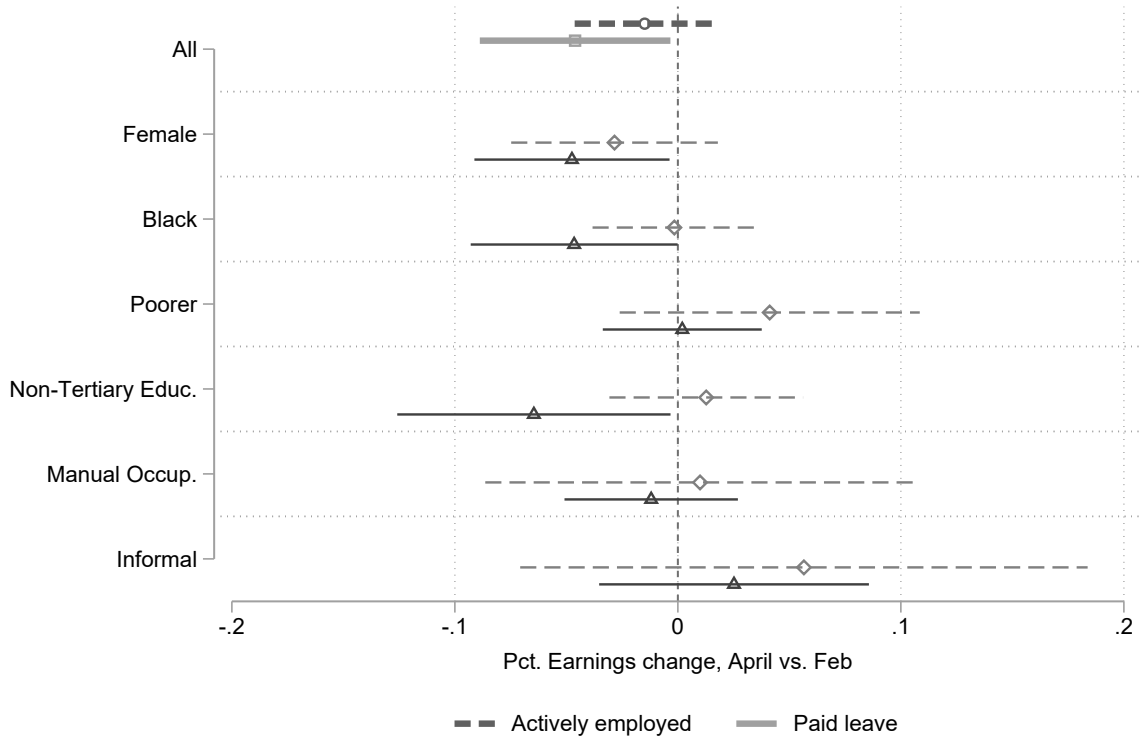


(b) Composition of Net Active Employment Loss by Worker Characteristics



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 indicate the proportion of adults by their employment status for February (before the lockdown), while the middle group of bars show 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 Feb, 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.

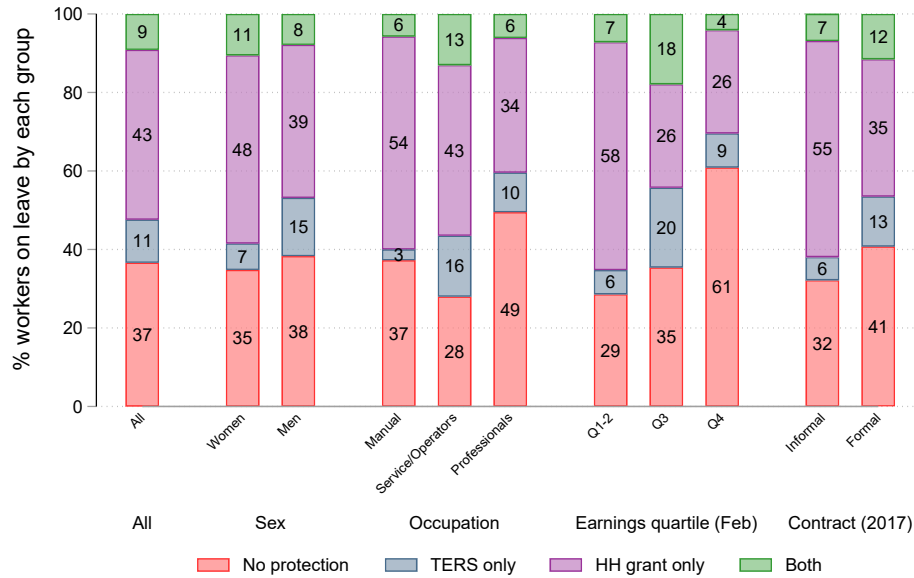
Figure 2: Change in Earnings for those paid in February and April



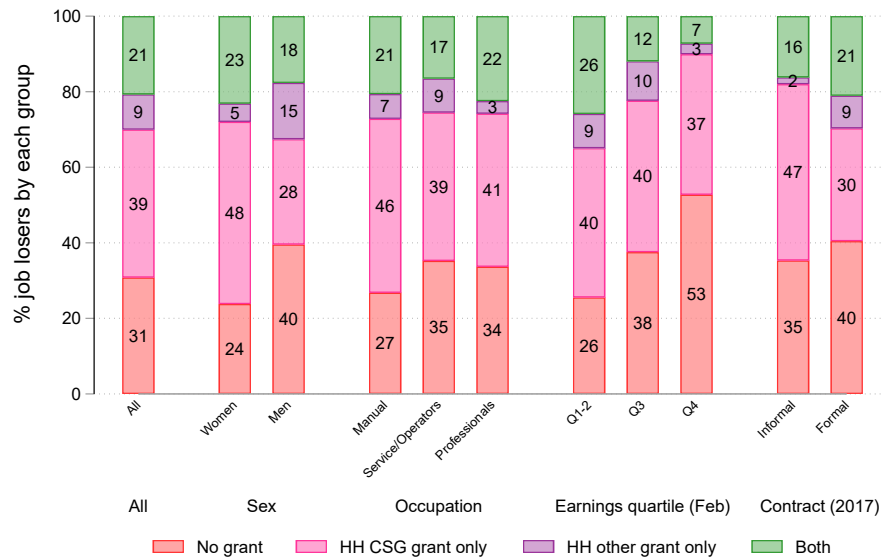
Notes: The figure shows the percent change in earnings for those workers who were actively working in February and either continued to work actively in April (dashed line) or were on paid leave in April (solid line). The first row shows the average change for all workers in these categories while the 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.

Figure 3: Social Protection Coverage for Workers No Longer Actively Employed

a) Workers who were actively employed in Feb but temporarily laid-off or on paid leave in April



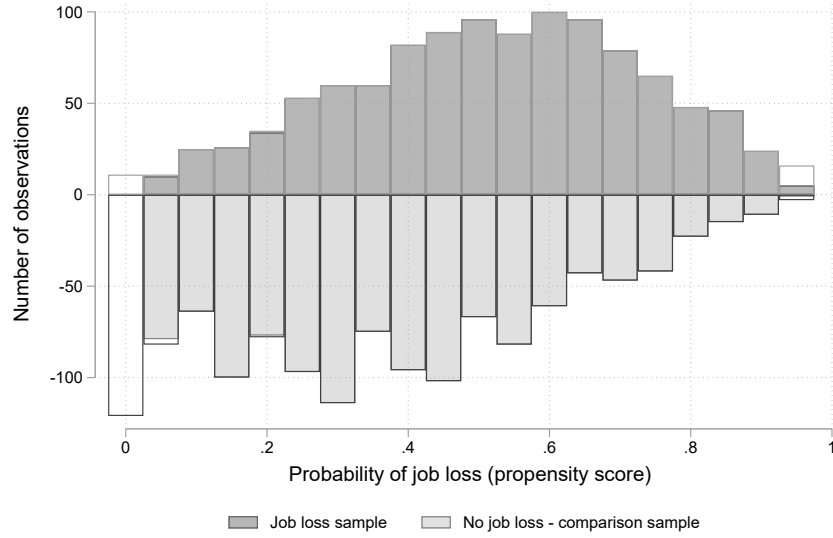
b) Workers who were actively employed in Feb but not employed in April



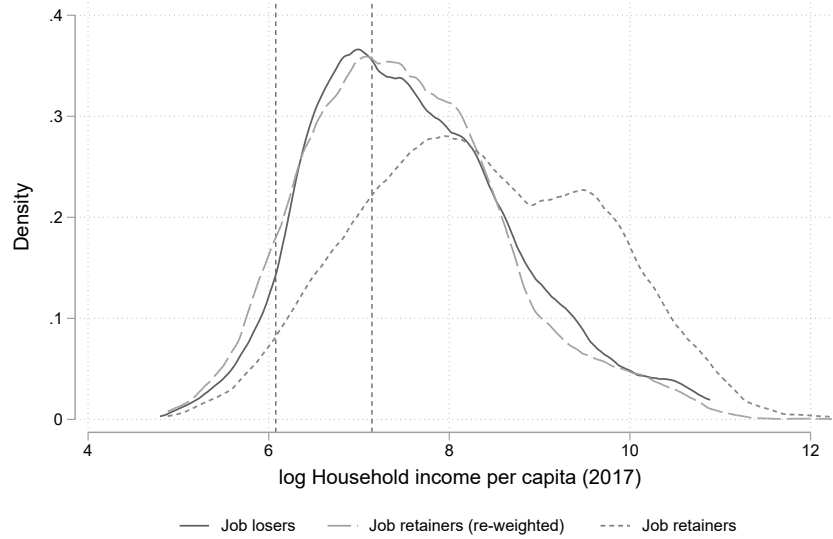
Notes: Panel (a) shows coverage of social protection for those who were employed in February and are “temporarily laid-off” or on “paid leave” in April. Grant receipt refers to receiving a grant in their household. Red sub-bars show the percentage in each category who are not reached by any type of grant 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 versus any other social grant. Estimates are weighted using the survey design weights.

Figure 4: DFL Re-weighting: Diagnostics Tests

(a) Propensity Score Overlap for Job Loss



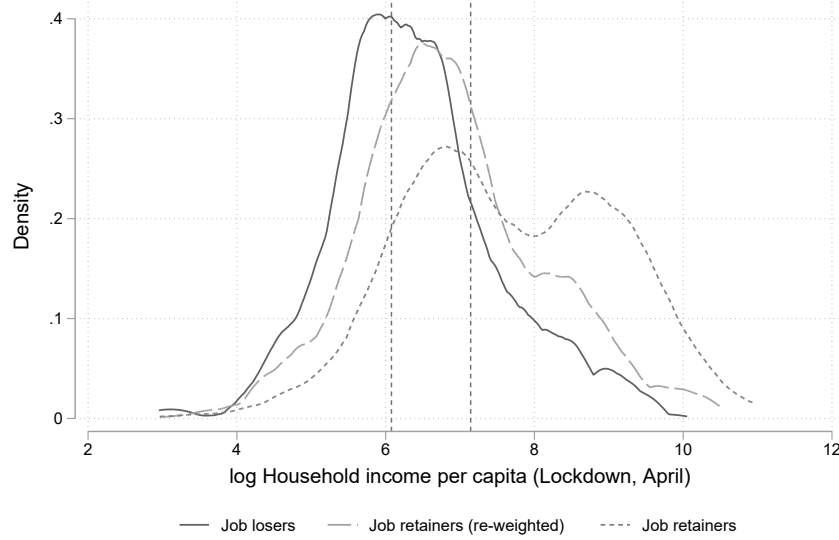
(b) Placebo Test for Job Loss



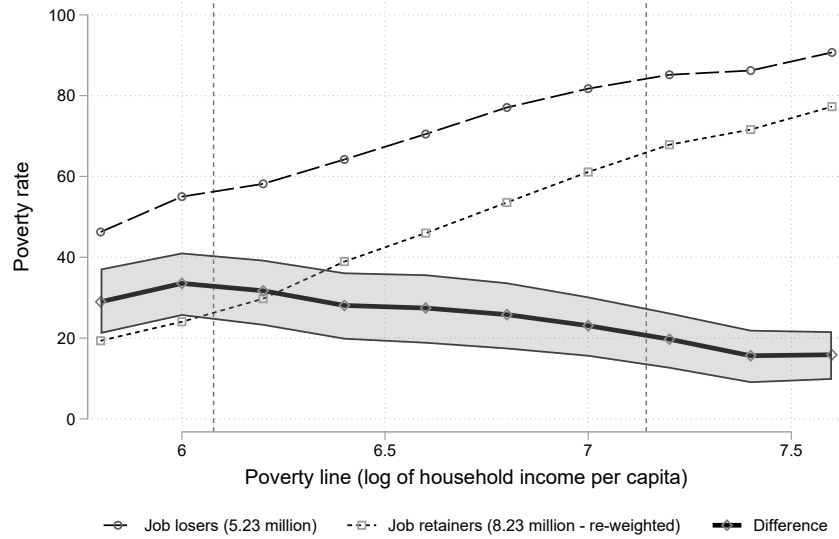
Notes: The figure presents evidence on the quality of our re-weighting procedures, where we DFL re-weight job-retainers to match job-losers. Panel (a) shows the overlap in job-loss propensity scores for job-losers and job-retainers. Empty bars show observations which are dropped from our matched sample because their propensity scores are below the 1st percentile or above the 99th percentile of propensity scores for job-losers. Panel (b) presents a type of placebo test, showing how our job-loss re-weighting procedures successfully re-weight job-retainers (from 2020) such that their re-weighted 2017 income distribution closely 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 5: Welfare Effects of Job Loss

(a) Effect of job loss on household income per capita



(b) Effect of job loss on poverty



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 job-retainers after DFL re-weighting. 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 re-weighting (dotted line). Their difference is shown by the solid line shaded with the associated 95% confidence interval (1,000 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.

References

- [1] Adams-Prassl, A., Boneva, T., Golin, M., and Rauh, C. *Inequality in the Impact of the Coronavirus Shock: Evidence from Real Time Surveys*. Working Paper 13183. IZA Institute of Labour Economics, Apr. 2020.
- [2] Alon, T., Doepke, M., Olmstead-Rumsey, J., and Tertilt, M. *The Impact of COVID-19 on Gender Equality*. Working Paper 26947. National Bureau of Economic Research, Apr. 2020.
- [3] Barrero, J.M., Bloom, N., and Davis, S.J. *COVID-19 Is Also a Reallocation Shock*. Working Paper 27137. National Bureau of Economic Research, May 2020. DOI: 10.3386/w27137.
- [4] Bartik, A., Bertrand, M., Cullen, Z., Glaeser, E., Luca, M., and Stanton, C. *How Are Small Businesses Adjusting to COVID-19? Early Evidence From a Survey*. Working Paper 26989. National Bureau of Economic Research, Apr. 2020.
- [5] Bassier, I., Budlender, J., Leibbrandt, M., Ranchhod, V., and Zizzamia, R. *Locked down and locked out: Repurposing social assistance as emergency relief to informal workers*. Working Paper 261. SALDRU, University of Cape Town, Apr. 2020.
- [6] Béland, L., Brodeur, A., and Wright, T. “The short-term economic consequences of Covid-19: exposure to disease, remote work and government response”. In: Discussion Paper Series 13159 (Apr. 2020).
- [7] Branson, N. *Longitudinal and Cross sectional Weights in the NIDS data 1-5*. Technical Note. Cape Town: Southern African Labour and Development Research Unit, 2018.
- [8] Brophy, T., Branson, N., Daniels, R., Leibbrandt, M., Mlatsheni, C., and Woolard, I. *National Income Dynamics Study Panel User Manual*. Technical Note Release

2018. Version 1. Southern Africa Labour and Development Research Unit, 2018.
URL: <https://www.datafirst.uct.ac.za/dataportal/index.php/catalog/712>.
- [9] Bui, T.T.M., Button, P., and Picciotti, E.G. *Early Evidence on the Impact of COVID-19 and the Recession on Older Workers*. Working Paper 27448. National Bureau of Economic Research, June 2020.
 - [10] Chetty, R., Friedman, J.N., Hendren, N., Stepner, M., and The Opportunity Insights Team. *How Did COVID-19 and Stabilization Policies Affect Spending and Employment? A New Real-Time Economic Tracker Based on Private Sector Data*. Working Paper 27431. National Bureau of Economic Research, June 2020.
 - [11] Coibion, O., Gorodnichenko, Y., and Weber, M. *Labor Markets During the COVID-19 Crisis: A Preliminary View*. Working Paper 27017. National Bureau of Economic Research, Apr. 2020.
 - [12] Cowan, B.W. *Short-run Effects of COVID-19 on U.S. Worker Transitions*. Working Paper 27315. National Bureau of Economic Research, June 2020.
 - [13] Department of Labour, Republic of South Africa. *COVID-19 Temporary Employee/Employer Relief Scheme (C19 TERS). Notice 215 of 2020*. Policy Document. Mar. 26, 2020.
URL: https://www.gov.za/sites/default/files/gcis_document/202003/43161gen215.pdf (visited on 04/25/2020).
 - [14] DiNardo, J., Fortin, N., and Lemieux, T. “Labor Market Institutions and the Distribution of Wages, 1973-1992: A Semiparametric Approach”. In: *Econometrica* 64.5 (1996), pp. 1001–1044.
 - [15] Fortin, N., Lemieux, T., and Firpo, S. “Decomposition methods in economics”. In: *Handbook of Labor Economics*. Vol. 4. Elsevier, 2011, pp. 1–102.

- [16] Gentilini, U., Almenfi, M., Dale, P., Blomquist, J., Natarajan, H., Galicia, G., Palacios, R., and Desai, V. *Social Protection and Jobs Responses to COVID-19: A Real-Time Review of Country Measures (May 22 version)*. Live Document. World Bank, Washington, DC., 2020. URL: <https://www.ugogentilini.net/>.
- [17] Gerard, F., Imbert, C., and Orkin, K. *Social Protection Response to the COVID-19 Crisis: Options for Developing Countries*. Policy Brief. Economics for Inclusive Prosperity, Apr. 2020. URL: <https://econfip.org/policy-brief/social-protection-response-to-the-covid-19-crisis-options-for-developing-countries/>.
- [18] Hassan, T., Hollander, S., van Lent, L., and Tahoun, A. *Firm-level Exposure to Epidemic Diseases: Covid-19, SARS, and H1N1*. Working Paper 26971. National Bureau of Economic Research, Apr. 2020.
- [19] Ingle, K., Brophy, T., and Daniels, R.C. *National Income Dynamics Study – Coronavirus Rapid Mobile Survey (NIDS-CRAM) panel user manual*. Technical Note Version 1. Cape Town: Southern Africa Labour and Development Research Unit, July 2020.
- [20] International Labour Organisation. *Statistics of work and of the labour force*. Report MESEU/2013. 2013. URL: https://www.ilo.org/wcmsp5/groups/public/---dgreports/---stat/documents/event/wcms_175150.pdf.
- [21] International Labour Organisation. *ILO Monitor: COVID-19 and the world of work. Third edition*. Report. Apr. 29, 2020. URL: https://www.ilo.org/wcmsp5/groups/public/@dgreports/@dcomm/documents/briefingnote/wcms_743146.pdf.
- [22] Kartseva, M.A. and Kuznetsova, P.O. “The economic consequences of the coronavirus pandemic: which groups will suffer more in terms of loss of employment and income?” In: *Population and Economics* 4 (2020), p. 26.

- [23] Kerr, A. “Job flows, worker flows and churning in South Africa”. In: *South African Journal of Economics* 86 (2018), pp. 141–166.
- [24] Kerr, A. and Thornton, A. *Essential workers, working from home and job loss vulnerability in South Africa*. Technical Paper 41. DataFirst, University of Cape Town, Apr. 2020.
- [25] Mahler, D., Laknner, C., Aguilar, A., and Wu, H. *Updated estimates of the impact of COVID-19 on global poverty*. Blog. The World Bank, June 8, 2020. URL: <https://blogs.worldbank.org/opendata/updated-estimates-impact-covid-19-global-poverty>.
- [26] Parolin, Z. and Wimer, C. *Forecasting estimates of poverty during the COVID-19 crisis*. Tech. rep. Center on Poverty and Social Policy at Columbia University, Apr. 2020.
- [27] Ranchhod, V. and Daniels, R.C. *Labour market dynamics in the time of COVID-19 in South Africa: Evidence from the NIDS-CRAM survey*. Policy Paper. SALDRU, University of Cape Town, July 2020.
- [28] Rothwell, J. “The effects of COVID-19 on international labor markets: An update”. In: Middle Class Memos (May 2020). URL: <https://www.brookings.edu/research/the-effects-of-covid-19-on-international-labor-markets-an-update/>.
- [29] SALDRU. *National Income Dynamics Study 2017, Wave 5 [dataset]*. Dataset. 2017.
- [30] SALDRU. *National Income Dynamics Study-Coronavirus Rapid Mobile Survey (NIDS-CRAM) 2020 Wave 1 [dataset]*. Dataset. 2020.
- [31] Statistics South Africa. *National Poverty Lines*. Statistical Release P0310.1. Statistics South Africa, July 2019.

- [32] Sumner, A., Hoy, C., and Ortiz-Juarez, E. *Estimates Of The Impact Of Covid-19 On Global Poverty*. Working Paper 43. UNU-WIDER, Apr. 2020.

Appendices

A Data and Sampling

In this data appendix we provide more detail on sampling, data quality and comparability across NIDS and NIDS-CRAM data. The full questionnaire and the data used can be found here at the NIDS-CRAM website.

A.1 Sampling Frame and Response Rates

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. NIDS was an individual-level panel, but was administered as a household survey. That is, *individuals* were followed over time, but in each wave of data collection individual-level questionnaires were administered to each household member (or a proxy questionnaire on behalf of absent members) and a household-level questionnaire was administered to the eldest female or the household member most knowledgeable about household matters. The fifth wave of NIDS, surveyed in 2017, was the most recent survey round.

The sample selected (based on age-eligibility) for the NIDS-CRAM mobile-phone survey was 17,568 out of approximately 30,000 individuals in NIDS Wave 5. The response rate was approximately 40%. The vast majority of non-response 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 non-response (Ingle, Brophy, and Daniels, 2020). In earlier waves of NIDS, which were roughly two years apart, between-wave attrition was between 20-30 percent and the refusal rate was around 3% (Branson, 2018). The final sample consists of 7,074 successfully completed interviews²¹.

The drivers of attrition in NIDS may differ systematically from NIDS-CRAM. In

²¹2 of the 7074 respondents reported an age of 17. We drop these observations from our sample.

NIDS, while the source of attrition was mostly high income earners (Brophy et al., 2018), between Wave 5 of NIDS and NIDS-CRAM, we suspect attrition is likely to be higher among those who are harder to reach telephonically – a characteristic which is more likely to correlate with *low* income.

Non-response adjustments, however, were made during the data collection process, by oversampling strata with low response rates using a batch sampling method. Each stratum was defined by a combination of 2017 household per capita income decile, race, age and urban/rural for a total of 99 strata. Sampling from each stratum was adjusted flexibly in response to the response rate in the previous batch (Ingle, Brophy, and Daniels, 2020). After data collection, non-response was further corrected with the production of post-stratified weights.

A.2 Comparability of NIDS and NIDS-CRAM

An important advantage of the longitudinal survey design is that NIDS-CRAM respondents can be linked back to their records in earlier NIDS waves. In addition to being useful for our econometric analysis, this also offers us the opportunity to report on the likely direction of over- or under-reporting of key variables in the NIDS-CRAM mobile survey, compared to the NIDS in-person survey. This is unlike many other recent rapid phone surveys conducted to measure the impact of COVID-19, which in the absence of a longitudinal in-person survey as a pre-cursor cannot identify these potential biases.

However, there are a number of features of NIDS-CRAM that limit our analysis and affect comparability to previous waves of NIDS and other sources of South African household survey data.

First, NIDS-CRAM did not survey all individuals co-resident with the original sample member as in previous NIDS waves, and there is no NIDS-CRAM household roster identifying individuals who are in the same household. As a result, unlike the previous

waves of NIDS, we must use income reported from a one-shot question – household income cannot be derived through aggregating individual income item-responses across the household as was done in NIDS (Brophy et al., 2018). Figure A1 shows that, using the NIDS 2017 data for adults in the NIDS-CRAM sample, the distribution of 2017 *one-shot* household income is substantially lower than the distribution of a 2017 *derived* household income. In NIDS-CRAM too, the distribution of the one-shot household income variable is substantially lower than the distribution of a roughly imputed 2020 household income variable (Figure A1).²² In both NIDS 2017 and NIDS-CRAM 2020, the distribution of one-shot household income is lower than that of a derived household income variable. NIDS-CRAM household income figures ought to be interpreted with this in mind.

Second, household income was only reported for April and not for February. This limits direct analysis of income changes over time and motivates our re-weighting strategy in Section 5. Third, while in NIDS, household-level questions were only asked of the eldest woman or household member most knowledgeable about household affairs, in NIDS-CRAM each respondent was surveyed on household-level questions, such as household income. Fourth, while data collection happened over May and June, earnings and employment status are reported retrospectively for February and April.

The methodological approaches adopted in this paper attempt to deal directly with these limitations.

A.3 Occupation categorisation

For clarity of exposition, in several exhibits we collapse the occupation categories in the NIDS-CRAM data into broader groups. NIDS-CRAM classifies occupations into the 10

²²This imputed 2020 household income variable is constructed by replacing the one-shot household income variable with a “lower bound” household income variable which is constructed by summing the limited income item-response variables (such as own earnings and household grant receipt) available in NIDS-CRAM. Where the “lower bound” estimate is higher than the one-shot household income variable, the one-shot variable is replaced with the lower-bound estimate.

International Standard Classification of Occupations (ISCO-08) categories.²³ Our broader occupation categories group these ISCO-08 groups into three broader categories:

1. “Managers”, “Professionals” and “Technicians and associate professionals” are classified as “Professionals”
2. “Clerical support workers”, “Service and sales workers”, “Skilled agricultural, forestry and fishery workers”, “Craft and related trades workers” and “Plant and machine operators, and assemblers” are classified as “Service/Operators”
3. “Elementary occupations” is reclassified as “Manual”.

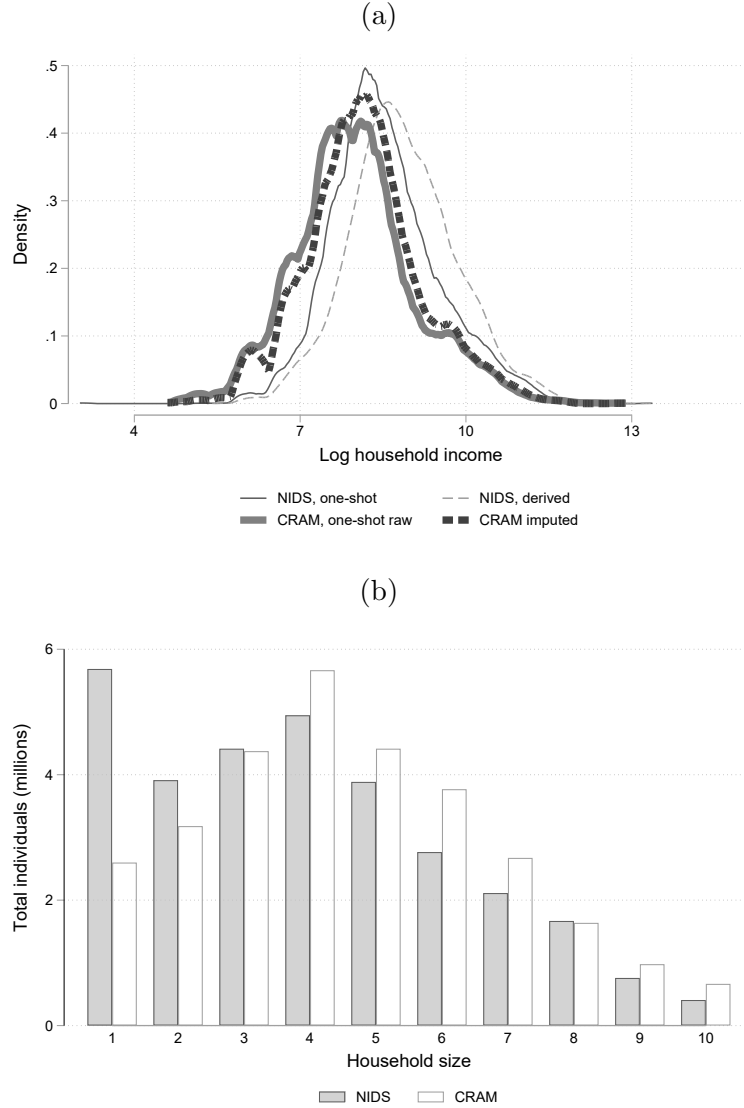
²³Only 3 observations report being in “Armed forces” occupations. These observations are dropped from our adapted occupation categorisation.

Table A1: Sample summary statistics, NIDS vs NIDS-CRAM

NIDS-CRAM			NIDS Wave 5 sample		
	Mean	SD		Mean	SD
Age			Age		
18-35	0.46	0.498	18-35	0.55	0.497
36-50	0.39	0.489	36-50	0.26	0.437
Over 50	0.15	0.356	Over 50	0.19	0.392
Female	0.60	0.489	Female	0.56	0.496
Black	0.96	0.189	Black	0.94	0.236
Education			Education		
Up to Primary	0.14	0.343	Up to Primary	0.19	0.390
Up to Secondary	0.39	0.487	Up to Secondary	0.45	0.498
Matric	0.25	0.433	Matric	0.20	0.403
Tertiary	0.23	0.419	Tertiary	0.15	0.360
Urban	0.78	0.416	Urban	0.58	0.494
Occupation			Occupation		
Professionals	0.21	0.406	Professionals	0.21	0.410
Service/Operators	0.51	0.500	Service/Operators	0.49	0.500
Manual	0.28	0.451	Manual	0.29	0.454
Household income per capita			Household income per capita		
Quartile 1	0.27	0.445	Quartile 1	0.27	0.443
Quartile 2	0.27	0.446	Quartile 2	0.27	0.446
Quartile 3	0.26	0.439	Quartile 3	0.25	0.433
Quartile 4	0.19	0.396	Quartile 4	0.21	0.406
Observations	6377		Observations	21978	

Notes: The table gives the mean and standard deviation for selected variables from the NIDS-CRAM and NIDS Wave 5 (2017) samples for adults aged 18-64. All statistics are from the unweighted raw samples.

Figure A1: Household Income and Size: NIDS Wave 5 and NIDS-CRAM

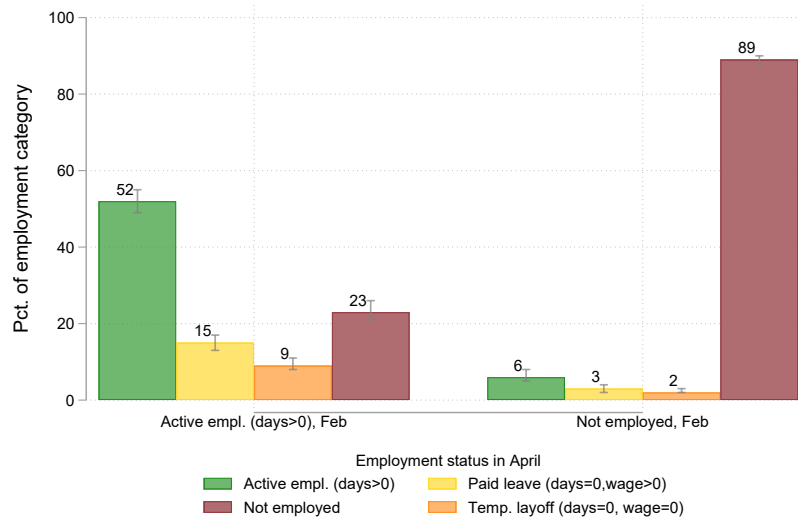


Notes: Panel (a) shows the densities of household income in NIDS Wave 5 (2017) and NIDS-CRAM (2020). Solid lines show the density using the raw one-shot reported income from the survey, while dashed lines show densities after imputations. The NIDS Wave 5 derived variable adds up the component parts of household income using the household co-resident roster, in addition to imputations for outliers. The imputation we use for NIDS-CRAM is replacing household income with a lower bound constructed by summing across an individual's reported earnings, and income due to social grants they report receiving individually or by a household member. Panel (b) shows household size as reported by individuals in NIDS Wave 5 and NIDS-CRAM.

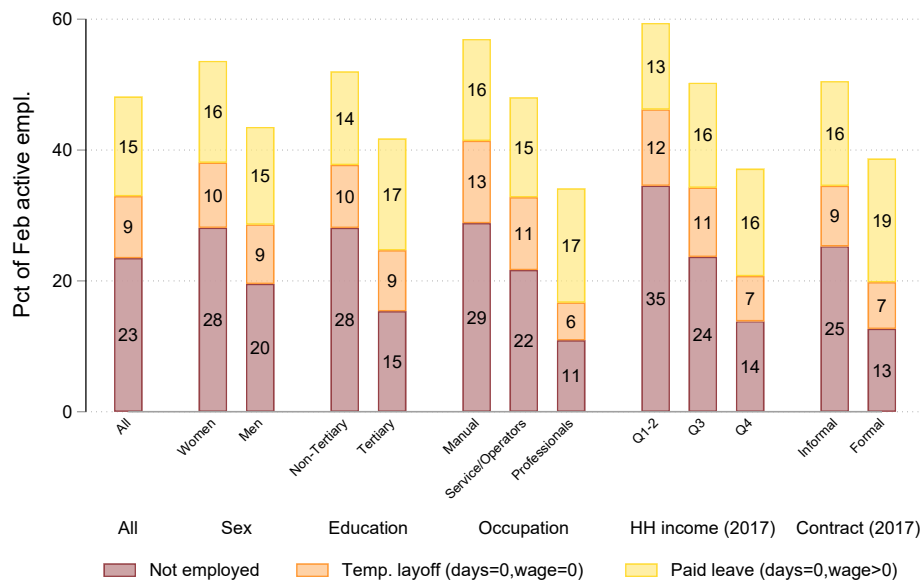
B Additional employment figures

Figure B1: Individual Employment Status Changes Over Lockdown

(a) Individual Employment Transitions from Feb. to Apr.



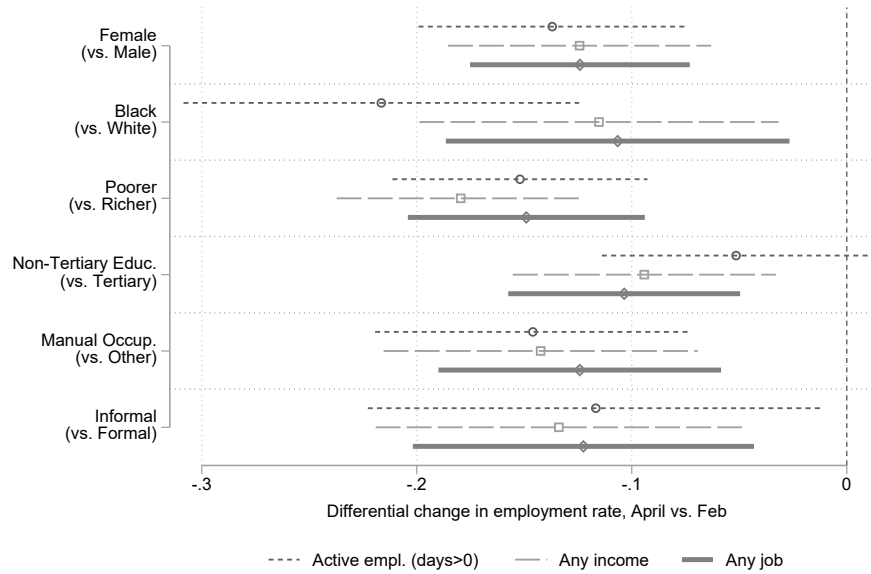
(b) April employment status for February actively employed, by worker characteristics



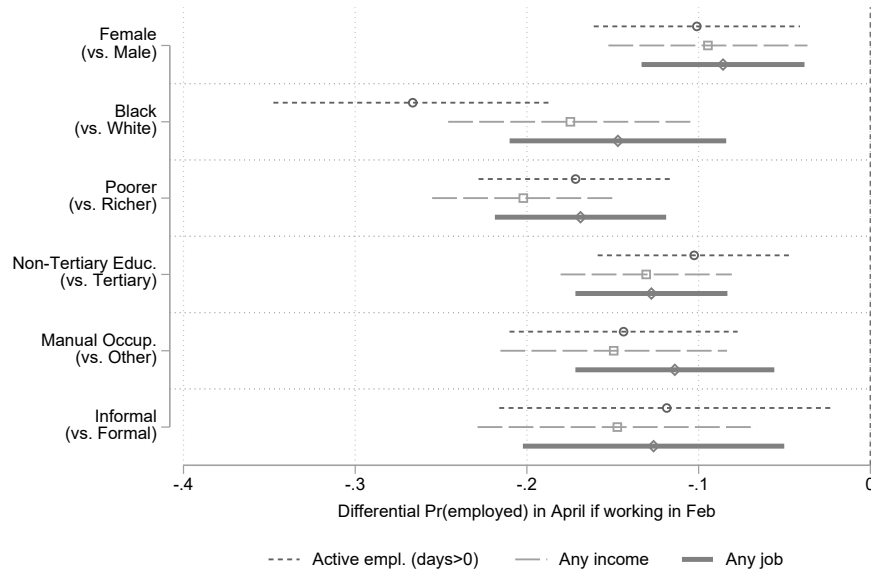
Notes: Panel (a) shows the job status transition patterns of individuals who were actively employed in February (left group of bars) and who were “Not Employed” in February (right group of bars) into the four employment categories in April. In Panel (b), the sum total in each bar is the percent of February actively employed who were no longer actively employed in April. Each sub-bar decomposes these shifts out of employment into categories of “Paid Leave”, “Temporary layoff” and “Not employed”. Note that the “Not employed” category includes those not economically active. While Figure 1 shows the *net* change in employment, this figure focuses on job losers and does not account for job finders. Estimates are weighted using the survey design weights.

Figure B2: Heterogeneity in Employment Rates

(a) Net changes in employment by worker characteristics



(b) Individual-level probability of April employment by worker characteristics for February employed



Notes: Panel (a) shows the net percentage change in employment between February and April (as a percentage of active employment in February) along various worker characteristics. Panel (b) plots the change in individual-level employment status over February and April along various worker characteristics for workers who were actively employed in February. Active employment denotes positive days of work; Any income denotes any wage (either actively employed or on paid leave); and any job denotes any employment relationship (actively employed, paid leave or temporarily laid off). Standard errors are clustered and stratified following the survey design, while estimates are weighted using the survey design weights.

C Robustness and additional results: poverty estimates

In this appendix we present and discuss robustness results associated with our estimation of COVID-19 poverty effects.

C.1 Poverty increase from any income loss

The local treatment effect from our main “job loss” specification in Section 5 is not a full estimate of COVID-19 induced poverty in South Africa. While job loss is probably the primary event facilitating a shift into poverty, other COVID-19 related factors such as reduced earnings or profits, deaths of earners, and household re-formation may also be important. Ideally, one would have a treatment variable of “income loss” rather than job loss to identify this broader poverty effect. NIDS-CRAM respondents report whether their household has lost income since the lockdown started by answering two questions asking if they experienced 1) any household income decrease and 2) loss of the main source of household income. However, we find that these questions are inconsistently answered. A substantial proportion of respondents simultaneously report no household income decrease in the first question and the loss of the main household income source in the second question. Further, respondents who have lost a job and earnings sometimes report no decreases in household income. Our unease with the inconsistent answering of these questions is the primary reason we prefer our job-loss specification for assessing poverty impacts, but this “income loss” specification is nonetheless useful as a robustness test.

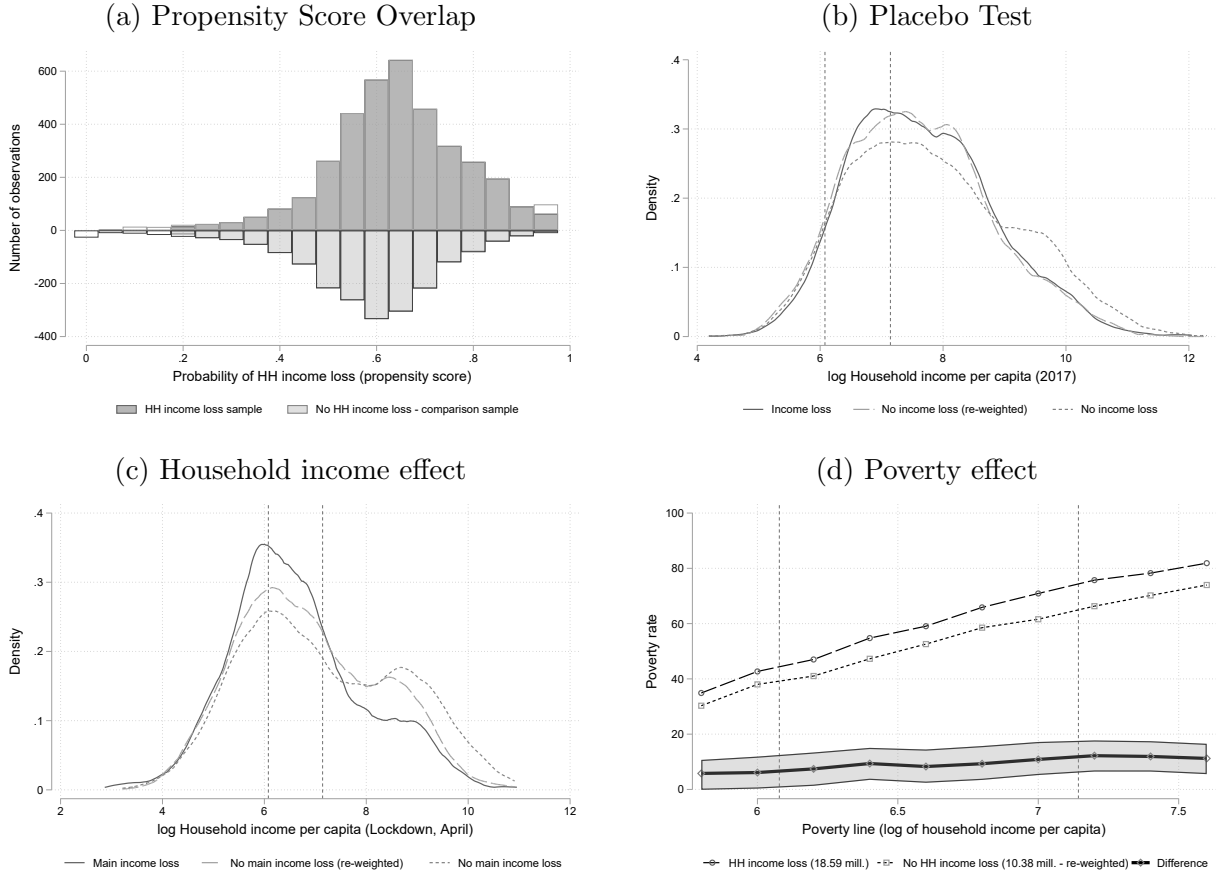
We define household income loss as a binary variable equal to one if an individual reports that their household has lost their main or any source of income, and equal to zero otherwise. When this binary variable is zero and an individual reports they have lost

their job, we set this variable to missing. We do so in order to have the counterfactual as close to a true “no income loss” group as possible. We then implement the same estimation procedure for “income loss” as we do for “job loss” in Section 5.

The propensity score overlap for this income loss specification has a long left tail, but there is significant overlap in the main masses of the densities (Figure C1 Panel (a)). The 2017 per capita household income densities of 2020 income-losers and 2020 re-weighted income-retainers overlap closely (Figure C1 Panel (a)), but it is somewhat concerning that the unadjusted income-retainer density is not very different to the income-loser density – the results of Section 3 lead us to expect that those worst-affected by COVID-19 would be poorer in 2017. Figure C1 Panel (c) reflects this ostensible lack of a substantial selection effect, with the re-weighting having a muted effect on the job-retainer April 2020 income density. The percentage point poverty increase associated with income loss is much smaller than that associated with job loss (Figure C1 Panel (c)), but for comparison needs to be scaled by the size of the affected population.²⁴ Multiplying a 7 percentage point increase at the \$1.90-a-day line and a 12% increase at the Statistics South Africa upper-bound line by 18.59 million income-losing adults, the income loss specification suggests that between 1.3 million and 2.2 million adults fall into poverty as a result of income loss between February and April. These numbers cannot be directly compared to the number of job-losers who fall into poverty (which excludes non-earners) or our estimates of job-loss affected dependents (which includes children), but it is encouraging that the income loss poverty effect falls within the range of numbers associated with our job-loss specification presented in Section 6.

²⁴Note that because logit convergence is slow for this specification, confidence intervals are estimated with 185 bootstrap repetitions rather than the 1000 used in our main job loss specification.

Figure C1: Welfare Effects of Income Loss

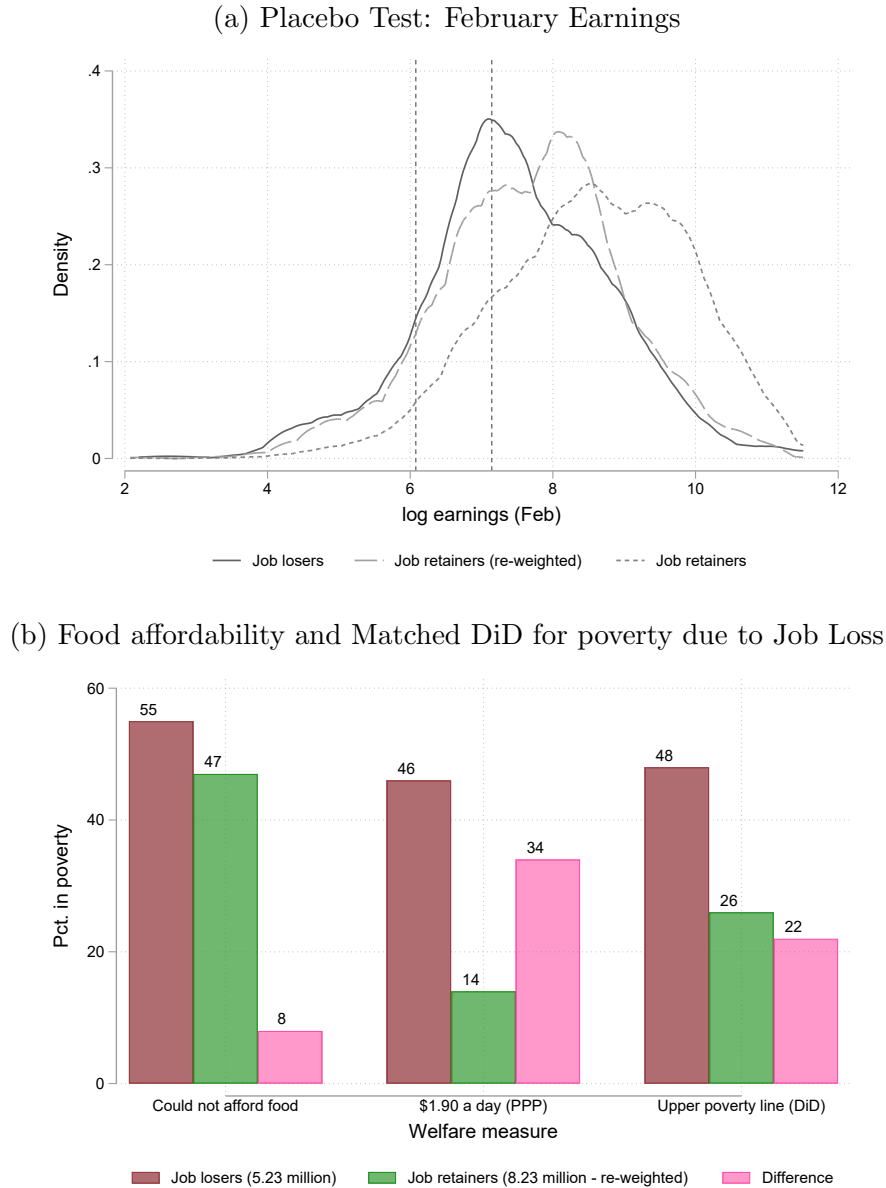


Notes: The figure presents evidence on the poverty impact estimates, where we DFL re-weight (household) income-losers to match household income-retainers. From left to right the dashed vertical lines show the World Bank \$1.90-a-day poverty line (converted in PPP terms) and the Statistics South Africa upper-bound poverty line. Panel (a) shows the overlap in household income loss propensity scores for income-losers and 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 income-losers. Panel (b) presents a type of placebo test, showing how our re-weighting procedures successfully re-weight income-retainers (from 2020) such that their re-weighted 2017 income distribution closely matches the 2017 income distributions of 2020 income-losers. We define household income loss as a binary variable equal to one if one reports that their household has lost their main or any source of income, and equal to zero otherwise. As we note in our exposition, if this is zero and an individual reports they have lost their job, we set this variable to missing in order to reduce inconsistencies due to reporting errors.

Panels (c) and (d) show changes in poverty associated with income loss. Panel (c) shows the household per capita income distributions for income losers (solid line) and for income retainers (dotted line). The dashed line is household income of the income retainers after DFL re-weighting. The difference between the solid and dashed lines reflects the treatment effect of income loss. Panel (d) shows the cumulative density of log household income per capita for income losers (dashed line) and income retainers after re-weighting (dotted line). Their difference is shown by the solid line shaded with the associated 95% confidence interval (185 bootstrap repetitions). This line therefore gives the increase in poverty associated with losing income if the poverty line is defined to be at any point along the x-axis. Estimates are weighted using the survey design weights.

C.2 Main specification robustness

Figure C2: DFL Re-weighting: Additional checks on poverty estimates of job loss



Notes: The figure presents further robustness checks for the quality of our re-weighting procedures, where we DFL re-weight job retainers to match job losers. Panel (a) presents another type of placebo test, comparing the 2020 February earnings distribution for job losers, job retainers, and re-weighted job retainers. Panel (b) shows changes in reported food affordability (left-most set of bars), where the proportion of job losers that report that they could not afford food (red bars) are compared to the DFL re-weighted proportion of job keepers that could not afford food (green bars). It also shows poverty associated with job loss (middle and right-most set of bars). Poverty is measured using household per-capita income being below one of two poverty lines: the World Bank \$1.90-a-day (PPP-adjusted) line and the Statistics South Africa upper-bound poverty line. The Matched DiD estimates show the difference for job losers in poverty between 2020 compared to 2017 (red bars), the difference for DFL re-weighted job retainers in poverty between 2020 compared to 2017 (green bars), and the difference in these differences (pink bars). Estimates are weighted using the survey design weights.