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Simulation of options to replace the special COVID-19 Social Relief of Distress grant and close the poverty gap at the food poverty line

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Abstract: We use a fiscal incidence model based on the South African 2014/15 Living Conditions Survey to simulate the poverty reduction impacts of a selection of medium-to-long-term social grant options with the goal of replacing the existing special COVID-19 Social Relief of Distress grant upon its expiry and closing the extreme (food poverty line) poverty gap. Our key findings are that the introduction of a household-targeted family poverty grant is theoretically able to reduce extreme poverty most efficiently; however, it faces stark implementation challenges. The basic income grant has the largest impact on poverty with its large budget and is possibly easier to implement through the tax system, but ties for the least efficient scenario. A redesigned special COVID-19 Social Relief of Distress grant is similar in budget size to the family poverty grant and presents a middle-ground in terms of efficiency and implementation challenges. Finally, topping up the child support grant is more efficient than implementing a public works programme, but both are small in size, and the former would exclude households without children, while the latter has operational constraints and competing goals, diluting its short-term poverty reduction impact. We find some sensitivity to the poverty line chosen and size of the budget, and strongly recommend that further research be conducted on the implementation aspects of new grants which may substantively change the theoretical results and ranking of options presented here. In addition, the sociological consequences of a possible shift towards a household-based social grants system may prove important.

Key words: fiscal incidence, social protection, social spending, poverty, South Africa

JEL classification: D31, H53, I38

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Overview

This paper is the work of a joint team from National Treasury and SALDRU. We model the poverty reduction impacts of a selection of medium-to-long-term grant social security policy options that could replace the existing Special COVID-19 Social Relief of Distress grant (SRD) upon it's expiry. The broad options modelled here were chosen by National Treasury, and are not exhaustive.

The main objective considered is eliminating income poverty at the 2021/22 Food Poverty Line of ZAR624[1] per person per month. The key metrics chosen for evaluating each grant are value-for-money ('efficiency'), affordability, and total reduction in the Food Poverty Line poverty gap. We estimate the size of the theoretical Disposable income poverty gap at the Food Poverty Line to be R45 billion in 2021/22, whereas the size of an observable income aggregate (which excludes non-verifiable household income components such as imputed rent or in-kind transfers) is closer to R63 billion.

Broadly we consider five types of additions to the existing social grant system, namely: increasing the value of the existing Child Support Grant, extending the Social Relief of Distress Grant, introducing a Basic Income Grant, introducing a new household-targeted 'Family Poverty Grant', and expanding public employment programmes. Within each of these 5 scenarios, we consider a number of variations on aspects of the programme design.

We find that, theoretically, a new Family Poverty Grant would be the most efficient means of reducing extreme (Food Poverty Line) poverty, with 56 per cent of spending going towards reducing the Food Poverty Line poverty gap, and the remaining 44 per cent of expenditure either going to those who are not extremely poor according to the Food Poverty Line or raising households' income over and above the poverty line (Spillover). Only the Basic Income Grant has a larger impact on poverty reduction at the Food Poverty Line. If perfectly implemented, it would reduce the Food Poverty Line poverty gap by over 70 per cent at a cost of R60 billion per year. However, the Family Poverty Grant has strenuous implementation requirements, which at best makes it feasible only over a longer time horizon: it likely relies on the successful design of incentives to accurately self-report income, and requires the creation of an up-to-date national household registry amidst high rates of household re-formation. Not only is implementation more difficult, but the efficiency score of the Family Poverty Grant, given its strict means-test, is also more likely to be affected by inclusion errors than other grants, making it more susceptible to re-ranking once errors of inclusion and exclusion are taken into account.

The Basic Income Grant, in contrast, is the least efficient, most expensive, option for reducing poverty but is likely relatively easier to implement and has the largest total impact on poverty reduction. Even with a clawback mechanism, reclaiming the value of the grant from all registered taxpayers earning more than R3 808 per month via a reduction in the income tax rebate, only 19 per cent of spending results in poverty reduction at the FPL. It nonetheless reduces poverty by more than 85 per cent of the Food Poverty Line poverty gap, at a high fiscal cost (between R194 and R256 billion per year).

Extending the Special COVID-19 Social Relief of Distress Grant presents a middle-ground between the highly efficient but potentially unfeasible Family Poverty Grant, and the inefficient

[1] In the rest of the paper, 'ZAR' will be shortened to 'R'.

but likely more feasible Basic Income Grant. When means tested at the FPL, 35 per cent of this grant goes towards reducing FPL poverty, reducing FPL poverty gap by 55 per cent at a cost of R71 billion per annum. In its current form, the Special COVID-19 Social Relief of Distress Grant has incorrectly excluded a high proportion of eligible claimants due to out-of-date information on employment status, with high consequences for welfare. The administration currently relies on data with lags of up to 18 months to verify claimants' employment status. In South Africa's context of high labour market churn, this implies exclusion errors of around 15 per cent (or in the worst-case, 1-in-3) of eligible claimants (or at worst 30 per cent).[2] We discuss some approaches to mitigating these exclusion errors in the case of the Special COVID-19 Social Relief of Distress Grant.

Increasing the value of the Child Support Grant is slightly less efficient than extending the Special COVID-19 Social Relief of Distress Grant when it comes to FPL poverty reduction, but is the easiest option to implement out of those we consider. This is a much smaller intervention, costing R28 billion but only reducing the FPL gap by 17 per cent, with 30 per cent of spending going towards reducing FPL poverty. While in some respects it is beneficial that the Child Support Grant top-up mainly targets children and women, this disadvantages it as a mechanism for bringing currently excluded individuals into the social security system. In addition, it may be undesirable to target general poverty reduction by increasing a grant which is designed to be for children specifically.

A public works type programme[3] takes a different approach to social protection, and our analysis does not measure all the different ways in which the programme might add value, but merely how it would fare as a direct food poverty relief mechanism. For example, we are unable to quantify the benefits that the work provides to local communities in terms of improved services, the opportunity cost of participants' time spent in jobs which preclude other unpaid work (such as care work), and the potential learnings or pathways to future jobs for programme participants. It is nonetheless interesting to understand how the distribution of wages provided to programme participants compares to other social grant payments, and how the programme fares in terms of short-term extreme poverty alleviation. We find that a public works expansion programme is similarly inefficient to the Basic Income Grant as a means to target extreme poverty, with 19 per cent of spending reducing the FPL poverty gap. However, public works programmes may face more substantial operational challenges to scaling up than the other options. The largest version we model employs 2 million participants, costs R35 billion per annum, and reduces the FPL poverty gap by 15 per cent.

We discuss several caveats associated with our findings. Three in particular are worth highlighting here. Firstly, we implement simple static modelling which does not attempt to incorporate behavioural, macroeconomic or general equilibrium responses to the additional expenditures. This approach has costs and benefits, but overall, the results should be understood as approximate and undertaking additional research will be important – in particular to better understand the possibilities and administrative costs of implementation of the different scenarios. Secondly, our mandate to examine reductions in *extreme* poverty (per the Food Poverty Line) can distort programme effectiveness relative to reductions in *overall* poverty. However, when one does use reduction in overall poverty (per the Upper Bound Poverty Line) as the key metric the results are generally similar. The Family Poverty Grant remains the most efficient measure, but both the Basic

[2] See Sections 3.2.3 and Appendix B for more information on the basis for these estimates.

[3] Public works in this report refers to an illustrative example of a public employment programme. In modelling this we draw from aspects of existing programmes, but it represents no existing programme.

Income Grant and the Child Support Grant rank better in terms of efficiency at Upper Bound Poverty Line poverty reduction than they do at Food Poverty Line poverty reduction. The Special COVID-19 Social Relief of Distress, and the public works programme go down in the ranking. The Basic Income Grant and public works programmes, in particular, becomes less inefficient, but the Basic Income Grant by more so than the public works programmes. Thirdly, while we view the selected scenarios and associated grant values as particularly relevant for public discussion, there are of course countless variations of interest. Efficiency scores, however, are sensitive to grant size,[4] and so while small changes to grant values will not affect efficiency conclusions reported here, large variations will.

[4] Note that the Family Poverty Grant efficiency is also sensitive to the definition of a household. In this analysis we use the Living Conditions Survey definition of a household, namely: all persons who live together and provide themselves jointly with food or other essentials for living or a single person living alone. This does not include persons in special dwellings, like boarding schools, retirement homes, hospitals, prisons, teachers' quarters, or nurses' homes.

1 Introduction

South Africa's already weak economic situation has been exacerbated by the COVID-19 pandemic, exacerbating unemployment and exposing the precarity and poverty many households face and the lack of social protection for the working age.[5] Until recently, support in terms of the Social Assistance Act has been limited to children, older persons and persons with disabilities while social relief of distress has been restricted to disaster incidents.[6]

The persistently high levels of poverty, unemployment, and inequality place strong pressure on the state's post-COVID-19 response. The latest official poverty numbers are from 2014/15 and showed that 55.5 per cent of South Africans were living in poverty and almost half of those were below the extreme poverty line[7] (25.2 per cent). Inequality and unemployment are amongst the highest in the world, at 0.63 on the Gini Index (2014/15), and a total unemployed population of 7.8 million people (11.1 million people including discouraged work-seekers). The latest employment statistics show 1.4 million fewer employed workers than pre-COVID-19. To mitigate the impacts of COVID-19 on poverty and unemployment, a number of measures, including the Special COVID-19 Social Relief of Distress Grant, were put in place to support vulnerable citizens.

In light of this, a team from the National Treasury and from SALDRU was formed to model the poverty reduction impact of a selection of medium- to long-term social assistance options that could replace the existing Special COVID-19 Social Relief of Distress mechanism upon it's expiry, and this paper documents the findings of the work: the authors simulate and compare a number of expenditure scenarios requested by National Treasury (and commented on and modified by the SALDRU team), that focus on the goal of eliminating income poverty at the Food Poverty Line of R624 per person per month (per the National Development Plan). The baseline framework on which we model the additional grants includes the main Department of Social Development led social assistance packages (The Old Age Pension, Child Support Grant, Care Dependency Grant, Foster Care Grant and Disability Grant), and also attempts to value in-kind services such as the housing subsidies, and Free Basic Services. There are some omissions, however, such as school feeding which is not included here.

We write this paper in the context of a constrained fiscus in the post-COVID-19 era. In response to the current high debt-to-GDP ratio, fiscal deficit, and economic contraction, the National Treasury has sought to reduce debt, through sharp expenditure reductions of R264.9 billion, or 4.6 per cent of GDP, over the 2021 MTEF period. The key metrics chosen for evaluating each grant are therefore affordability, total reduction in the FPL poverty gap, and value-for-money ('efficiency'[8]).

Note that while this paper specifically focuses on the narrow goal of eliminating poverty at the FPL, we recognise that targeting this most vulnerable subset of a much larger pool of the poor

[5] Social assistance is available for the newly unemployed, but unavailable for those that have never worked or who have been unemployed for a long time.

[6] Social welfare services also play a role in poverty mitigation while other social protection measures provided through the agriculture, small business support, and other sectors are limited in scale.

[7] Measured against the Upper-bound poverty line of R992 per month, and the Food poverty line of R441 month. Note that the Living Conditions Survey of 2014/15 is last official poverty survey for South Africa.

[8] We will explain the concept of efficiency in greater details in Section 2.2. on methodology, but we define it here as the share of spending that reduces the poverty gap.

and vulnerable could result in undesirable outcomes for those above the Food Poverty Line but who are still poor. We try to highlight throughout the paper where these are likely to occur.

We similarly note that poverty reduction—which we focus on here—is just one of the goals of social protection. Other goals could include widening the social security safety net to ensure that all households are protected from economic shocks and other crises, reducing inequality of opportunity (through, for example, targeting aid to groups such as women who face additional disadvantages including higher unpaid care burdens), creating or ensuring equality and justice by minimising the numbers of incorrectly excluded grant applicants.

Finally, although this paper focuses on the microeconomic impacts, further research on the wider macroeconomic impacts of both the grant expenditures, and the financing mechanisms used to fund them is important to better understand the implications of each of the grant options. This is perhaps particularly important in the case of the BIG, given the size of the expenditures.

The following expenditure scenarios are simulated:

- Increase the value of the Child Support Grant to equal the Food Poverty Line.
- Reinstate the Special COVID-19 Social Relief of Distress grant and extend to Child Support Grant caregivers, with variations on qualifying age and income threshold.
- Provide a Basic Income Grant to working-age adults, with differing degrees of universality.
- Provide a new 'Family Poverty Grant' targeted at households with income below the Food Poverty Line, inspired by certain aspects of Brazil's Bolsa Familia.
- A public works programme inspired by aspects of the Expanded Public Works Programme and Community Work Programme.

In all cases, we use household survey data to model these scenarios, by assigning the additional income to programme recipients and then examining how poverty and distributional metrics change as a result of the additional income. Our static modelling does not attempt to incorporate behavioural or general equilibrium responses to the additional expenditure. This is an important limitation of our analysis but comes with the advantage of avoiding reliance on strong assumptions. However, readers should keep in mind the limitations of our method and recognise that specific percentage-point results are approximate. Further detail is provided on each scenario in Section 3.

The social policy options are considered here with a medium-to-long-term framework in mind. Consequently, there is scope to move beyond current implementation constraints at the Department for Social Development or the South African Social Security Agency (SASSA), and to consider grants which will require new implementation infrastructure. Nonetheless, we cannot ignore that some of these scenarios are much harder to implement than others. Where actually-implemented policies substantially differ from our modelling assumptions, results on the efficiency of those policies will similarly become less accurate.

Our key findings are that the introduction of a household-targeted Family Poverty Grant is theoretically able to most efficiently target the Food Poverty Line and Upper-bound Poverty Line poverty gap, however it would require a dramatic expansion of existing administrative capacity, as it relies on the successful design of incentives to accurately self-report income and develop a household registry, and it would face additional challenges against high rates of household re-formation. The Basic Income Grant is the least efficient, and the most expensive, but achieves a more than 80 per cent reduction in the Food Poverty Line Poverty gap.

The Special COVID-19 Social Relief of Distress grant budget is similar in size of grant disbursements to the Family Poverty Grant and presents as a middle-ground between the highly efficient but potentially unfeasible Family Poverty Grant, and the inefficient but likely more feasible Basic Income Grant. A key constraint to effective implementation of the Special COVID-19 Social Relief of Distress has been the availability of data to verify information of claimants, particularly in the South African context of substantial informal labour and high labour market churn.[9] It is expected that lags in data have incorrectly excluded high numbers of unemployed applicants in the existing implementation, as well as incorrectly included a fair number of employed applicants in the higher income deciles.[10] While there are too many unknowns for us to consider these issues systematically across scenarios, we discuss some of the expected barriers to implementation in Section 4.3.

While the Child Support Grant is more efficient than public works programmes, both are small in size, and neither is exclusively designed to target short-term poverty. A public works programme would presumably have at least equally important labour-market goals, which are likely to impair its impact on direct poverty alleviation and coverage of the extreme poor,[11] and the Child Support Grant is designed to target child poverty, thereby making it difficult to ensure coverage of other vulnerable individuals and relying on household members to distribute the grant fairly (as does the Family Poverty Grant).

2 Data and methodology

2.1 Data

Our primary dataset is the Living Conditions Survey of 2014/5 (LCS 2015), which is a household level survey with indicators of household income, housing, and education and health. In order to appropriately cost and measure the impact of the grant scenarios, we update the 2014/5 Living Conditions Survey (LCS) to more closely resemble the mid-pandemic environment as it was at the end of 2020. We do this in three steps. Firstly, we forecast income to pre-pandemic levels using a per capita growth in GDP from 2014 to 2019 of 17 per cent. Secondly, we reweight the dataset to a) match 2020 demographics, disaggregated by race, age, gender and province,[12] and b) match the administrative records on the taxable income distribution. Finally, we use the Quarterly Labour Force Survey (QLFS) to calculate the change in employment from 2015 Q1 to 2020 Q1, and from 2020 Q1 to 2021 Q1, and implement these changes in the LCS dataset by randomly shocking certain individuals from employment to unemployment, based on a set of demographic and employment characteristics.

We can then recalculate the components of taxable income, market income, direct taxes and contributions, and direct transfers so as to generate a new Disposable income aggregate for 2021. Checking the decay in the Disposable income aggregate, we end up with a percentage change

[9] It is expected that lags in data have incorrectly excluded high numbers of applicants in the existing implementation of the Social Relief of Distress grant.

[10] We explain this in detail in Sections 3.2.3 and Appendix B. Note that deciles are calculated based on household per capita income, and so a child in a rich family, for example, will be in a higher decile despite earning zero individual income.

[11] An example of how this might work in practice, is that an EPWP programme with certain training targets is incentivised to target individuals who are more easily trainable.

[12] We do this using Wittenberg's maxentropy package in Stata.

decrease in Disposable income of 2.3 per cent, slightly higher than the percentage change decrease in the GNI of 1.1 per cent and in the GDP of 2.0 per cent.

Table 1: Per capita growth in GNI, GDP, and Disposable income constructed within the LCS

Statistic / aggregate	2014/15 (R)	2019/20 (R)	Percentage change	2020/21 (R)	Percentage change
GNI	71 870	83 926	16.8	83 007	-1.1
GDP	73 690	86 375	17.2	84 606	-2.0
Disposable income (LCS)	41 175	47 763	16.0	46 675	-2.3

Source: authors' calculations based on LCS 2014/15, QLFS 2015 Q1, QLFS 2020 Q1, and QLFS 2021 Q1

Mean Disposable income decreases for all deciles from 2019/20 to 2020/21 except Decile 10 which sees a slight increase (Table 2).

Table 2: LCS Disposable income 2015, 2020, and 2021

Decile of per capita Disposable income	Disposable income (per capita, per month)			Percentage change		
	A. 2014/15	B. 2019/20	C. 2020/21	A to B	B to C	A to C
1	170	209	199	22.7	-4.6	17.0
2	329	453	440	37.8	-2.8	33.9
3	474	652	621	37.4	-4.8	30.9
4	661	881	832	33.4	-5.7	25.9
5	915	1 173	1 093	28.1	-6.8	19.4
6	1 284	1 567	1 445	22.1	-7.8	12.6
7	1 899	2 253	2 049	18.6	-9.0	7.9
8	3 080	3 628	3 390	17.8	-6.6	10.1
9	5 970	7 062	6 752	18.3	-4.4	13.1
10	19 757	21 368	21 512	8.2	0.7	8.9
Total	3 454	3 925	3 835	13.6	-2.3	11.0

Note: the mean values per decile allow reranking such that columns are ranked by 2015, 2020 and 2021 Disposable income respectively.

Source: authors' calculations based on LCS 2014/15, QLFS 2015 Q1, QLFS 2020 Q1, and QLFS 2021 Q1.

Further details on this process, and a set of robustness checks, are included in Appendix A.

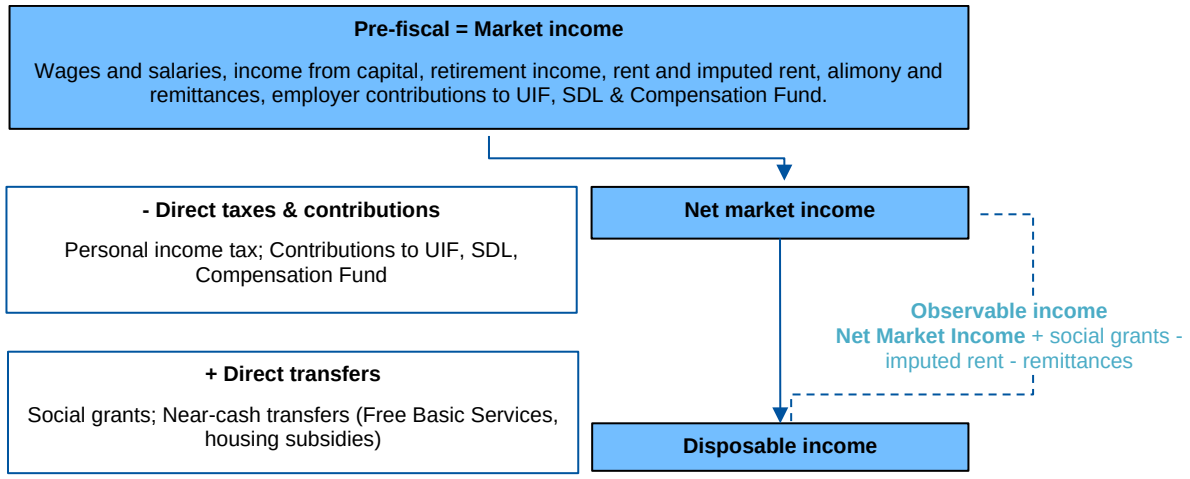
2.2 Methodology

We construct the scenarios on top of an existing model built for a 2014/15 Fiscal Incidence analysis (CEQ Assessment). We improve on the previous version of the model for 2014/15[13] and simultaneously update the instruments to match the 2020/21 policy environment and budget numbers.[14]

A flow chart which describes the construction of the CEQ Income Concepts from Market to Disposable income is included below. We start at Market income, constructed using income from private sources such as earnings from labour and capital, and grossed up by employer contributions to the Unemployment Insurance Fund (UIF), Skills Development Levy (SDL), and the Compensation Fund. We then subtract the direct taxes and contributions to generate Net market income, and add in the value of social grants received, as well as near-cash transfers such as Free Basic Services and housing subsidies to reach Disposable income. Disposable income is the primary income concept we use for measuring poverty.

Note that for all of the expenditure scenarios which require means-testing, we construct an aggregate which we call Observable income. The aggregate is designed to match what the administrative authorities are able to measure. It overlaps with Disposable income, but excludes the value of near-cash transfers, imputed rent[15] and remittances. It is only used to determine means-tested programme eligibility; poverty is still measured using Disposable income.

Figure 1: Income concepts



Source: adapted from Lustig (2018).

[13] Deductions for retirement contributions and medical tax credits are more precisely modelled, and Market Income is grossed upward to include employer contributions to the Compensation Fund, Unemployment Insurance Fund, and Skills Development Levy.

[14] Goldman, Woolard & Jellema (2020) describes the allocation methodology for each of the fiscal instruments, and the income concepts, included here however the concepts shown in the original CEQ Assessment are based on the official Statistics South Africa (Stats SA) consumption welfare aggregate, while here we based our Income Concepts on an income aggregate.

[15] Imputed rent is the rent a house owner would be willing to pay to live in their own house. It is included in the income aggregates because 1) the additional money saved can be used to purchase other goods, thereby increasing the purchasing power of a household to consume a basic basket of necessities and 2) the value of imputed rent can be understood as a type of in-kind income the owner receives as a monthly return on their housing asset.

In each subsection we present descriptive statistics regarding simulated programme costs, coverage, and benefits. For coverage we show both the number of direct programme beneficiaries and the sum of direct beneficiaries and 'indirect' beneficiaries (those co-resident with direct beneficiaries). When showing benefits, we show the average per month value of the grant received by direct beneficiaries.

In the analysis of each scenario, we refer to and draw from a set of indicators, which we define here for ease of reference:

- Incidence: the ratio of total expenditures per decile to total income per decile.
- Concentration shares: the shares of total expenditure distributed to each decile.
- The impact of the grant on poverty reduction—using the poverty headcount ratio (per cent of the population below the poverty line) and the poverty gap (the average shortfall below the poverty line across the population, normalised by the poverty line).
- A measure of FPL spending efficiency:[16] the proportion of scenario spending which contributes to a reduction in the FPL poverty gap.

We also use the term 'Spillover', in the paper, by which we refer to the spending which raises previously poor households' incomes over and above the poverty line.

In all cases, the baseline is Disposable income excluding the new scenario spending, and we compare this with Disposable income including the new scenario spending. This allows the reader to observe the *Marginal impact on poverty reduction*[17] of the simulated scenario. This also means that the analysis we present here is explicitly about *additions* to the existing social grant system. We do not analyse the impact of existing social grants, which we know already have a substantial impact on poverty reduction.[18]

2.3 Deciles, and national poverty lines

In order to better understand the information reported in the tables and graphs throughout this paper, it is helpful to keep certain statistics in mind. The minimum, mean and maximum of monthly per capita income at baseline Disposable income are shown for each decile in Table 3 below, and the values of the national poverty lines in Table 4. Note that all deciles are calculated based on baseline Disposable per capita income.

[16] Note that when we use efficiency here and throughout this paper, we are referring to what the CEQ Handbook calls the Fiscal Gains to the Poor Effectiveness Indicator.

[17] See Lustig (2018) for a discussion of Marginal impacts.

[18] See, for example, Goldman, Woolard and Jellema (2020).

Table 3: Minimum, mean, maximum per capita Disposable income per decile of Disposable income

Per capita monthly Disposable income			
Decile	**Min**	**Mean**	**Max**
1	-	199	347
2	347	440	530
3	530	621	719
4	719	832	955
5	955	1 093	1 248
6	1 248	1 445	1 684
7	1 684	2 049	2 546
8	2 546	3 390	4 486
9	4 486	6 743	10 170
10	10 170	21 488	164 066

Source: authors' calculations based on LCS 2014/15, QLFS 2015 Q1, QLFS 2020 Q1, and QLFS 2021 Q1.

Putting this information together, we see that the FPL of R624 per month falls within the third decile of baseline Disposable income, the LBPL falls within the fourth decile, and the UBPL falls within the sixth decile.

Table 4: 2021 Poverty line values

Poverty line	**2021 line values**
Food Poverty Line (FPL)	624
Lower-bound Poverty Line (LBPL)	890
Upper-bound Poverty Line (UBPL)	1 335

Source: reproduced from Statistics South Africa (2021: 3, Table 1). 2021 line values are per person, per month.

3 Scenarios

This section first conducts a within-scenario comparison of each of the scenario variations, and then chooses one variation to compare against the other scenarios in Section 4.

3.1 Child support grant

3.1.1 Description and methodology

In this scenario, the statutory Child Support Grant (CSG) amount is increased by 36 per cent from R460 to the current FPL of R624 per month.[19] We simulate two variations on this scenario: The first where the existing income threshold does not increase along with the grant value (i.e. R4600

[19] The CSG values are based on reported values in the survey, which will make the results look less effective compared to the other, simulated scenarios. We observe amounts received per caregiver not per child, and there are multiple adults with children in each household, which complicates linking CSG recipient children within the household to caregivers. We therefore back out the number of children by taking the value of the grant received and dividing by the statutory amount for each household. We calculate the new CSG grant amount in the first variation, by multiplying by 1.36 (the ratio of R624 and R460), and we calculate the CSG grant amount in the second variation by applying the statutory amount multiplied by 2.0 - the average number of CSG recipient children per caregiver according to the survey.

per month[20] despite that the legislation provides for the CSG threshold to be the size of the grant value multiplied by ten); and a second sub-scenario where the income threshold increases in line with the new grant amount, to R6 240 per month. Of the existing social grants, the CSG reaches by far the largest number of direct and indirect beneficiaries, and previous research has shown it to be highly progressively targeted (Bassier et. al, 2021). Raising the value to the FPL is anticipated to disproportionately benefit women and children, given the demographic structure of CSG-receiving households.

3.1.2 Results

The average monthly value of the grant for beneficiaries increases from R458 to R623, with the increased size of the grant, and the number of direct beneficiaries increases from 6.99 to 7.30 million when we raise the threshold to the R6240 required by the current regulations. The total cost increases by R27.7 billion when we increase the size of the grant, and a further R4.7 billion when we increase the threshold (Table 5).

Table 5: CSG value, beneficiaries, and total annual cost

Simulation	**Average monthly value of grant (R)**	**Total benef. (millions)**		**Cost (R, billion)**
	Individual	**Direct**	**Direct + Indirect**	**Annual**
Baseline (R460, R4600)	458	6.99	30.0	76.8
Increased grant size (R624, R4600)	623	6.99	30.0	27.7
Increased size and threshold (R624, R6240)	624	7.30	30.8	32.4

Note: [1] given that we view caregivers and not children receiving the CSG, average monthly value of grant for individuals is here the value of amount received by caregiver, divided by average of 2.0 children per caregiver. [2] Grant values do not perfectly match statutory values because the baseline grant and first variation are based on directly observed benefits in the dataset, rather than simulated values.

Source: authors' calculations based on LCS 2014/15, QLFS 2015 Q1, QLFS 2020 Q1, and QLFS 2021 Q1.

Figure 2a and 2b show graphs of incidence and concentration for the CSG. A graph of incidence shows the amount of grant expenditure provided to a particular decile, as a share of income earned by the decile. It shows the benefit to an individual in that decile of a transfer and is a measure of both size and distribution. Ultimately the incidence will explain graphically why we are seeing a particular poverty impact. We also show the concentration shares, which show the percentage of the transfer going to each decile, abstracted from programme size. The concentration shares are also an absolute measure of distribution - they are not relative to income. Comparing concentration shares with income is particularly useful for understanding the efficiency results and can (roughly) tell us whether a transfer is progressive relative to income.

In Figure 2b the baseline and variation with increased grant size are perfectly overlaid, meaning that increasing the size of the CSG does not change its distribution across the deciles. This is because increasing the size of the CSG results in the same factor increase in the grant amount for all recipients. Once we add the additional spending above the current threshold, however, richer households become eligible for the grant. The grant becomes less pro-poor, as a result, with

[20] The threshold is actually implemented as R4600 for a single individual, and R9200 for a married couple. For simplification we only report the single threshold here, but we retain the ratio of 2:1 size of the income threshold for married couples and single individuals.

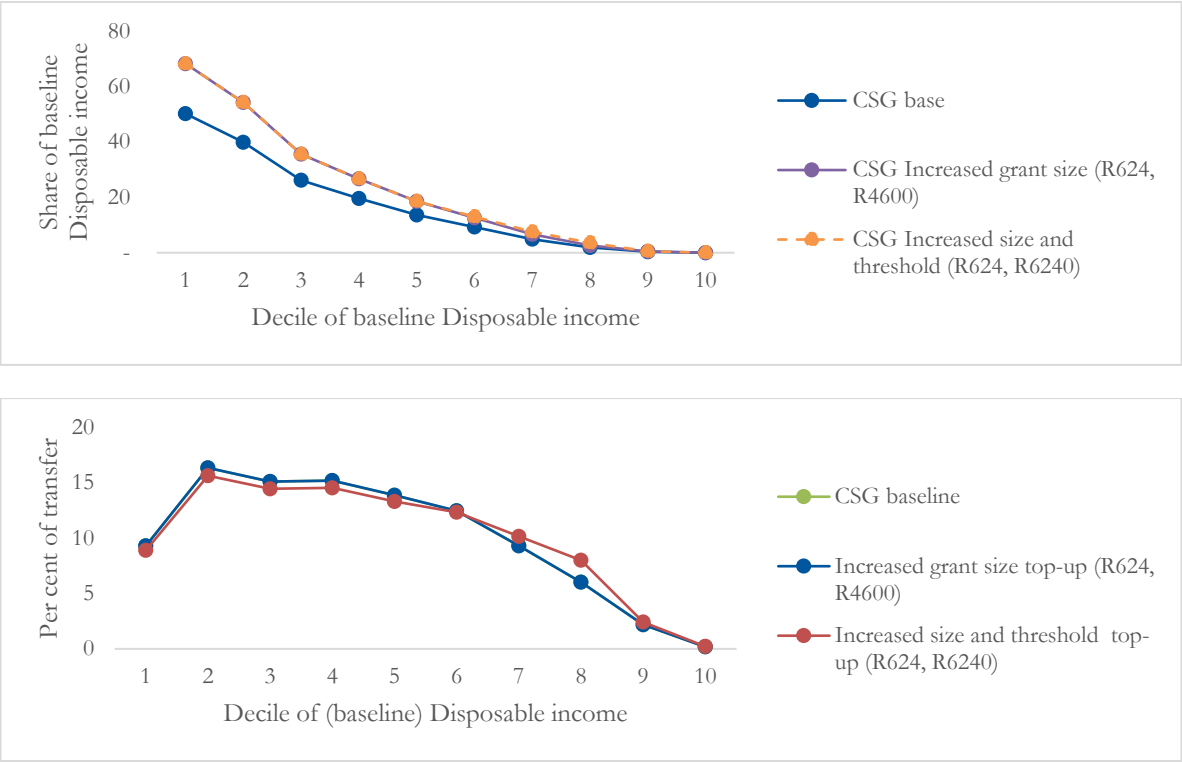
Deciles 1-6 receiving a slightly smaller share of total disbursements, and Deciles 7-10 receiving a larger share.

Note also that the share of new CSG expenditure appears lower in Decile 1 than in Deciles 2 and above at Disposable income (Figure 2b). This is purely because the Disposable income measure already includes the baseline CSG measure, and thus a substantial amount of reranking has occurred - by which we mean that a number of households which were in Decile 1 have moved to higher deciles because of the CSG receipt. Comparing the same graph ranked by Market income (prior to the inclusion of the baseline CSG), the results show roughly equal shares of CSG expenditure going to Deciles 1 and 2.

In all three variants, the incidence of the grant becomes smaller as we move towards the richer end of the distribution. This is partly because the grant values are smaller as a share of income for the upper deciles, and partly because there are fewer recipients.

Increasing the grant size increases the incidence across all deciles, as there is no change to numbers of recipients, only to grant size. Raising the threshold, however, has no impact on the incidence in deciles 1-3[21] (Figure 2a) as the additional expenditure all goes to individuals above the threshold.

Figure 2: Child support grant a. Incidence (top panel), b. Concentration shares (bottom panel)



Note: poverty is measured at Disposable income.

Source: authors' calculations based on the LCS 2014/15, QLFS 2020 Q1, and QLFS 2020 Q2.

Figure 3a shows the impact on poverty of a particular variation. The first bar shows the size of the *baseline* FPL Poverty Headcount and Poverty Gap, and the second and third bars show the *scenario* FPL poverty headcount and gap. By comparing the size of the FPL Poverty Gap at the baseline

[21] Only in deciles 6-8 are the increases greater than 0.4 percent of income per decile and range from 0.4 to 1 percent

(10.2 per cent) with the FPL Poverty Gap after the introduction of the Increased grant size (8.3 per cent), we can determine that the marginal contribution of the CSG Increased grant size top-up to FPL poverty gap reduction is 1.8 percentage points (10.16 – 8.33) - another way of saying that is the poverty gap is reduced by 1.8 percentage points.

Figure 3b shows the FPL efficiency scores[22] of the two top-ups, both combined with the original CSG and on their own. This measure calculates the expenditure that goes to topping the baseline Disposable income of the poor up to the FPL, but not including any Spillover, as a share of total grant expenditure. In 3b we show the efficiency measure of the CSG at the baseline for comparison with the top-ups, as well as, for completeness, the efficiency of the total grant once the top-ups are combined with the baseline.[23]

From the zero additional incidence in Deciles 1-3 for the second variation (Figure 2a), we can know that none of the grant expenditure is going to those households with income lower than the FPL. As a result, the efficiency of the second top-up in Figure 3b is zero. Expenditures used to top up the CSG size are less efficient than the baseline CSG at 29.5 per cent. This is because, once the baseline CSG is already included, the top-up is more likely to result in Spillover.[24]

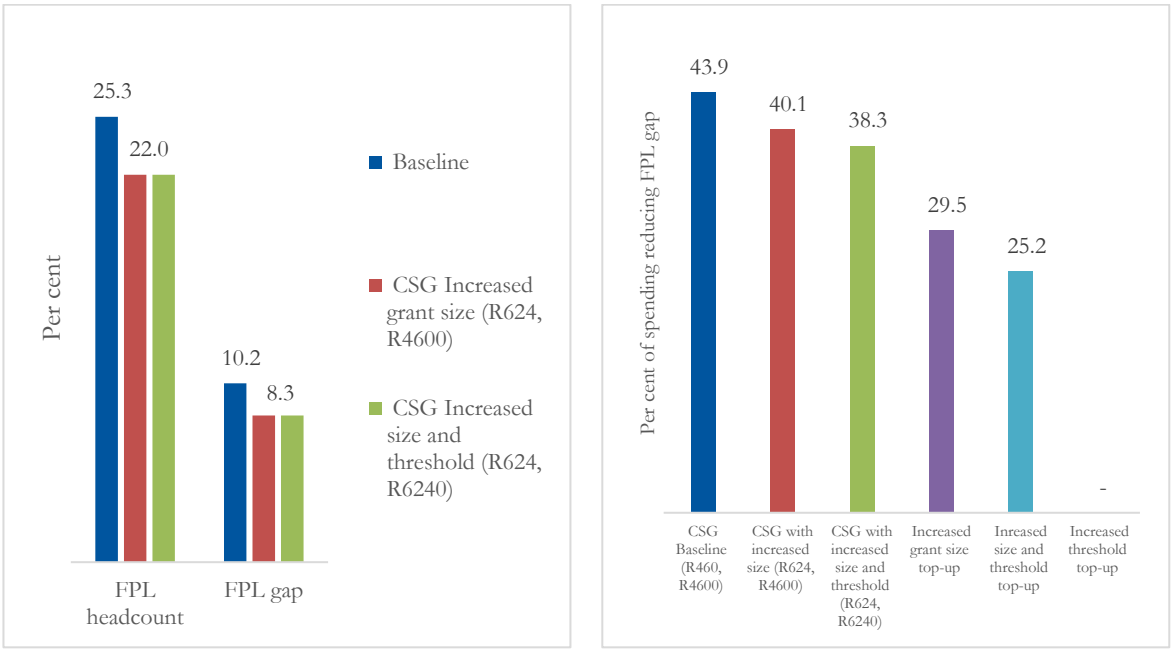
The increased grant size CSG top-up reduces the FPL poverty headcount and gap by 3.3 percentage points and 1.8 percentage points respectively, while increasing the eligibility threshold results in no additional poverty reduction impact at the FPL (Figure 3a) given that no expenditure reaches deciles 1-3.

[22] As described in Section 2.2., by efficiency we mean the percentage of grant expenditure that goes to reducing the FPL gap.

[23] Note that for the cumulative measures this is calculated as the share of CSG expenditure that goes to those individuals who are poor, as measured by their Disposable income **less baseline CSG expenditure**, and which tops them up to the FPL poverty line, as a share of total CSG expenditure. For the top-up measures, we measure income by Disposable income plus baseline CSG expenditure.

[24] In Decile 2, 160 thousand individuals that are poor in the baseline scenario, are now above the FPL once the grant size is increased. In Decile 3, there are 1.6 million additional individuals over the FPL.

Figure 3: a. Poverty reduction (LHS panel), and b. FPL Efficiency (RHS panel).



Note: [1] poverty is measured at Disposable income. [2] For the grant which include the baseline CSG grant only, efficiency is calculated as the share of CSG expenditure that i) goes to those individuals who are poor, as measured by their Disposable income without the CSG, and ii) which tops them up to (but not above) the FPL poverty line, as a share of total CSG expenditure.

Source: authors' calculations based on the LCS 2014/15, QLFS 2020 Q1, and QLFS 2020 Q2.

It is not surprising that increasing the size of the CSG (an already very effective grant) reduces its efficiency at the FPL, as it now brings individuals (further) above the poverty line. However, an increase in the size of the CSG pay-out, at a cost of R27.7 billion, is relatively easy to implement, and as noted above it disproportionately benefits women and children. On the other hand, it does not significantly expand the social safety net to include new individuals, and using a grant designed to target children for a more general expansion of the safety net would mean relying on indirect targeting of other population groups and would exclude poor households without children. In Section 4 we show how the topping-up of the CSG without changing its eligibility threshold performs against the alternative scenarios, given the negligible benefit at the FPL of raising the threshold.

3.2 Special COVID-19 Social Relief of Distress

3.2.1 Description and methodology

This scenario largely mimics the Special COVID-19 Social Relief of Distress (SRD) grant—a R350 grant currently provided by the Department of Social Development. While the design of the grant was ostensibly targeting those with no means of supporting themselves, in reality the grant was provided to those whose status was deemed to be not formally employed, with a means test applied only for those individuals who appealed their grant denial. It was first introduced in May 2020 and has since been extended three times, with the latest extension until March 2022. There have been various calls to make the grant permanent.

In the modelled scenario, the default grant is provided to all those aged 18-59 who are not formally employed[25], and not receiving any other social grant income (except the CSG, foster child grant or care dependency grant). Only the formally employed are excluded because the state does not have records of informal employment to exclude these workers, and they may face increased labour market precarity in any case. In some variants we impose additional individual income means tests, or vary the age restriction. The actual experience of SRD roll-out has been that many fewer people receive the SRD than are eligible in the survey data. This is discussed in Section 3.2.3. We include in our scenarios a 'scaled-down' version of the SRD, where we randomly select eligible individuals up to a population of 9.5 million SRD recipients, to match the maximum number currently budgeted for.[26] Note that the take-up rate directly impacts the cost of the grant; the full take-up and scaled down versions may be interpreted as bounds on the actual cost (the scaled down variation is 45 per cent of the cost of the full-take up variation). Similar considerations apply to the costing of the other variations.

The scenario includes the following specific variations:

i. SRD full take up: R350, as described (no scaling, no income threshold);
ii. Emergency: R350, recipients scaled down to reach 9.5 million (designed to replicate the current number of recipients);
iii. Job seekers: R350, no scaling, but restricted to age 18-34;
iv. FPL threshold: R350 with the additional eligibility criterion of individual observable income (if any) being less than R624 per month;
v. NMW threshold: R350 with the additional eligibility criterion of individual reported income (if any) being less than R3 570 per month (the national minimum wage);
vi. SRD 624 NMW threshold: as above with NMW threshold, but with increased value of grant to FPL (R624 per month).

Note that we do not apply an income threshold in scenarios i through iii, as we aim to replicate how the grant was actually administered, rather than how it was originally conceived.

3.2.2 Results

The number of recipients, and therefore the cost, changes substantially in the different variations of the SRD (Table 6). The full-take up scenario provides an SRD grant directly to 21.5 million individuals. Given that no means test is applied in this scenario, there are 1.6 million not formally employed working age individuals with income (not from formal employment) above the National Minimum Wage who are nonetheless eligible for the grant. At the NMW threshold, in which 19.9 million individuals are directly eligible for a grant. We model two variants of this—one at the R350 grant size, and one at the size of the FPL of R624. Implementing a lower threshold, at the FPL, reduces the number of recipients to 16.8 million, and restricting to adults age 18-34 (young 'job-seekers') further curbs the numbers to 13.4 million. In the 'emergency' variant we restrict the SRD recipients to 9.5 million (again with no income threshold), as noted above, and finally the SRD with the FPL threshold implemented together with the CSG top-up (not shown here) reaches 19.9

[25] One significant limitation of the LCS data is that it reports on the formal/informal sector of work, but not on whether the work itself is formal or informal (some people have informal employment in the formal sector). It also does not contain any variables which would allow us to infer work status (such as whether or not the respondent has a work contract, and benefits from paid annual leave). We use the formal sector as a proxy for informal employment in general.

[26] We call this the 'emergency' scenario, to reflect that limited receipt may be partly caused by the short timeframe of current SRD implementation, which affects both the number of applications and the accuracy of the approval process.

direct beneficiaries. In all variations, beneficiaries receive a value of R350 except for variation (iii) where we increase the size of the grant to R624.

Table 6: SRD value, beneficiaries, and total annual cost

Variation	Average value	Total beneficiaries (millions)		Annual cost
	Individual, per month	Direct	Indirect	(R, billion)
Full take-up	350	21.5	48.7	90.3
NMW threshold	350	19.9	46.7	83.7
R624 at NMW threshold	624	19.9	46.7	149.2
FPL threshold	350	16.8	43.0	70.6
Youth / job-seekers	350	13.4	38.8	56.2
Emergency	350	9.5	31.8	39.9

Source: authors' calculations based on LCS 2014/15, QLFS 2015 Q1, QLFS 2020 Q1, and QLFS 2021 Q1.

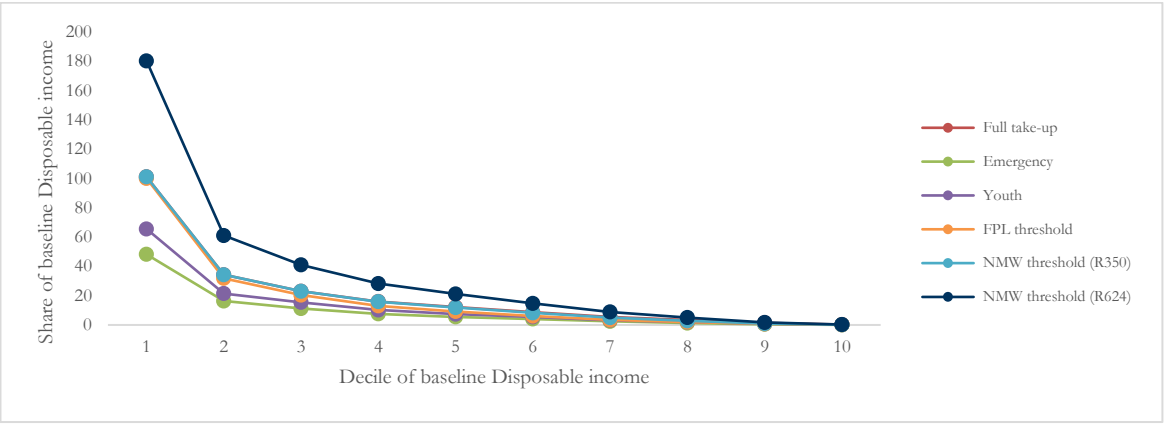
All variations on the SRD have a similar distribution, in terms of the share of the total which goes to each decile (Figure 4b), however the incidence per decile varies with the number of beneficiaries within the decile receiving the grant (Figure 4a). For example, the NMW threshold variation results in a benefit for the first decile to the size of 180 per cent of total baseline Disposable income in Decile 1, while the Emergency variation provides a benefit of only 48 per cent.

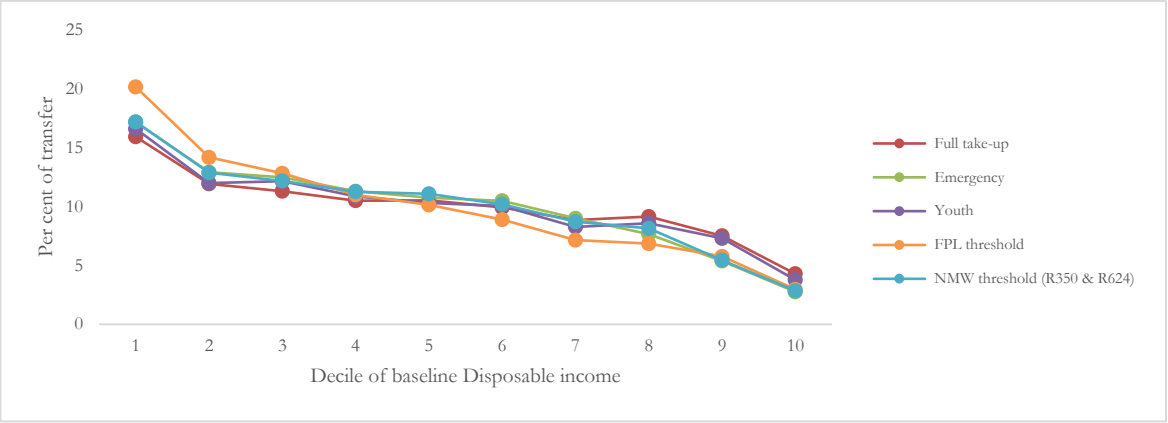
The variation which provides R624 per beneficiary, with a threshold at the NMW provides the largest disbursement for Deciles 1-3 (Figure 4a). It therefore reduces FPL poverty significantly more than the other variations. The headcount index drops to 8.9 per cent (16.4 p.p. reduction) and the poverty gap to 1.8 per cent (8.3 p.p. reduction) (Figure 5a). It costs R149.2 billion - more than double the variation with an FPL threshold and a R350 grant value (Table 6). Unsurprisingly, this variation is the least efficient at FPL poverty reduction in terms of share of expenditure which reduces the FPL gap (Figure 5b). Given that the size of the grant is larger, it is more likely to create Spillover above the FPL than the other variations, and the eligibility threshold is higher than the FPL, resulting in larger income increases for households above the FPL (mainly Deciles 4-10[27]). The concentration shares are exactly the same as for the R350 variation with the NMW threshold (Figure 4b).

The SRD at full take-up costs slightly more (at R91.2 billion) than the variation with a NMW threshold and with a R350 grant, and yet has almost identical incidence in Deciles 1-3 (It is not visible in Figure 4a, covered by the NMW variation). It therefore has the same impact on the poverty headcount and poverty gap (reducing the headcount to 17.3 per cent, and the poverty gap to 4.5 per cent, Figure 4a). This is unsurprising because any individual excluded from the SRD by the NMW threshold already lives in a household with a per capita Disposable income greater than the FPL, and so the two policies reduce poverty to the same extent. However, the NMW threshold does ensure some high-income individuals are excluded from SRD eligibility, thus reducing the programme's budget and improving its efficiency (28.2 for full take-up SRD versus 30.4 for the NMW threshold variation, Figure 5b).

[27] There are increases in Deciles 8 to 10, although not visible on the graph.

Figure 4: Social Relief of Distress a. Incidence (top panel), b. Concentration shares (bottom panel)





Note: poverty is measured at Disposable income.

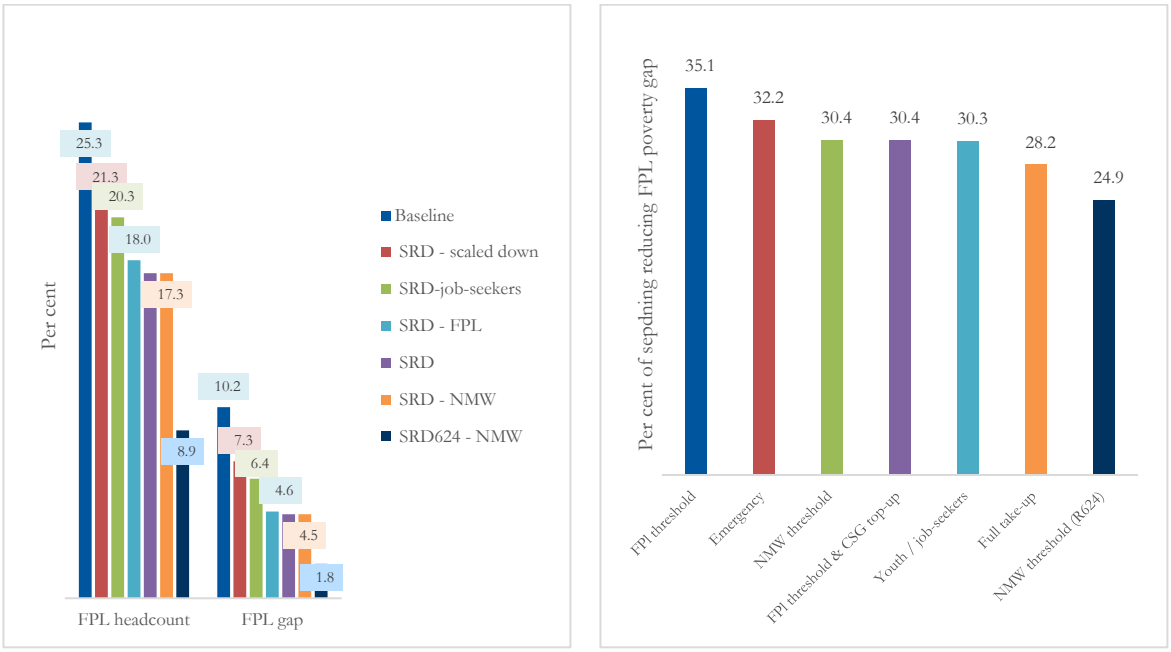
Source: authors' calculations based on the LCS 2014/15, QLFS 2020 Q1, and QLFS 2020 Q2.

The variation with a means-test cut-off at the FPL, has a (barely discernible) lower incidence in Deciles 1-3 than the full take-up and NMW variations, and therefore the remaining poverty is slightly higher at 18.0 per cent headcount, 4.6 per cent FPL gap. Nonetheless, it directs a larger share of total expenditures to the poorest decile (Figure 4b), and so scores higher on the FPL Efficiency measure (the higher the threshold is above the FPL, the more likely we are to include individuals with a per capita income above the FPL).

The 'emergency' SRD, scaled down to 9.5 million beneficiaries, and the youth SRD, limited to ages 18-35, have the lowest marginal impact on poverty reduction. This is because they have the smallest incidence in Deciles 1-7 of all the variations. Nonetheless, they are still fairly effective in terms of the proportion of expenditure targeting the FPL, with only the FPL threshold variation scoring higher in terms of efficiency, and the youth variation scoring similarly to the NMW threshold variation. This result can be understood by examining the average value of grant received per beneficiary household, not shown here. Households receiving at least one SRD grant in the full take-up variation, for example, receive on average R625 per household (as there is an average of 1.8 SRD recipients per household). In the emergency scenario, however, households receive on average R477 per household (an average of 1.4 recipients per household). The full take-up SRD will therefore result in more Spillover for households than the emergency SRD.

Given its relatively high efficiency, we choose the R350 SRD with the FPL threshold to compare with the alternative scenarios in Section 4.

Figure 5: SRD a. Poverty reduction and b. FPL Efficiency



Note: poverty is measured at Disposable income.

Source: authors' calculations based on the LCS 2014/15, QLFS 2020 Q1, and QLFS 2020 Q2.

3.2.3 Current SRD implementation

The substantial implementation difficulties associated with the current SRD roll-out are detailed in Appendix B, based on meetings with SASSA officials. They give rise to a concern that we may be overstating the efficiency of the SRD. The relevance of these difficulties for our analysis will depend on whether we believe that a future SRD grant implemented as a medium to long-term policy option is likely to be plagued by the same challenges as an SRD grant implemented in the midst of a pandemic, and whether these challenges are particular to an SRD type grant or are likely to be faced across all scenarios.

As a result of the lags in employment verification, combined with very high rates of labour market churn in South Africa (especially so during the pandemic), an extremely large proportion of eligible SRD applicants are likely to be incorrectly rejected from grant receipt. Our preferred estimate is that the proportion of false rejections is close to 15 per cent, and not larger than 30 per cent.

There are reasons that our approximate estimates of false rejections may be overestimated or underestimated, and further investigation into SASSA's verification systems would allow a more precise figure. We arrive at this estimate via two methods, one based on compounding observed short-term (3-month) rates in labour market churn from the Quarterly Labour Force Survey, and the second considering the actual rejection rates published by SASSA for the August 2021 SRD applications.

Using the first method, compounding observed short-term rates in labour market churn, we estimate that about one third of eligible applicants could be rejected because their employment status is tested against outdated administrative databases, with employment as long as 18 months before the time of application sometimes liable to cause rejection. There are a number of caveats association with this result. Firstly, extrapolating short-term (3 month) churning rates back 18 months is likely to *overestimate* labour market transitions and false rejections, as the long-term

unemployed have less dynamic labour market histories than the short-term unemployed. Secondly, however, the QLFS reporting period of one week's labour market history every 3 months will cause us to miss any labour market transitions between the 3-month interview periods, causing us to *underestimate* labour market dynamism and false rejections. Thirdly, our false rejection rates are based on pre-pandemic data, and there is evidence that labour market churn increased significantly during the pandemic (Espi-Sanchez, Leibbrandt and Ranchhod 2021). In particular, people who were formally employed in March 2020 but who lost their jobs during the pandemic will be counted as *employed* in the most recent IRP5 data and rejected on the basis of the SRD employment test—potentially a substantial extra source of false rejections.

As a second method, we consider the actual rejection rates published by SASSA for the August 2021 SRD applications. Using this method, if we assume *all* of the IRP5 rejections to be false, this suggests a 15 per cent false rejection rate (accounting for false inclusions). However, this does not take into account the likelihood that a significant proportion of the UIF rejections may be false (as discussed in the Appendix B). This is much lower than the 1-in-3 estimated purely based on historical data. If we assume all of the UIF *and* IRP5 rejections to be false, we arrive at an upper bound estimate of the false rejection rate of 30 per cent, close to our 1-in-3 estimate. This estimate is derived as follows: of about 13 million applications, SASSA records 64 per cent verified as eligible and 29 per cent rejected as ineligible. Of the 29 per cent of applicants that were denied a grant, the largest number was based on Unemployment Insurance Fund records (48 per cent), and the second largest based on IRP5 records (33 per cent).[28] We can calibrate this to the information provided by SASSA that the main lags in employment verification are in the IRP5 database.

Additional issues faced with the means-testing of the SRD suggests that a coordinated database or agreement with banks (potentially with administration costs) are key requirements for the income test. If well implemented, they might reduce the need for unreliable information on employment status with long lags, as this can be inferred from regular payments of salary by an employer. Inferring income from bank statements may still be impossible in the case of own-account workers receiving fee income from a variety of clients and at variable amounts, however, and differentiating between valid types of bank inflows, such as transfers on behalf of others (such as remittances received on behalf of children) would also lead to false rejection cases. Of course, any errors or lags in means testing will incur similar issues to the false exclusions and inclusions discussed above.

The results of a sensitivity test show that our chosen efficiency measure is not impacted by the false exclusions, even in the worst-case scenario of a one in three rate of false exclusions (see Appendix D for details), and under the assumption that poor claimants are excluded at the same rate as non-poor claimants[29] (Table 7). This is because the errors of exclusion reduce total expenditure, while the proportion of expenditure reducing the FPL poverty gap remains constant. False inclusions, on the other hand, do decrease the efficiency of the SRD, but the effect is not large (reducing efficiency from 28% to 26%).

We wish to emphasise the impact of the false exclusions on poverty: by these rough estimates they result in a 2.8 percentage points (1.7 million individuals) lower reduction in the FPL poverty headcount, and 1.8 percentage points lower reduction in the poverty gap, bringing the impact of

[28] Other applications were denied based on failed identity verifications (9 percent), and the remaining 11 percent was the result of failing the age restriction requirements, already receiving a SASSA or NSFAS grant or loan, being registered on the government payroll or receiving a government pension.

[29] False rejections likely apply to those who recently lost jobs. This could mean they are poorer (since they lost their main source of income) or richer (the networks that allowed them to get a job in the first place may support them while unemployed).

the SRD at full take-up closer to the impact of the Jobseeker SRD. Reading testimonials (see, for example, Nkuna, 2021) are useful for an understanding of the importance of such an error for the individual, as well as the degree to which it reduces trust in the public sector, by violating principles of administrative justice, and foments a sense of desperation.

Table 7: SRD, errors of exclusion

Scenario variation	FPL headc.	FPL gap	Total benef. (million)	Cost (R, billion)	Effect.
SRD full take-up	17.3	4.5	21.5	90.3	28.1
i. False rejections and inclusions	20.1	6.2	16.0	67.2	26.0
ii. False rejections only	20.2	6.3	14.0	58.6	29.4

Source: authors' calculations based on LCS 2014/15, QLFS 2015 Q1, QLFS 2020 Q1, QLFS 2021 Q1, and NIDS-CRAM.

3.3 Basic Income Grant

3.3.1 Description and methodology

Similar to the R350 SRD grant, there have been various calls to introduce a Basic Income Grant (BIG) in order to address widespread poverty. In the BIG interventions we model here, the default grant is provided at the level of the FPL (R624 per month) to all individuals age 18-59 years old.[30] Three variations are modelled:

i. A 'default' BIG, universal in nature but for working-age individuals only;
ii. a BIG with a means test such that eligible individuals must earn below the NMW of R3 731 per month, if they have individual income;
iii. a BIG which in addition to the working-age restriction includes a 100 per cent 'clawback' of the grant benefit from individuals above the tax threshold (Figure 6), described in detail below.

Proponents of the BIG often claim that the cost of the grant will be made feasible by clawing back the amount of the transfer from those citizens earning above a certain threshold. It is not straightforward to claim back 100 per cent of the grant amount using Personal Income Tax marginal tax rates or even a surcharge on the Personal Income Tax, however the tax rebate provides a potential mechanism through which this can be achieved.

The primary tax rebate is an amount (currently) of up to R15 714 per year, provided to all taxpayers in the form of a credit subtracted from their tax liability. It is designed to protect the tax threshold, so that workers are not taxed from the first rand earned. In practice it also functions somewhat like a BIG for formal workers, however it is regressive at the bottom end, in that those with a tax liability less than R15 714 per year, do not benefit from the full rebate.

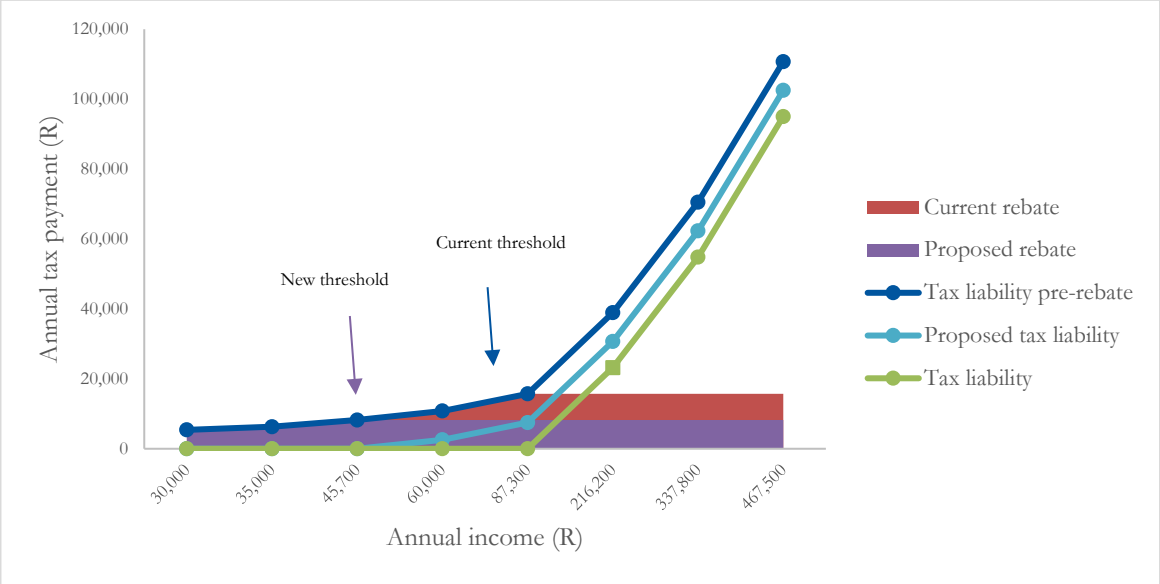
Here we suggest that the rebate could be reduced for those above a particular threshold by the same amount as the BIG, effectively establishing the desired 100 per cent clawback mechanism for the BIG. It could work as follows: the rebate is reduced by R624 per month (R7 488 per year), which reduces the existing rebate from R15 714 per year to R8 266 per year. However, given that the tax rebate is simply the size of the tax threshold multiplied by the lowest marginal tax rate

[30] Those below 18 and above 59 are eligible for the Child Support Grant and Old Age Grant respectively.

(currently at 18 per cent), this reduction of the rebate would imply a new tax threshold for paying non-zero tax of R45 700 (18 per cent of R45 700 is R8 266).

One cannot currently receive a tax credit from the rebate which is higher than one's tax liability, otherwise individuals would pay negative tax (i.e. a transfer). In this variation on the scenario, then, all those below the new tax threshold would continue to receive the same rebate as previously (at the size of their tax liability), however now they would additionally receive the BIG, resulting in a net benefit of R7 488. Everyone between the new threshold and the existing threshold would receive a smaller rebate than previously, with those with higher income receiving a smaller rebate. On net, they would not receive the full value of the BIG, but they would still be better off, as the size of the BIG would be greater than the reduction in the rebate. Finally, everyone *above* the current threshold would receive zero net gain, with a rebate reduction of R7 488, and a BIG of the same size. The rebate clawback mechanism is shown in Figure 6.

Figure 6: Rebate clawback



Source: authors' calculations based on the LCS 2014/15, QLFS 2020 Q1, and QLFS 2020 Q2.

Coincidentally, the new tax threshold of R45 700 per year, at R3 808 per month, is close to the NMW of R3 730 per month.

3.3.2 Results

In the working-age and NMW-targeted variations of the BIG, the value provided to each recipient is R624 per month. However, restricting recipients to individuals earning below the minimum wage reduces the number of beneficiaries from 34.2 to 25.0 million, with an ensuing reduction in total cost.[31] In the variation with clawback the average value of the grant among recipients is R597 per individual, because the net benefit of the intervention depends on an individual's tax liability, as discussed above. The number of recipients receiving a net benefit is 28.8 million (Table 8).

[31] Note that 25.0 million recipients is higher than the 19.9 million recipients in the NMW variant of the SRD. This is because those eligible for the SRD are not formally employed, and not receiving other grant income (such as the NSFAS, and foster care and care dependency grants), and below the NMW, whereas the BIG is provided to all individuals below the NMW.

Note that in the variation which only has a working-age restriction (with the greatest coverage), 34.2 million individuals receive the grant directly and 57.7 individuals benefit directly or indirectly through a household member—97 per cent of the population. Coverage is however very high for all of the variations, as would be expected.

The implication of this widespread coverage is an expensive program, between R194 and R256 billion per year. To put this into context, this entails more than doubling the existing expenditure of South Africa's already-expansive social grant system (R190 billion in the 2019/20 financial year, SASSA, 2020).

Table 8: BIG value, beneficiaries, and total annual cost

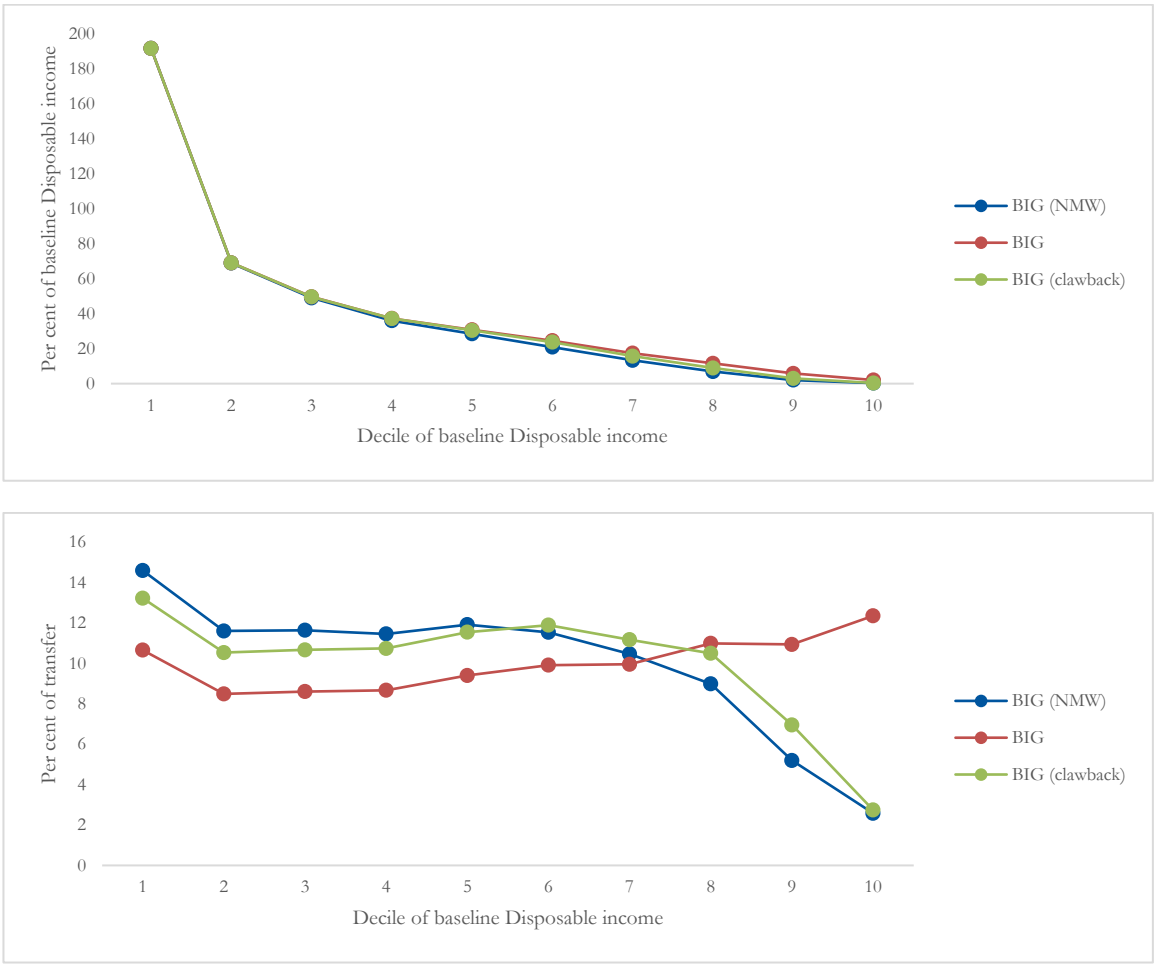
Scenario	Average value	Total beneficiaries (millions)		Annual cost
	Individual (per month)	Direct	Direct + Indirect	(R, billion)
BIG at NMW threshold	624	25.0	50.6	187.0
BIG (working-age individuals)	624	34.21	57.6	256.2
BIG with clawback	597	28.82	54.3	206.4

Source: authors' calculations based on LCS 2014/15, QLFS 2015 Q1, QLFS 2020 Q1, and QLFS 2021 Q1.

The impact on poverty (Figure 8a) is almost exactly the same for all three grants. This is because less than 0.1 per cent of individuals living in a household with per capita Disposable income below the FPL earn more than the NMW, and the new threshold is very close to the NMW. As a result, the FPL headcount is reduced from 25.3 to 7.6 per cent (7.7 per cent for the NMW variation) and the gap from 10.2 to 1.4 per cent (a marginal impact on poverty reduction at the FPL of 17.6-17.7 and 8.7 in the headcount and the gap respectively). These are, unsurprisingly, very large poverty reductions. Note that the marginal impact of the BIG at NMW threshold on poverty reduction is slightly higher than the marginal impact of the R624 SRD at NMW threshold. This is because the BIG is given to a larger pool of eligible individuals (25.0 vs 19.9 million). The marginal impact of the BIG on poverty reduction is 17.6 and 8.7 for the headcount and gap respectively, versus 16.4 and 8.3 for the SRD.

The unrestricted working-age and clawback variations respectively cost roughly R60 billion and R10 billion more than the version targeted to the NMW, however, with this additional expenditure going to those above the FPL poverty line (Figure 7a), thereby reducing the overall proportion of expenditure going to the lower deciles in these variations compared to the NMW targeted BIG (Figure 7b). The result is lower FPL poverty reduction efficiency (15.2 and 18.5 per cent respectively, compared to 20.1 for the version targeted to the minimum wage—see Figure 8b).

Figure 7: Basic Income Grant a. Incidence (top panel), b. Concentration shares (bottom panel)

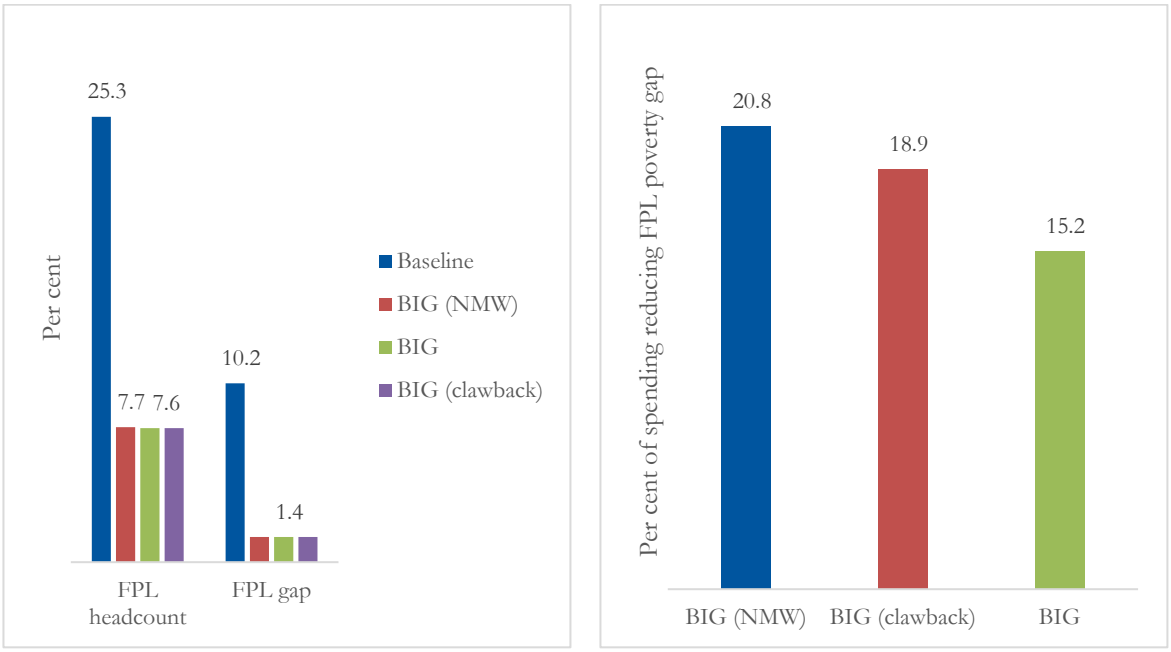


Note: poverty is measured at Disposable income.

Source: authors' calculations based on the LCS 2014/15, QLFS 2020 Q1, and QLFS 2020 Q2.

A BIG targeted to those below the National Minimum Wage may be the most efficient at reducing FPL poverty out of the BIG options considered, but applying a means-test obviates some of the main justifications for UBIG policies: ease of implementation and low (or no) risk of exclusion errors. Given Section 3.2.3's discussion on exclusion errors in implementation of the SRD grant, this is no mere philosophical point but one which, in practise, might have a large impact on the actual implementation of a BIG. Out of the targeted options, the BIG plus clawback option appears *a priori* simpler to implement. It suggests using the tax system for determination and payment of the social grant, thereby making use of an already-existing and enforced government system for income reporting and ineligibility criteria. This seems likely to reduce the risk of errors of exclusion by only excluding those registered and with sufficient taxable income (while those not registered as taxpayers default to receiving the full value of the grant).

Figure 8: BIG a. Poverty reduction (LHS) and b. FPL Efficiency (RHS)



Note: poverty is measured at Disposable income.

Source: authors' calculations based on the LCS 2014/15, QLFS 2020 Q1, and QLFS 2020 Q2.

We choose the BIG variant with clawback to compare with the alternative scenarios in Section 4, given that it performs well compared to the NMW threshold variant, there are likely ways of simplifying its administration through the use of the tax system, and it gradually phases out to zero further up the income distribution rather than having a discontinuous change in value.

3.4 Family Poverty Grant

3.4.1 Description and methodology

The Family Poverty Grant (FPG) scenario is inspired by the Brazilian Bolsa Familia's success at designing a system that precisely targets the national poverty line. The Bolsa Familia comprises a basic benefit provided to extremely poor households and four variable benefits depending on whether there is a child, a young person, a pregnant woman or nursing mother in the family or household. It also provides incentives for households to invest in education. The last component is a benefit to overcome extreme poverty if the family's per capita income, including all the benefits, is still below the poverty line.

The aspect of interest is the highly effective targeting of the Bolsa Familia grant to the extremely poor, and we abstract from the education and health conditionalities of the Brazilian programme and its six different components or sub-benefits. We therefore model the scenario without the complexity of the Brazilian version with its six different components or sub-benefits.

In this simulation, the benefit is targeted at families with per capita income below the FPL of R624 per month, where income includes existing social grants. Three variations are modelled:

i. Variable benefit: provide R460 to each working-age adult in each eligible household.
ii. Top up: Where household per capita income is below R624, top it up such that it equals R624.

iii. Two-step: Combination of (1) and (2) where we first provide the variable benefit to households, and then top any households that remain below the poverty line to the poverty line.

As mentioned above, the baseline income measured here (as with all the grant scenarios simulated in this analysis) incorporates grants and subsidies already in the system including the Old Age Pension, the Child Support Grant, the Disability Grant, Foster Care Grant, and Care Dependency Grants. Despite that these grants are provided to individuals in the household that may not choose to share it with other household members, this measure treats it as household income, as does the poverty measure.

This scenario is targeted to per capita household income, which is the same metric used for poverty measurement. As such we would expect to achieve extremely efficient poverty reduction with this policy. Indeed, this is what our simulation results suggest. However, if it can be implemented, for the reasons discussed below, this option is only realistically going to be fully rolled-out in the medium-to-long term, likely requiring a stop-gap grant to be implemented while the FPG administrative system is developed.

There are good reasons household-targeted grants are not ubiquitous and have never been implemented in South Africa before. A primary issue is that while it is easy for us to simulate such a grant in survey data, it would be exceedingly difficult to implement in practice. The grant requires not only reasonably accurate self-reporting of income to determine eligibility (which is implicit in all grant means tests), but requires this for all household members. Furthermore, the top-up measure requires precise measurement of household income to determine the grant *value*, which is different for each household depending on their extent of poverty. There are very strong incentives to under-report income in response to this grant. Accurate measurement of household means would be made more difficult by high rates of employment churn in South Africa, during normal times (Kerr, 2018) and in response to the pandemic (Espi-Sanchis et al, 2021).

We incorporate some small part of this difficulty in our implementation here, which uses reported income from household survey data, by using the 'Observed income' measure to determine grant eligibility and the value of the Top-up, while poverty is measured using Disposable income (see Section 2.2).

Perhaps an even greater challenge is that this grant requires the creation of a national household registry, which defines 'households' and lists their members. No such registry exists in South Africa, and the creation of such a registry would be exceedingly difficult and contentious in a country characterised by high rates of migrant labour, extended family support networks, and high rates of household dissolution and re-formation, and large populations living in informal settlements.

3.4.2 Results

In each variation of the FPG, the number of households receiving the grant is about 5.0 million—although there are about 100 thousand households (2 per cent of 5.0 million) which have no working age adults, and so they receive the variations with the top-up, but not the variable benefit for working-age adults.

Given the similar numbers of beneficiaries across the variations, the annual cost mainly varies with the size of the benefit. The two-step variation is the most expensive, covering both the cost of the variable benefit, and an additional top-up to the poverty line. 1.6 million households (7.3 million individuals) are topped up to, or above the FPL in step one, and an additional 2.0 million

households (7.8 million individuals) are topped up to the poverty line in step two. The remaining 1.3 million households are already above the FPL at Disposable income—while below it at Observable income.

Table 9: FPG value, beneficiaries, and total annual cost

Scenario	Average value	Total beneficiaries (million)		Annual cost
	Household (per month)	Household recipients	Individual recipients	(R, billion)
FPG - variable benefit	980	4.99	19.78	58.7
FPG - top-up to FPL	1 221	4.99	20.02	73.1
FPG - two step	1 413	4.99	20.02	84.6

Note: individual recipients is number of households multiplied by household size.

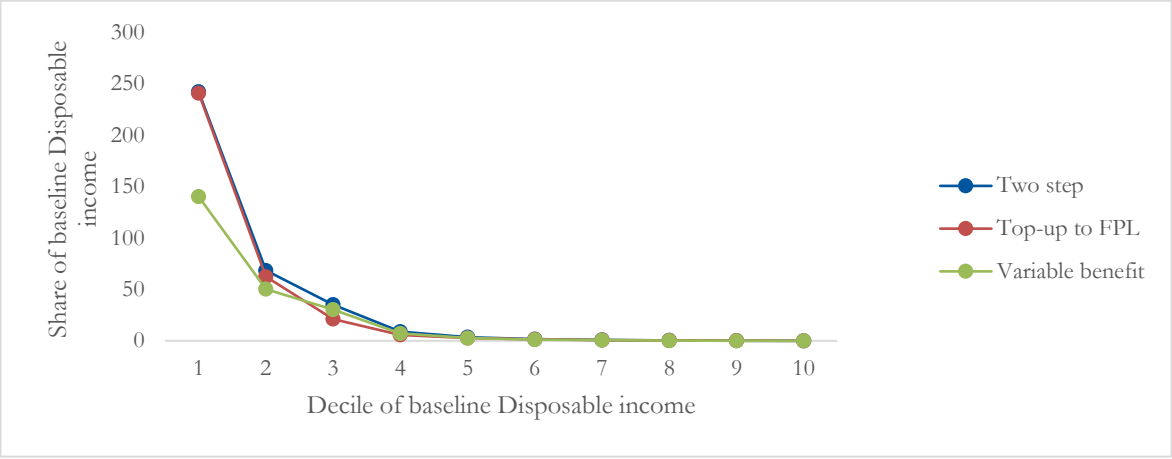
Source: authors' calculations based on LCS 2014/15, QLFS 2015 Q1, QLFS 2020 Q1, and QLFS 2021 Q1.

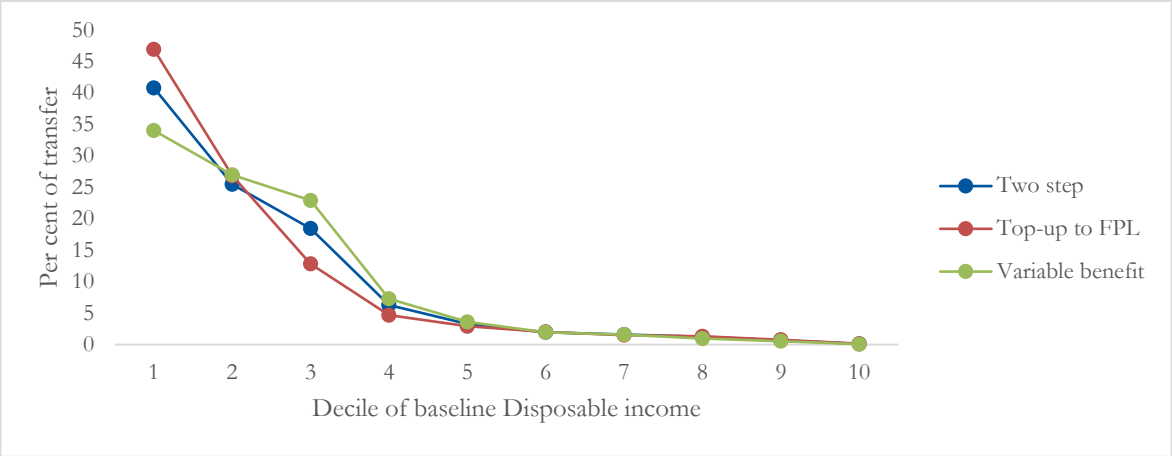
Here the difference between Observable and Disposable income becomes important, as shown in Figure 1. While we are targeting the Disposable income poverty gap, we assume that the administration is only able to observe the distance of a household's *Observable* income from the poverty line. We see the impact of this in Figures 9a and 9b. Were we able to perfectly target Disposable income, the incidence and concentration shares would be zero from Decile 4 to 10 for the top-up variation.

Due to its targeted design where the size of the benefit is greater for poorer households, the top-up variation results in the largest share of total grant disbursements going to Decile 1, while the variable benefit variation has a less steep decline between Deciles 1 and 3, as an eligible household receives an amount which varies only according to the number of working-age adults (though there are of course fewer eligible adults in decile 3 than Deciles 1 and 2, and there are also fewer eligible adults in Decile 1 than 2) (Figure 9b).[32]

[32] Decile 1 has 3.6 million working-age adults per decile, decile 2 has 2.9 million, and decile 3 has 1.5 million working-age adults below the FPL.

Figure 9: Family Poverty Grant a. Incidence (top panel), b. Concentration shares (bottom panel)





Note: poverty is measured at Disposable income.

Source: authors' calculations based on the LCS 2014/15, QLFS 2020 Q1, and QLFS 2020 Q2.

Poverty at the FPL is zero in the top-up and two-step variations, despite the imperfect measurement of Disposable income. This is because Disposable income is greater than Observable income for all observations.[33] The variable benefit version of the grant, however, reduces the FPL poverty headcount from 25.3 to 13.1 and the gap from 10.2 to 2.8 (a marginal impact on poverty reduction of 12.2 and 7.3 percentage points respectively, Figure 10a).

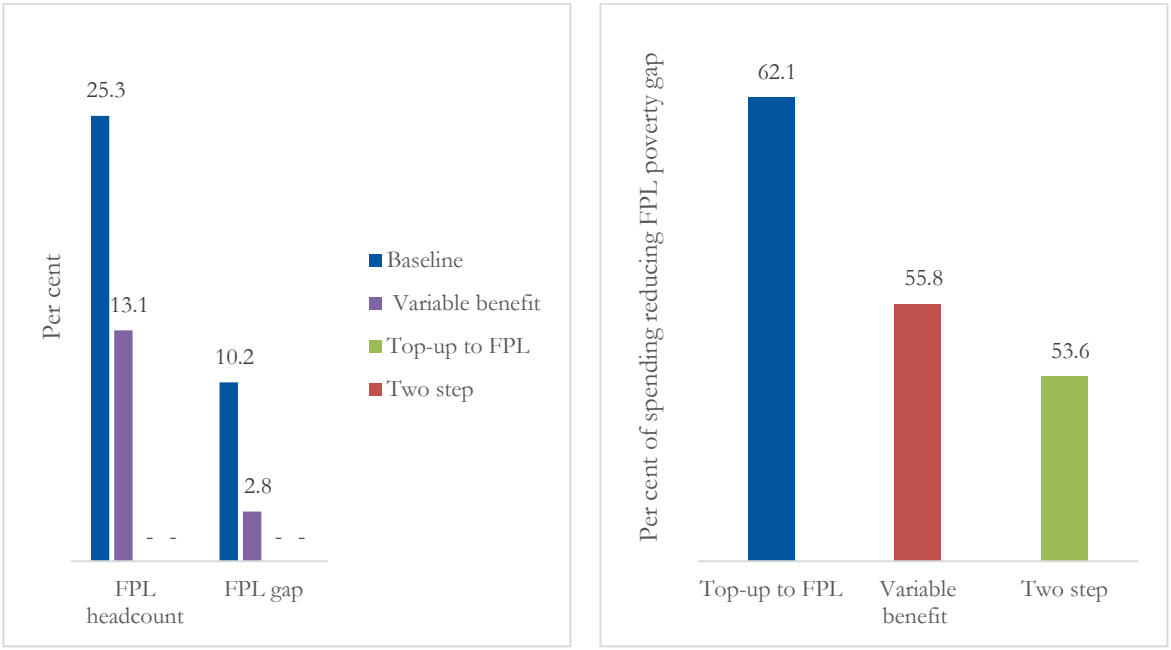
The variation which is a pure top-up to the FPL is unsurprisingly the most efficient grant at targeting the FPL, with the only Spillover due to the difficulties with observing Disposable income. The variable benefit and the two step both result in more expenditures going to targeting LBPL and UBPL poverty and above.

More surprisingly, however, while both the two-step and variable benefit variations are highly efficient the two-step variation is marginally less efficient than the simple variable benefit (Figure 10b). This despite the former being explicitly designed to close the gap to the FPL. This is because, of the 20.0 million beneficiaries of the two-step grant, only 15.1 million are actually poor at baseline Disposable income (rather than just observable income), and of those 10.1 million receive a higher

[33] Disposable income is Observable income plus remittances, imputed rent, and the value of the housing and Free Basic Services near-cash transfers, see Section 2.2 on Methodology.

grant amount from the two-step than from the variable benefit to adults, thereby more likely to take them above the poverty line than in the variable benefit variation. 4.4 million individuals receive the same size transfer in both variations.

Figure 10: FPG a. Poverty reduction (LHS) and b. FPL Efficiency (RHS)



Note: poverty is measured at Disposable income.

Source: authors' calculations based on the LCS 2014/15, QLFS 2020 Q1, and QLFS 2020 Q2.

The FPG, while a promising candidate in theory, requires substantial new infrastructure for implementation, which may take time to build. Its success hinges on the capacity of the administration to build a system that is able to carefully enforce accurate self-reporting of incomes and establish a household registry. Evidence from implementation of the CSG shows, unsurprisingly, that without such an enforcement mechanism some households will incorrectly declare zero income (National Treasury, Personal communication, 17th September 2021).

There is also the risk that changing the grant conditions to take account of family income would have household formation effects - incentivising household reformation to optimize grant claims, and creating further dependence on the willingness of household members to share. If the household registry is only infrequently updated, it may also 'trap' people into households they would rather leave, in order to receive the grant. More generally, such a dramatic shift in the structure of social support in South Africa requires research into its sociological implications (Klasen & Woolard, 2009; Hamoudi & Thomas, 2014).

We select the variable benefit for the comparison in Section 4, which is relatively simpler to administer, though it still requires a database measuring household income and household membership.

3.5 Public works programme

3.5.1 Description and methodology

For a variety of reasons, some people prefer social expenditure through public employment programmes rather than cash transfers. We do not view these cash transfers and public works

programmes as perfectly substitutable or mutually exclusive, rather seeing both types of interventions as having distinct, complementary benefits.

It is important to note upfront that our modelling here is a more incomplete accounting of costs and benefits than in the cash transfer simulations. We are unable to take into account all of the additional complexity that is present in a public works programme in such a way as to make it comparable with the cash grants discussed here, such as the positive services the work provides to communities, the opportunity cost of participants' time spent in jobs which preclude other unpaid work (such as care work), and the potential learnings or pathways to future jobs for programme participants. We also cannot account for the costs of programme administration or work inputs, which are understood to be higher than for cash transfer schemes. Nonetheless, if, given a finite budget, we are choosing between implementing an additional cash transfer programme or a public works programme, we think it is still useful to see how the programme compares against the usual metrics used for evaluating cash transfer programmes, while recognising that the programme as a whole should not be judged on that basis.

We run four variations in which eligible individuals are paid an hourly rate for 100 days of work in the year. Eligible individuals are persons aged 18-59 who are not formally employed but are economically active. Assuming that binding constraints are due to challenges of scaling up more than budget constraints, we run two variations capped at 1 million and 2 million programme participants respectively, and for each of these we run two different wage variants, namely:

- Payment at NM Wage (R21.69 per hour / roughly R1450 per month for 100 days over 12 months);
- Payment at EPWP wage (R11.93 per hour / roughly R800 per month for 100 days over 12 months).

While information on the distribution of labour in the Community Work Programme was not available, the distribution of Expanded Public Works Programme participants roughly follows the distribution of unemployment across income deciles (see Appendix C).[34] We therefore randomly select participants from the pool of 14.7 million eligible individuals. Note, however, that basing the programme on the distribution of Expanded Public Works Programme, rather than the Community Work Programme, may make the programme appear less targeted to the poor and the reader should keep this in mind when looking at the comparisons in Section 4. Many of the Expanded Public Works Programmes require some level of skill (Meth, 2011), and the work also tends to be more urban in nature than the Community Work Programme (K. Philip, Personal communication, 21st September 2021).

[34] This result is based on analysis from the General Household Survey and the Labour Force Survey data on the Expanded Public Works Programme.

3.5.2 Results

By design, the public works programmes have 1 to 2 million direct beneficiaries and provide a wage to workers of either R795 or R1446 for 8 days work per month.[35] This works out to a cost of programme wages of between R9.5 and R34.7 billion.

Table 10: Public works value, beneficiaries, and total annual cost

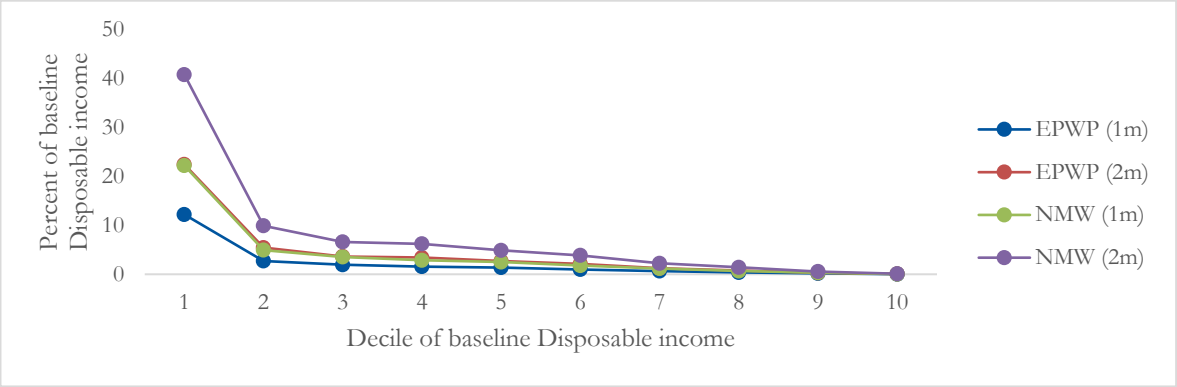
Simulation	Average value	Total beneficiaries (millions)		Annual cost
	Individual (per month)	Direct	Indirect	(R, billion)
EPWP-Wage at 1m	795	1.0	5.07	9.5
EPWP-Wage at 2m	795	2.0	9.84	19.1
NM-Wage at 1m	1 446	1.0	5.07	17.3
NM-Wage at 2m	1 446	2.0	9.84	34.7

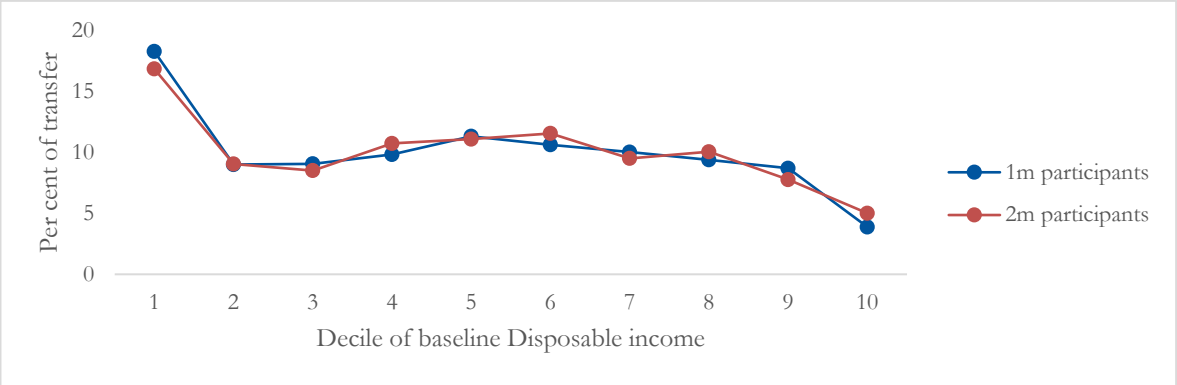
Source: authors' calculations based on LCS 2014/15, QLFS 2015 Q1, QLFS 2020 Q1, and QLFS 2021 Q1.

Given that the beneficiaries are all randomly selected out of the unemployed, the two 1-million-participant variants have exactly the same concentration shares, as do the two 2-million-participant variants (Figure 11b). The incidence graph, then, simply reflects the number of beneficiaries, and the size of the wage for the two programmes, as does the impact on poverty (Figure 12a). As a result the incidence of the NMW variation with 2 million participants is the highest, and the incidence of the EPWP variation with 1m participants is the largest. The EPWP variation with 2 million participants and the NMW variation with 1-million participants are very close (Figure 11a).

[35] 8 hours per day, 8.3 days per month (100 days divided by 12 months), at the EPWP rate of R11.93 per hour, or the NMW rate of R21.69 per hours (Department of Employment and Labour, 2021).

Figure 11: Public works a. Incidence (top panel), b. Concentration shares (bottom panel)





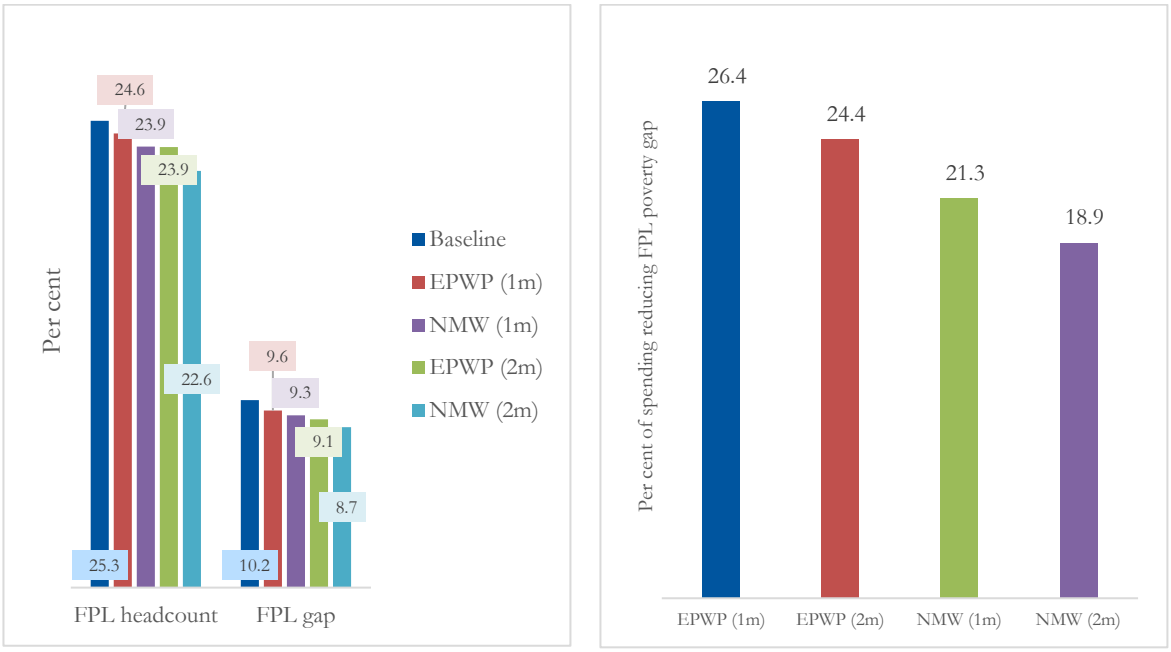
Note: poverty is measured at Disposable income.

Source: authors' calculations based on the LCS 2014/15, QLFS 2020 Q1, and QLFS 2020 Q2.

It is clear which variant will have the smallest and largest impact on poverty—the EPWP-Wage variant with 1 million participants provides the lowest wage to the least workers, while the NM-Wage at 2 million participants provides the highest wage to the largest number of workers. What may be less obvious, *a priori*, is that despite it having a larger budget and reaching more participants directly, that providing an EPWP wage to 2 million people reduces the FPL poverty gap marginally less than providing a NM wage for 1 million people, given the indirect impact of household members co-residing with a participant in the EPWP scenario (Figure 11a). In practise, of course, the latter depends on intra-household dynamics, which we do not take into account here.

The efficiency of each variant at targeting the FPL poverty gap is unsurprising, with the programmes which provide the lower wage and the least participants appearing more efficient, while the programmes which provide the higher wage and the most participants appear less efficient. A higher wage and a higher number of participants leads to a higher average amount per household (due to a higher number of average recipients per household), which results in greater Spillover.

Figure 12: Public works a. Poverty reduction (LHS) and b. FPL Efficiency (RHS)



Note: poverty is measured at Disposable income.

Source: authors' calculations based on the LCS 2014/15, QLFS 2020 Q1, and QLFS 2020 Q2.

In Section 4 we compare the NM-Wage option at 2 million individuals despite it being the least effective option in terms of targeting the poverty gap. This is because of the relatively small size of the public works programme budgets compared to the other scenarios, and in light of recent impulses to pay at least the national minimum wage even in public employment schemes (for example, the Presidential Employment Stimulus). The higher efficiency of the EPWP-wage option, and its reduced reliance on intra-household sharing should be borne in mind when interpreting the results.

4 Comparison

4.1 Results

Putting all of this together, we end up with a range of selected options with a total annual cost ranging from R27.7 billion for the CSG top-up, to R206.4 billion for the BIG with clawback (Table 11). Note that most of the options are potentially amenable to scaling up or down, however, and we show scaled versions of these variations in Section 4.3 below.

Table 11: Comparison value, beneficiaries, and total annual cost

Simulation	Average value	Total beneficiaries (millions)		Annual cost
	Individual (per month)	Direct	Indirect	(R, billion)
CSG with increased grant size*	623	7.0	30.0	27.7
SRD with FPL threshold	350	16.9	43.1	70.6
BIG (clawback)	597	28.8	54.3	206.4
FPG variable benefit**	1 001	4.9	19.8	58.7
Public works (NMW) at 2m	1446	2.0	9.8	34.6

Note: * Average value of grant varies slightly from the statutory rate, given that values are reported directly by survey participants. See Section 3.1.1. for more detail. ** Average value is per household, and direct beneficiaries reports number of households.

Source: authors' calculations based on LCS 2014/15, QLFS 2015 Q1, QLFS 2020 Q1, and QLFS 2021 Q1.

Looking at the efficiency of these grants at targeting FPL poverty allows comparison of these methods despite the large budget differences (Figure 13a). The FPG grant is the most efficient, as it directs the largest share of its expenditure to reducing the FPL poverty gap (with an FPL efficiency score of 62.1), and costs roughly R60 billion. This is unsurprising given its household targeting mechanism. On average households receive a benefit of R1000 per month (reflecting that recipient households have an average of 2.2 adults per household). In total it reduces the baseline Disposable income FPL poverty gap by 73 per cent (only the BIG is able to achieve more than 80 per cent reduction, Figure 13b).

If truly provided to all those who are eligible without implementation error, the SRD has the second highest budget at R71 billion, despite paying out only R350 in grants per individual, and it reduces the poverty gap by 55 per cent. The budget is so high because 17 million working-age adults lack formal employment and receive individual monthly income of less than R624 per month (if they receive any individual income at all) - a strong reflection of the depth of the unemployment and poverty, and the importance of developing a social protection package to protect these individuals, who are largely excluded from the current social grant system. One of the strengths of the SRD option, then, is its wide coverage; it indirectly reaches 43.1 million individuals (72 per cent of the population), if properly implemented. It has the second highest efficiency score out of the grant options considered (at 35.1 per cent), and in terms of administrative complexity it builds on an existing yet imperfect system, and falls somewhere between the extremely demanding FPG and more universal BIG. In its theoretical form, the SRD is an appealing grant option, although as we explain in Section 3.2.3 and Appendix B, in reality there have been implementation problems.

The CSG top-up option is marginally less efficient at reducing FPL poverty than the SRD at 29.5 per cent, but it is exceptionally easy to implement. It costs only R28 billion, and it reaches 7.0 million caregivers who each receive about R623 per child.[36] The major benefit of a CSG top-up, apart from its targeting to women and children, is that its administrative infrastructure already exists, and we know it can be accurately implemented. A primary disadvantage is that a top-up would not extend the social safety net, instead supporting those who already receive some benefits. In particular, unemployed, prime-aged adults who are not disabled will only receive benefits

[36] A reminder that this is lower than the proposed new statutory rate of R624 given that this is based on direct identification and annual amounts, and not all caregivers will have received the grant for the full year.

indirectly, via household sharing, if they have CSG-receiving children in the household. Given that the CSG and SRD are both implementable and similar in efficiency, a combination of the two (with the CSG including the raised threshold) would cost R98 billion and reduce the FPL poverty headcount and gap to 13.7 and 3.5 per cent respectively.

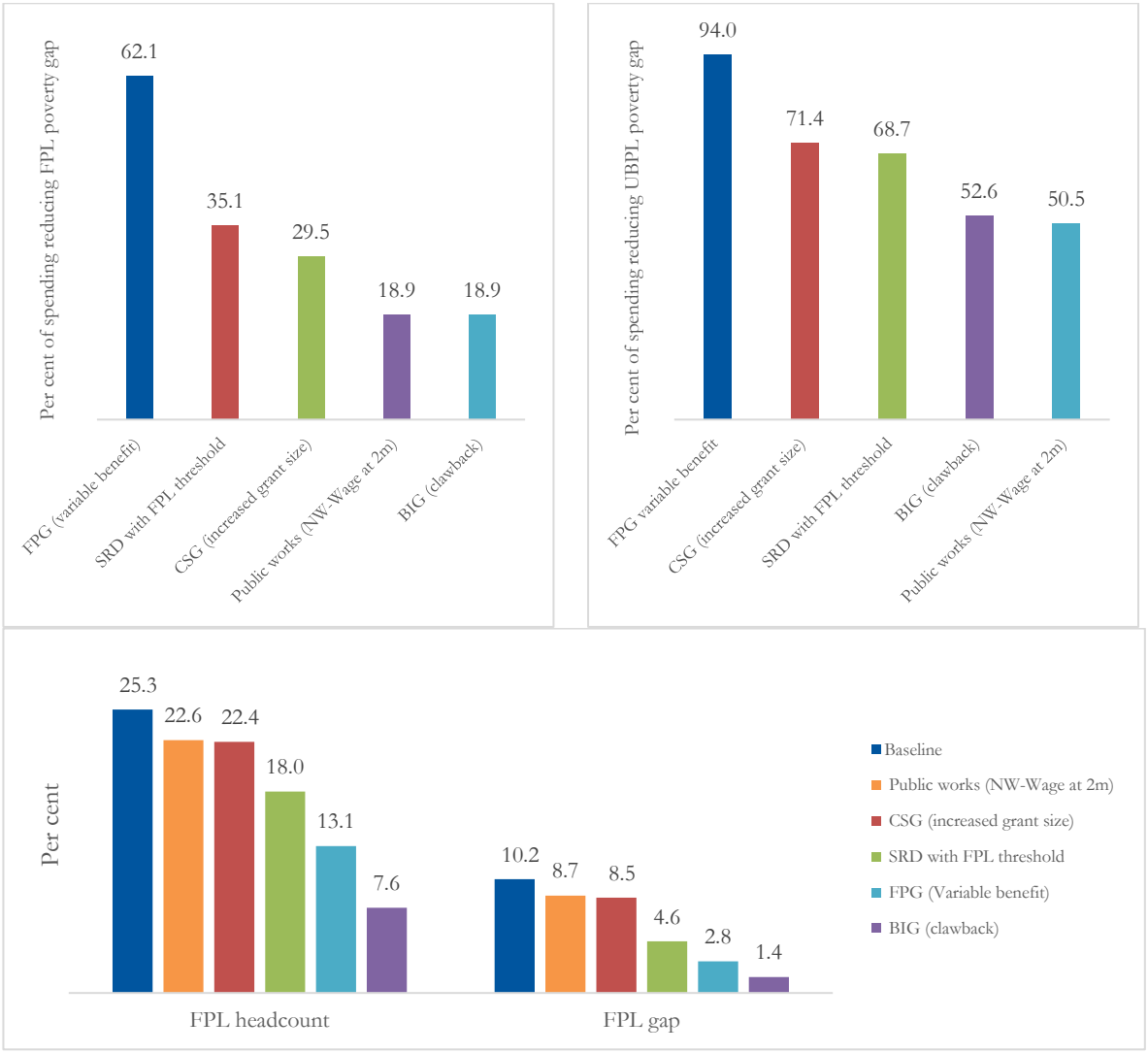
The public works scenario has a wage bill of R35 billion, and the smallest number of recipients, and so the disbursement per individual is higher than any of the grant options, at roughly R1 500 per month in return for 8 days of work per month (2 days per week). While this is appropriate as compensation for work done, it is not the most effective means of *immediately & directly* alleviating extreme poverty, and the poverty gap is reduced by only 14 per cent. This highlights that using a public works programme for immediate poverty alleviation results in an inherent conflict of priorities, while it nonetheless remains relevant in reducing future poverty by building human capital and reducing unemployment. We cannot model some of the most important supposed benefits of jobs programmes, such as learning-on-the-job and services to local communities, which may reduce poverty in the longer term, and which are not measured in our analysis. In our view direct comparison between the public works programme and the other grants is difficult, and while it can be used as a complementary measure to a grants-based poverty alleviation strategy, it should not be seen as a replacement. The scenario is as efficient at the FPL as the BIG (at 18.9 per cent).

The BIG with clawback—relatively untargeted compared to the other grants—ties for the least efficient at targeting the FPL poverty gap at 18.9 per cent and has the highest budget at R206 billion.[37] However, it is of course the option which comprehensively targets all working-age poverty, not just at the FPL, and reduces the FPL poverty gap the most, by 86 per cent. It is also likely to reduce errors of exclusion due to implementation issues. The BIG entails a very large poverty reduction at very large cost, and ultimately its desirability will depend on judgments about the feasibility of significantly raising taxes, and/or how the BIG is valued relative to existing and future expenditures which could have their spending reduced to support the BIG. While these decisions have technical components, they are also normative and political questions about government priorities which are beyond the purview of our technical paper.

Note that the BIG with clawback is similar to the R650 SRD grant with the NMW threshold variation discussed in Section 3.2.1. The main difference is that the BIG includes formal workers below the NMW, as well as individuals receiving other grants (NSFAS, the disability grant) and the SRD does not. This reduces the total cost from R206 billion to R149 billion, and the number of beneficiaries from 28.8 million to 19.7 million. The FPL efficiency score increases from 18.9 to 24.9 with the SRD. This can be explained, once again, due to the smaller grant amount received on average (due to a smaller average number of recipients) per household, which reduces the proportion of Spillover (R1206 or 1.9 grant recipients per household for the BIG, versus R1096 or 1.8 grant recipients per household for the SRD). The SRD efficiency score is higher, but this may not compensate for the implementation challenges experienced by the SRD that we expect will be less prevalent with a BIG.

[37] Note that if we had used the BIG variant targeted to the NMW, the grant would rank just above the public works programme with an efficiency score of 20.8.

Figure 13: Comparison: a. Efficiency at FPL (top LHS panel), b. Efficiency at UBPL (top RHS panel) and c. Poverty reduction (bottom panel)



Note: poverty is measured at Disposable income.

Source: authors' calculations based on the LCS 2014/15, QLFS 2020 Q1, and QLFS 2020 Q2.

4.2 Sensitivity to poverty line

As we have attempted to highlight throughout this paper, comparing grants based purely on their impact on the FPL poverty headcount and gap is undesirable. It discounts the impact that these grants have on those above the FPL, and yet who are still poor (measured by the UBPL) or vulnerable.

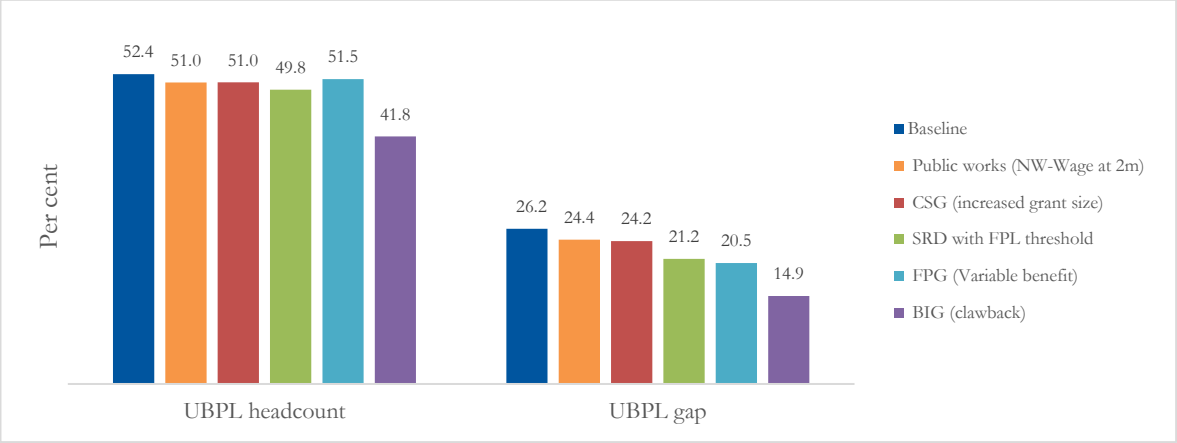
Figure 13b shows the proportion of spending which reduces the *upper-bound* poverty gap. As would be expected, all grants have higher efficiency scores at the upper-bound, as a greater proportion of their expenditure reduces the UBPL gap than the FPL gap, by definition. At the larger poverty line, the CSG top-up is now more efficient than the SRD, while the BIG is slightly more efficient than the public works programme.

The efficiency measures need to be interpreted in conjunction with gross poverty reduction associated with each scenario. Transferring R1 to the poorest person in the country would entail a

100 per cent efficiency score according to this measure but would clearly not indicate a viable poverty reduction program.

The BIG does the most to reduce poverty at both the FPL (Figure 13c) and UBPL (Figure 14), followed by the FPG (when looking at gross poverty reductions, in general the poverty gap is a superior measure than the headcount ratio). Both the BIG and the SRD perform significantly better at UBPL poverty reduction than they do at FPL poverty reduction. The BIG, in particular, shows how well-suited it is for poverty reduction at greater scale.

Figure 14: Comparison: UBPL Poverty reduction



Note: poverty is measured at Disposable income.

Source: authors' calculations based on the LCS 2014/15, QLFS 2020 Q1, and QLFS 2020 Q2.

4.3 Scalability

An additional measure by which we can evaluate the grant options discussed in this paper is by their scalability - understanding how their impact and efficiency changes as we scale them up or down. A grant which is highly sensitive to programme size, will be less flexible as an instrument, and this may prejudice impact as poverty goals change over time. Here we rescale all the grant programmes to the same total budget size by adjusting the per-person grant value (wage values in the case of the public works programmes), and then examine the change in efficiency of these similarly sized interventions, and their impact on poverty reduction.

The results cannot tell us about the efficiency of the implemented programme at its actual size but rather tell us how the programmes would compare *were they to be assigned the same budget*. This exercise is most useful if the budget for the intervention is clear in advance. After discussion with Treasury officials, we scale the size of each scenario and variation up or down until we reach a budget of R45.4 billion—the size of the Disposable income gap.

The results demonstrate which programmes are more or less sensitive to programme size, given the number of participants, and the distribution of benefits. The change in the average size of the grant is shown in Table 12 below, and the scaled impacts on poverty and efficiency are shown in Figures 15 and 16.

Table 12: Average value of grant, original, and scaled

Scenarios	Average value to individual, per month		Total beneficiaries (millions)	
	Original	R45.4b	Direct	Indirect
CSG Increased grant size (R624, R4600)*	330	541	7.0	30.0
SRD at FPL threshold	350	225	16.8	43.0
BIG (clawback)	597	131	28.8	54.3
FPG variable benefit	1 001	774	4.9	19.8
Public works (NW-Wage at 2m)	1 446	1 897	2.0	8.5

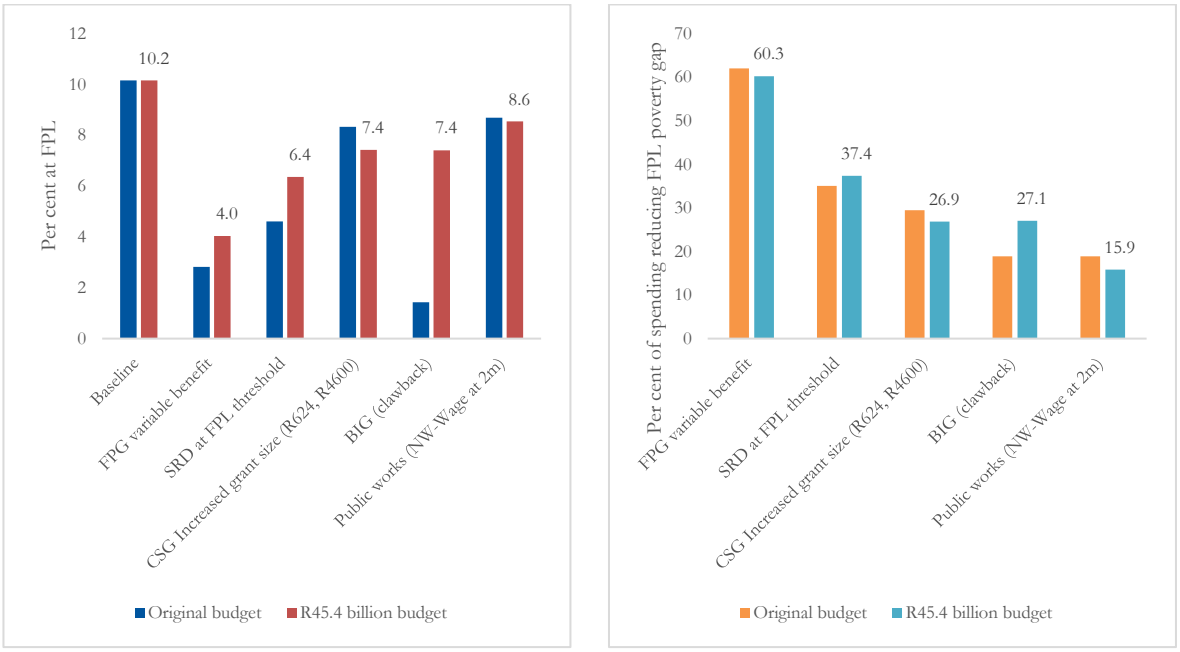
Note: * Amounts shown here are per caretaker, for ease of comparison.

Source: authors' calculations based on LCS 2014/15, QLFS 2020 Q1, and QLFS 2021 Q1.

The ranking of grant impact on the poverty gap at the FPL remains the same, except for the scaled-BIG, which now drops from its position as the grant with the most impact (and the largest budget), to having the same impact as the scaled-CSG. The value of the scaled-BIG reduces dramatically to less than a quarter of the average scaled-CSG top-up value in order to provide 29 million beneficiaries with the grant within a R45 billion budget envelope. Its efficiency ranking at the FPL also increases from the least efficient of the grants, to just slightly above that of the scaled-CSG. For details of within scenario reranking of variations at the R45 billion envelope, see Appendix D on Scalability.

The CSG and public works Programmes become less efficient at the larger size. When we increase the size of the wage, we concentrate more expenditure with a few individuals. The Spillover is larger, and the additional impact on poverty is relatively small.

Figure 15: Scaled grants, FPL a. Poverty impact (gap) (LHS), and b. efficiency (RHS)

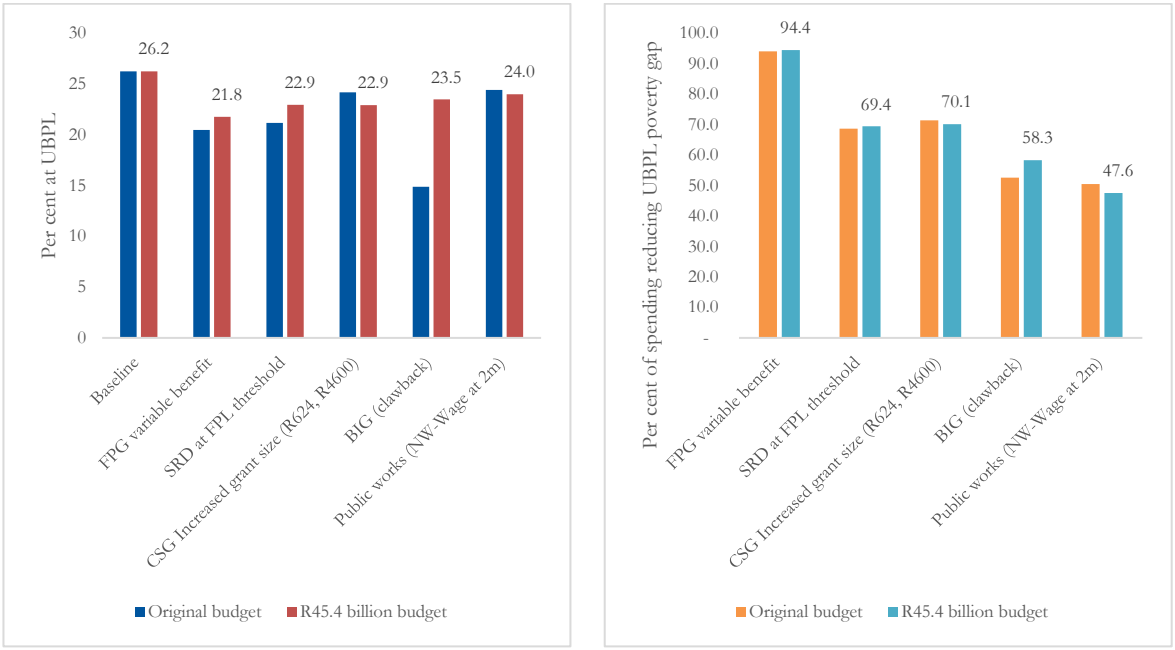


Source: authors' calculations based on the LCS 2014/15, QLFS 2020 Q1, and QLFS 2020 Q2.

At the UBPL the rankings of the grant impact on poverty don't change, although the differences between the grant impact reduces and the scaled-SRD and scaled-CSG are now equally impactful.

In terms of efficiency, the scaled-CSG overtakes the scaled-SRD at the UBPL, as with the original rankings, and the scaled-BIG drops back to fourth place, below the scaled-CSG and the scaled-SRD.

Figure 16: Scaled grants, UBPL a. Poverty impact (gap) (LHS), and b. efficiency (RHS)



Source: authors' calculations based on the LCS 2014/15, QLFS 2020 Q1, and QLFS 2020 Q2.

4.4 Theory meets implementation

This paper focuses on the theoretical impact of the grant design, and does not attempt to comprehensively quantify implementation risks. Nonetheless, once the theoretical results are understood, policymakers will naturally need to investigate the feasibility of implementation of various options in conjunction with various stakeholders before they can move ahead. As discussed, how well these grants perform in practice, compared to how they are theoretically modelled here, will depend significantly on how they are implemented, and this should be an important consideration in policy selection, in particular given that the implementation errors (as discussed in Sections 3.2.3 and Appendix B) may cause a reranking of the options compared to our theoretical efficiency ranking presented earlier.

The theoretical results of this section show that the FPG is exceptionally efficient at reducing FPL poverty. However, the implementation challenges associated with introducing a new household-targeted grant, discussed in Section 3.4.1, are uniquely acute, and more research is required to determine whether concerns about unintended consequences of the new grant can be adequately addressed. The FPG is also likely to be particularly sensitive to inclusion errors which will affect its efficiency ranking.[38]

[38] Given the large proportion of individuals receiving the SRD with income above the FPL, errors in SRD inclusion that tend to include only those above the FPL will likely only decrease efficiency slightly. Errors in FPG inclusion, on the other hand, will alter the baseline distribution of the grant to poor claimants substantially, since those correctly included are highly likely to be poor while those incorrectly included are highly likely to be non-poor. Substantial

The clawback BIG is at the other end of the spectrum: relatively easy to implement due its lack of complex targeting, but the most inefficient when it comes to FPL poverty reduction, entailing large poverty reduction but at a very large cost. The BIG is especially inefficient at reducing FPL poverty, performing better at the UBPL.

The SRD presents some kind of middle-ground, with a policy that is certainly more implementable than the FPG, but is not free from serious problems in its current implementation, as discussed in Section 3.2.3 and Appendix B, which must be addressed if the policy is made permanent, including a coordinated database and reliable income assessment mechanism, and identifying and resolving current barriers to access.

The CSG top-up and the public works programs do not look especially appealing in our comparative analysis of this section, but our analysis does not account for the ease of CSG top-up implementation, or possible non-modelled benefits of public works, discussed in Section 3.5.

5 Conclusion

In this paper we aim to provide technical information which will assist the reader in thinking through the options of a social grant for the medium to long term which will substantially reduce the FPL poverty gap. We examine a set of five scenarios, each with various design variations, and show the impact of each on poverty reduction, and provide graphs of concentration shares and incidence per decile to explain the results. We also compare the proportion of expenditure which goes towards reducing the FPL poverty gap as a measure of FPL poverty reduction efficiency. We then select one variation from each scenario to compare across scenarios. The results are summarised in Table 13.

The FPG and BIG are entirely new grants, and the SRD and public works programmes are simulated, given the lack of accurate information in the LCS on actual recipients. In assuming perfect take-up for most variations, we model the upper bound impact of the grants for each scenario, except the CSG, where our analysis is based on actual receipt of the grant in the survey data. Our new but still limited experience of actual SRD roll-out allows us to provide a summary of some SRD implementation issues and highlight the extent to which they have mitigated the grant's impact on FPL poverty reduction compared to the theoretical expectation. For all of the grants, accurate implementation will be vital if we hope to realise poverty reduction effects similar to what we have discussed in this paper.

The FPG variation, at around R60 billion in grant disbursements, is theoretically able to most efficiently target the FPL and UBPL poverty gap. There are acute implementation difficulties associated with this new household-targeted grant and it would require up-to date data on households, substantial capacity building, piloting and a medium-term horizon to roll it out. An entirely new household means-testing process and household registry infrastructure would need to be created.

The BIG, at over R200 billion in grant disbursements is the least targeted, and so the least effective, and yet has the largest impact on poverty due to the sheer numbers of grant recipients and size of the budget. This is important, because while the BIG policy design is able to scale to a size where

implementation errors are therefore more likely to have a substantial impact on the efficiency score (and even ranking) of the FPG.

it can largely eradicate poverty, many of the other options examined here (e.g. public works, the CSG) are not. Given the inaccuracies in the currently-implemented SRD means-test, a BIG mechanism might preclude administrative errors of exclusion, although barriers to accessing the application process will likely remain. The BIG could make use of the existing tax system to manage targeting via clawback of the grant from taxpayers, rather than requiring new targeting infrastructure. The extreme inefficiency of the BIG at the FPL looks less concerning at the UBPL.

The SRD, at R70 billion in grant disbursements, is less efficient than the FPG, and does not rely on the difficult-to-operationalise concept of a 'household', yet it nonetheless requires significant improvements in current administrative capacity for it be a realistic long-term social protection measure. The SRD presents as a middle-ground between the highly efficient but potentially unfeasible FPG, and the inefficient but likely more feasible BIG. It also works reasonably well at both the FPL and UBPL but improves in performance when UBPL poverty is the target.

The public works scenario at R35 billion in wage disbursements, modelled based on the distribution of Expanded Public Works Programme participants, suggests that using a public works programme to immediately and directly alleviate poverty is unlikely to be as efficient an instrument as a well-targeted cash transfer. We expect this result to hold less strongly using the distribution of Community Works Programme participants, but there is no survey data to confirm this. Nonetheless, public works programmes have a host of other potential benefits, which do not enter into the metrics used here and so are not discussed, that make it worthy of consideration as a complementary policy measure to a grant that targets poverty.

Finally, the CSG top-up at R28 billion is an established method for reducing poverty and requires little additional administrative capacity. It performs reasonably well at the FPL and outperforms the SRD at the UBPL in terms of efficiency, although the SRD achieves more poverty reduction. It is a ready vehicle for poverty reduction, and the impacts modelled here are likely the closest to what the actual poverty impacts would be, compared to our other scenarios. It has limited capacity to scale up, however. Normatively, one may have concerns about using a tool for reducing child poverty to target poverty in general, and unlike the other options considered, the CSG top-up does not directly entail expanding the social security net to new recipients excluded from the current system.

Table 13: Summary of headline results

Rank	Poverty reduction (FPL gap)	Efficiency (FPL)	Poverty reduction (UBPL gap)	Efficiency (UBPL)	Implement ability
CSG (increased grant size)	4	3	4	2	1
SRD with FPL threshold	3	2	3	3	3
BIG (clawback)	1	4.5	1	4	2
FPG variable benefit	2	1	2	1	5
Public works (NMW) at 2m	5	4.5	5	5	4

Note: where a grant's ranking is tied with another grant, we use an average of the two scores for both grants.

Source: authors' elaboration.

Policy always entails trade-offs, but the results here, along with additional work forthcoming which models a variety of options for raising additional revenues through tax collection, confirms that it is possible to make a substantial dent to South Africa's extreme poverty levels in the medium- to long-term. We estimate that the SRD at full take-up including implementation issues, reduces the remaining poverty gap (of 10.2 per cent at Disposable income, post-the inclusion of social grants),

by roughly 40 per cent. The SRD has the *potential,* however, to reduce the gap by 55 per cent in the medium term, when implemented with a value of R350 and an individual FPL means test. In the long-term a well-administered FPG which provides a variable benefit based on the number of adults in the household, or a BIG for working-age adults, could reduce the poverty gap by 72 and 86 per cent respectively.

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Appendix

A Data

The Appendix contains a brief description of the data method used in this paper and robustness checks.

The primary challenge we faced in doing this analysis is that the datasets available for household income analysis in 2021 are well out of date, and most relevantly are all pre-pandemic. In this paper, we use the Living Conditions Survey (LCS) collected in 2014/15 as it has the most detailed disaggregation of income and expenditure, is the official dataset used to calculate the poverty statistics, and it feeds into the model generated for the 2014/15 CEQ Assessment (see Goldman, Woolard & Jellema, 2020). However, we compare against results in the National Income Dynamics Survey (NIDS) collected in 2017 to test for robustness.

We update demographic and employment variables to reflect the COVID-19 employment loss. We do this in three steps. Firstly, we forecast income to pre-pandemic levels using per capita growth in GDP. Secondly, we reweight the dataset to a) match 2020 demographics, disaggregated by race, age, gender and province, and b) match the administrative records on the taxable income distribution. Finally, we use the Quarterly Labour Force Survey (QLFS) to calculate the change in employment from 2015 Q1 to 2020 Q1, and from 2020 Q1 to 2021 Q1, and implement these changes in the LCS dataset by randomly shocking certain individuals from employment to unemployment, based on a set of demographic and employment characteristics.

There are many assumptions built into this updating process. We test for implausible deviations and alternative assumptions using other datasets, but there is unavoidably some uncertainty. Further details of the data construction, robustness tests and illustrations of their use will be available in a forthcoming working paper.

Income and consumption update

Following Younger et. al (2020), we inflate the Statistics South Africa (Stats SA) consumption-based welfare aggregate from 2015 using per capita growth in GDP to pre-pandemic 2019 levels.[39] This results in an increase in 2019 consumption expenditures of 17 per cent. We then calculate shares of reported income for each component of income (remittances, royalties, annuities, alimony, rent, farm, interest, dividends, shares, unit trusts and pension income) and multiply that by the Stats SA welfare aggregate. We use these new income components to recalculate gross taxable income and earnings in the dataset.

The result is a 2.3 per cent decline in Disposable household income from pre- to mid-pandemic in the LCS, compared to a 2.0 per cent decline in GDP in the administrative records, and a 4.9 per cent decline in NIDS, compared to a 1.1 per cent decline in GNI in the administrative records.[40]

[39] Our process differs in that we use nominal, rather than real growth, and we do not implement the 85% pass-through.

[40] Note that we use GDP in the LCS, because we begin by updating the welfare aggregate, based on household consumption, whereas we use GNI in NIDS, because we update the Disposable income aggregate, based on household income.

Table 14: Income update validation

Statistic / aggregate	2014/15 (R)	2019/20 (R)	Percentage change	2020/21 (R)	Percentage change
		LCS			
GDP	73 690	86 375	17.2	84 606	-2.0
Disposable income (LCS)	41 175	47 763	16.0	46 675	-2.3
		NIDS			
GNI	79 866	83 926	5.08	83 007	-1.1
Disposable income (NIDS)	49 646	52 679	6.1	50 094	-4.9

Source: authors' calculations based on LCS 2014/15, NIDS 2017.

Demographic updating

We update the demographic characteristics of the LCS 2015 sample to match the Statistics South Africa (2020) mid-year population estimates by age, gender, race and province totals. We also match the proportions of taxpayers by income bracket with the tax records (National Treasury, 2020). The process consists of re-weighting the sample, as outlined in Wittenberg (2008), using Wittenberg's 'maxentropy' programme in Stata.

Employment updating

We use the Quarterly Labour Force Survey as the benchmark indicator of the state of the labour market. We calculate changes in QLFS employment between 2015q1 and 2020q1, and between 2020q1 and 2021q1, by demographic (age and education) and employment (informal vs. formal sector) cells. We then match these changes in the LCS by changing the employment status of a randomly selected proportion of individuals in each cell, until the percentage employment change in each cell matches the QLFS. For individuals whose employment status changes from not employed to employed, we assign the median earnings from the relevant employment cell.

Comparisons to other datasets

We use the LCS for this project for two reasons: i) it is the official dataset used to calculate poverty and inequality statistics, and ii) it is the dataset underlying the South African CEQ Assessment. However, the National Income Dynamics Survey of 2017 (NIDS) has the advantage over LCS of containing detail on sector and occupation data,[41] as well as broad informality (e.g. informal employment in the formal sector). It is also more recent. We therefore perform a similar updating process on the (NIDS) with these additional characteristics to create finer matches with the QLFS data, and compare the results.

We also compare to the SA-MOD dataset created by Michael Noble and Gemma Wright. This dataset uses NIDS 2017 and updates by *reweighting* demographic and employment characteristics, in contrast to our employment updating process which adds and subtracts earnings income from individuals as we shift their employment status. We chose the latter approach because it has the benefit of not assuming that individuals who become unemployed during the pandemic live in households which resemble those of individuals who were unemployed before the pandemic. A

[41] The LCS has some information on sector and occupation, but it is sparse, and in an open-response format, which we were not able to make use of within the timeframes of this project. The matching process with the QLFS could be improved in the future, however, by classifying these variables using the additional detail provided in the QLFS.

similar method is used by the CEQ Institute to measure the impact of the lockdown on poverty and inequality in various countries (see, for example, Younger et. al. (2020)).

Summary statistics of employment proportions

We present statistics of employment, for totals and by category, for our main dataset (LCS), the reference dataset (QLFS), as well as the robustness datasets (NIDS and SAMOD). The population totals are very similar in all of these for the updated period, at about 34 million.

Employment in the household surveys is generally larger than employment as recorded in the QLFS. While QLFS suggests there were about 15 million employed in 2015 and 15.8 million employed in 2017, the LCS suggests this was closer to 16.3 million in 2015 and the NIDS suggests a figure of 17.5 million in 2017 (Table 13).

Table 15: Employed individuals, LCS, NIDS, QLFS

	Employed individuals (millions)	
Dataset	**Household survey**	**QLFS**
LCS (2015)	16.3	15.0
NIDS (2017)	17.5	15.8

Source: authors' calculations based on LCS 2014/15, NIDS 2017, QLFS 2015 Q1, and QLFS 2017 Q1.

Employment in the SA-MOD dataset (based on the NIDS survey) is closest to employment in the QLFS. Given that SAMOD is adjusted to match the QLFS, rather than applies the change in employment in the QLFS to the change in the survey, this is unsurprising. In 2021 Q1, QLFS employment was 14.5 million, compared to 15.8 million in the LCS dataset post-adjustment, 15.3 million in NIDS, and 14.2 million in SAMOD (Table 14).

Table 16: Employed individuals post-adjustment, LCS, NIDS, QLFS, SA-MOD

Dataset		**Employed (millions)**
QLFS 2021 Q1		14.5
Post-adjustment	LCS	15.8
	NIDS	15.3
	SA-MOD	14.2

Source: authors' calculations based on LCS 2014/15, NIDS 2017, QLFS 2015 Q1, QLFS 2017 Q1, SAMOD.

Correspondingly, the proportions employed by each category (sex, age, race, education, rural, informality status) in the adjusted LCS and NIDS are generally higher than in QLFS and SA-MOD. Note that the sparser matching of the LCS does over-estimate the employment drop for some categories, such as the White population group, which while likely not a problem for the analysis of poverty undertaken in this paper, does preclude more granular analysis by race.

Table 17: Proportion employed by sex, age, race, education, geographical area, informal status

	QLFS			LCS		NIDS		SAMOD
Year	**2015**	**2020**	**2021**	**2015**	**2021**	**2017**	**2021**	**2020**
				Stats				
Total population (millions)	31.3	33.9	34.4	30.7	34.2	32.4	34.2	34.1
Total employment (millions)	15.0	15.9	14.6	16.3	15.8	17.5	15.6	14.2
Employment rate (per cent)	48.1	46.8	42.4	53.0	46.3	54.1	45.7	41.8
				Sex				
Male	54.7	52.5	47.6	59.2	51.3	62.1	52.5	49.8
Female	41.7	41.2	37.2	47.0	41.5	46.3	39.0	34.0
				Age				
18-24	19.3	15.8	11.0	28.1	20.5	27.4	18.1	14.2
25-34	51.2	46.7	41.0	54.5	41.8	60.5	50.2	45.1
35-49	62.9	61.9	57.6	67.2	61.6	65.3	56.6	53.2
50-59	54.5	56.1	52.5	60.1	55.5	53.5	47.0	43.9
				Race				
White	69.7	70.0	68.7	77.4	67.0	65.6	63.4	53.1
Non-White	46.1	45.0	40.4	50.8	44.7	53.1	44.2	39.1
				Education				
Less than matric	40.8	39.5	34.6	44.1	37.6	45.3	34.8	46.8
Matric	51.2	47.6	42.4	58.5	49.1	54.9	41.8	1.8
Tertiary	79.7	75.2	74.7	83.4	77.8	75.3	75.6	10.0
				Geographical area				
Rural	36.1	35.7	32.7	37.4	33.4	43.0	37.6	n.a.
Urban	54.0	51.8	46.8	60.2	52.3	59.1	49.4	n.a.
				Informal				
Sector	12.0	12.4	10.6	17.3	15.8	n.a.	n.a.	n.a.
Broad	13.8	13.5	10.4	n.a.	n.a.	17.2	11.8	3.6

Source: authors' calculations based on LCS 2014/15, NIDS 2017, QLFS 2015 Q1, QLFS 2017 Q1, SAMOD.

Summary statistics of poverty and inequality

The increase in poverty due to COVID-19 employment loss is higher in NIDS than in LCS, at all poverty lines. At the FPL, poverty increases in the LCS by 3 percentage points, versus 4.5 percentage points in the NIDS. This is to be expected given the percentage reduction in income of 4.9 in NIDS versus 2.3 in LCS from 2019/20 to 2020/21. The SAMOD dataset's poverty increase is higher still, given that it is matched to QLFS with its lower employment rates. Finally, inequality as measured by the Gini increases slightly across both LCS and NIDS.

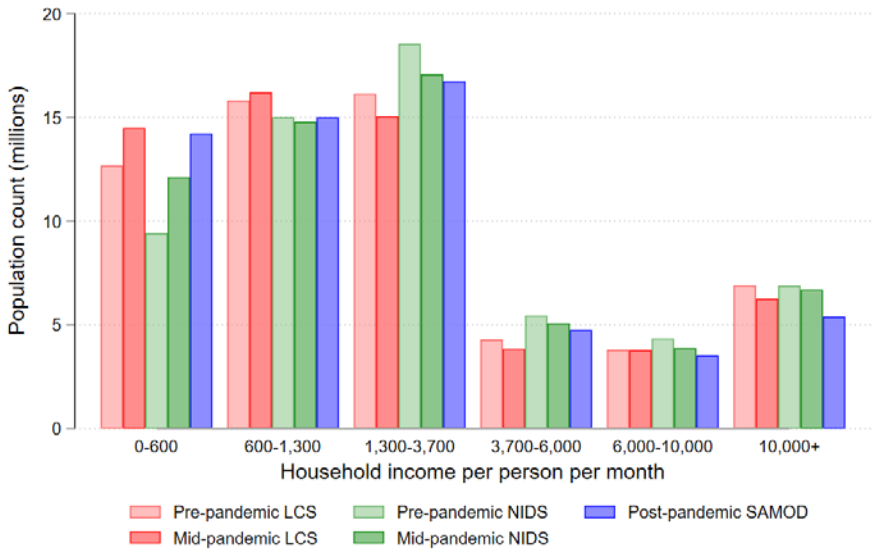
Table 18: Poverty and inequality

	LCS		NIDS		SAMOD
	2015	2021	2017	2021	2020
		FPL			
Headcount (%)	22.2	25.3	16.7	21.7	24.6
Gap (%)	9.2	10.2	5.3	8.9	11.8
		LBPL			
Headcount (%)	33.7	37.44	28.4	33.3	36.7
Gap (%)	14.9	16.59	10.6	14.5	17.5
		UBPL			
Headcount (%)	48.7	52.4	42	46.1	50.1
Gap (%)	23.8	26.24	18.9	23	26.2
		Inequality			
Gini coefficient	68.7	68.34	66.2	65.9	68.3

Source: authors' calculations based on LCS 2014/15, NIDS 2017, QLFS 2015 Q1, QLFS 2017 Q1, SAMOD.

Examining the distributions of per capita household income in the original and adjusted surveys for LCS and NIDS, and for SAMOD, we find that the adjusted datasets all have broadly similar distributions, and that the LCS and NIDS have more individuals in the poorer income categories after the employment adjustment. Figure 15 shows the distribution of household income per person across various brackets, before and after the simulated employment changes for NIDS and LCS (and after for SAMOD). For example, the light-shaded red shows the number of household members in that income bracket in LCS before the employment change, and the darker red shows the number after the employment shock. As expected, there is a large increase in both LCS and NIDS in the lowest bracket, before versus after the employment shock, and there is a correspondingly large drop in the number of people earning between 1,300 and 3,700 for LCS and NIDS. This is due to large net employment losses shifting households down the income distribution.

Figure 17: Household income



Source: authors' calculations based on LCS 2014/15, NIDS 2017, SAMOD.

B Current R350 SRD Implementation

In order to better understand the differences between the numbers of eligible recipients, and the number of beneficiaries of the SRD grant, we held a useful meeting with SASSA representatives, which informs the information contained in this Appendix.

The SRD grant verification process generally does not exclude applicants based on income. A means test is currently only conducted for those who appeal a rejection, and the threshold applied is R585 per month, verified by looking at bank account inflows, where possible. The bank checks rely on bilateral agreements between SASSA and major banks (ABSA, Nedbank, FNB, Capitec Bank and Standard Bank), who check each individual record and indicate to SASSA whether there were flows above R585. Bank statements were verified for only a few hundred thousand out of 13 million applications (likely due to low rates of appeals).

In the September 2021 SRD round, more than 80 per cent of excluded individuals were excluded based on employment status. In general applicants are rejected if they are found to be formally employed, personally claiming another social grant, or receiving NSFAS assistance. While the restriction against claimants of other social grants is easily and accurately verified using SASSA's SOCPEN database of social grants claimants, the verification of employment is an error-prone process that likely results in a high proportion of false rejections.

The employment status of an individual is primarily inferred from two imperfect databases derived from i) records of Unemployment Insurance Fund contributions (UIF) and ii) employee tax returns (IRP5). UIF contributions records are available for the most recent month, and they are often incorrect: companies may fail to update the identity information when they replace employees, which can result in a previous employee with a false employment status, and a new employee with a false unemployment status. IRP5 records are available for the previous tax year (currently March 2020 to February 2021), and indicate whether someone was employed at any point over the entire period. If an applicant was formally employed in March of 2020 and retrenched in April 2020, they will nonetheless have an IRP5 record and be excluded from receiving the SRD (unless they appeal) until February 2021. The impact of outdated employment data is exacerbated by a context of high job turnover in the South African labour market. Employment is also separately checked against databases of public employment such as PERSAL (the personnel and salaries management system used by the national and provincial government) and the equivalent South African Police Services employment database, and the Government Employees Pension Fund database is used to exclude those receiving pension benefits or making contributions.

We roughly estimate the scale of false exclusions to be 1-in-3, based on historic survey data. The false exclusion of SRD applicants based on Statistics South Africa's Quarterly Labour Force Survey (QLFS) suggests that 7.5 per cent of those that are currently not formally employed were actually formally employed 3 months before. Similarly, the NIDS-CRAM survey suggests 10 per cent of those who were not formally employed in October were actually formally employed four months earlier. However the false rejection rate of roughly 1-in-3 is derived from the time lag for the IRP5 data can be up to 18 months.[42] Of course, accounting for false *inclusions*—those who are employed at the time of application but fail to be rejected because they falsely *appear* to be unemployed in

[42] The probability an unemployed person was employed at any point, extrapolating the QLFS estimate from 3 months to 18 months is equivalent to one minus the probability an applicant was not employed 3 months ago (1-0.075) compounded to 18 months (raised to the power 18/3), or 1-(1-.075)^(18/3)= 0.37. The NIDS-CRAM estimate is similar.

the databases—presents further problems to the verification system. Note this estimation is not calibrated to actual rejection rates reported by SASSA—we discuss this below.

Table 17 summarizes the impact on the scenario results once these errors are accounted for. Panel A presents the SRD scenario closest to the current criteria (with full take-up). Headcount poverty at the FPL under the original version of the SRD with full take-up and no errors is reduced from 25.3 to 17.3, and the FPL poverty gap is reduced from 10.2 to 4.5. The grant is received by 22 million claimants, and costs R90 billion per year, with 28.0 per cent of spending going to reducing the poverty gap of poor claimants (see Section 3.2.2).

Accounting for false rejections and false inclusions, the poverty headcount and gap are higher than in the original variation at 20.1 per cent and 6.2 respectively. The total number of beneficiaries decreases from 21.5 to 16.0, reducing the size of the budget (Table 17, panel A). Counter-intuitively, perhaps, the FPL Efficiency score is only slightly lower when taking these errors into account. In fact, when only accounting for false rejections (if perhaps we think there is some self-selection and false inclusions are likely to be low), the resulting efficiency is actually *higher* than under the original scenario. We assume here that poor claimants are falsely excluded at the same rate as non-poor claimants, meaning that the proportion of spending allocated to the poor under the original and false rejection simulations are similar. Of course, the same does not apply to false inclusions, as those included are employed and therefore less likely to be poor.

Applying a means-test reduces a portion of the false inclusions - the employed, non-poor claimants. When we account for false rejections and inclusions in the variant of the SRD scenario which applies an income threshold at the FPL, the non-poor claimants who are actually employed are excluded by the means test (Table 17, panel B).

Table 19: SRD - errors of exclusion and inclusion

Scenario variation		FPL headc.	FPL gap	Total beneficiaries (millions)	Cost (R, billion)	Efficiency
A. SRD full take-up		17.3	4.5	21.5	90.3	28.1
i.	False rejections and inclusions	20.1	6.2	16.0	67.2	26.0
ii.	False rejections only	20.2	6.3	14.0	58.6	29.4
B. SRD with FPL threshold		18.0	4.6	16.8	70.6	35.1
i.	False rejections and inclusions	20.6	6.4	11.0	46.3	36.5
ii.	False rejections only	20.6	6.4	10.9	45.9	36.4

Note: efficiency is measured as the proportion of spending going to reducing the poverty gap for those below the FPL. The FPL headcount ratio is 25.3 per cent and the FPL poverty gap is 10.2 per cent.

Source: authors' calculations based on LCS 2014/15, QLFS 2015 Q1, QLFS 2020 Q1, QLFS 2021 Q1, and NIDS-CRAM.

The table above assumes that the only source of error is the employment verification using the IRP5 database. In reality, the means test itself (grant B above) may have substantial errors. There may be other sources of error, such as inaccurate recording of employment, as in the case of the UIF database (as suggested by the SASSA officials).

We can get calibrate these statistics by considering the actual rejection rates published by SASSA for the August 2021 SRD applications. Of about 13 million applications, SASSA records 64 per cent verified as eligible, and 29 per cent rejected as ineligible. Of the rejected, 48 per cent are rejected based on the UIF and 33 per cent based on the IRP5. Thus, even if we assume *all* of the

IRP5 rejections are false, this only suggests a 15 per cent false rejection rate (accounting for false inclusions implied by table 19). This is much lower than the 1-in-3 estimated purely based on historical data. This may be lower than the actual false rejection rate: as discussed above, a significant proportion of the UIF rejections may be false. By assuming all of the UIF and IRP5 rejections to be false, we arrive at an upper bound of a 30 per cent false rejection rate: close to our 1-in-3 estimate.

Finally, it remains unclear why there are so few valid claimants. While the survey data suggest 22 million claimants should qualify, only approximately 13 million applied and 9 million were verified. The value of the grant (R350) is a substantial proportion of income for those below the FPL or UBPL, and it therefore seems unlikely that this would be by choice, and more likely that individuals are experiencing barriers to access. For the SRD to be better implemented in future, these barriers would need to be identified and resolved.

C Public works recipient distribution

The number of jobs proposed under the public works scenario variations (1 or 2 million) is far less than the total number of unemployed. As a result, a method of selecting participants from the pool of eligible participants needs to be decided on.

Unfortunately, we could find no direct research on the household income distribution of the Community Works Programme or even Expanded Public Works Programme (EPWP) participants. However, the Labour Force Survey (LFS) of 2007 and the General Household Survey (GHS) of 2019 both record whether the respondent has participated in the EPWP. We impute household income in both surveys, subtracting out a predicted EPWP earnings at minimum wage for full time over a month. This could be an underestimate (most EPWP programmes pay more than the minimum) or an overestimate (many programmes do not provide full time work).

Below is the comparison of quintiles of household income per capita for EPWP workers (with EPWP wages subtracted from income) and the unemployed from both the LFS 2007 and the GHS 2019. The LFS records a lower number of EPWP workers since it captures those currently participating in the programme, and the GHS records a higher number of EPWP workers, since it includes anyone who participated in a programme in the previous 6 months. Unemployment is higher in the LFS than GHS.

While the surveys reflect very different years and questions, the table suggests a coherent picture. We interpret this as saying (a) the distribution of EPWP workers does seem to come more from the bottom quintile (about 30-40 per cent), but with substantial proportions from higher quintiles (26-36 per cent in quintiles 4 and 5), and (b) the distributions of EPWP workers and unemployed match well.

As a result, we choose to draw equally from the unemployed.

Table 20: Distribution by quintile: EPWP workers vs. unemployed

	Labour Force Survey		**General Household Survey**	
	EPWP	**Unem.**	**EPWP**	**Unem.**
Total population	**10 391**	**7 860 101**	**427 022**	**6 502 829**
Quintile 1	0.33	0.36	0.39	0.31
Quintile 2	0.17	0.23	0.22	0.24
Quintile 3	0.14	0.19	0.13	0.20
Quintile 4	0.13	0.15	0.12	0.16
Quintile 5	0.23	0.07	0.14	0.09

Source: authors' calculations based on 2007 LFS and 2019 GHS.

D Scalability

Table 21: scaled scenarios: poverty impact, efficiency

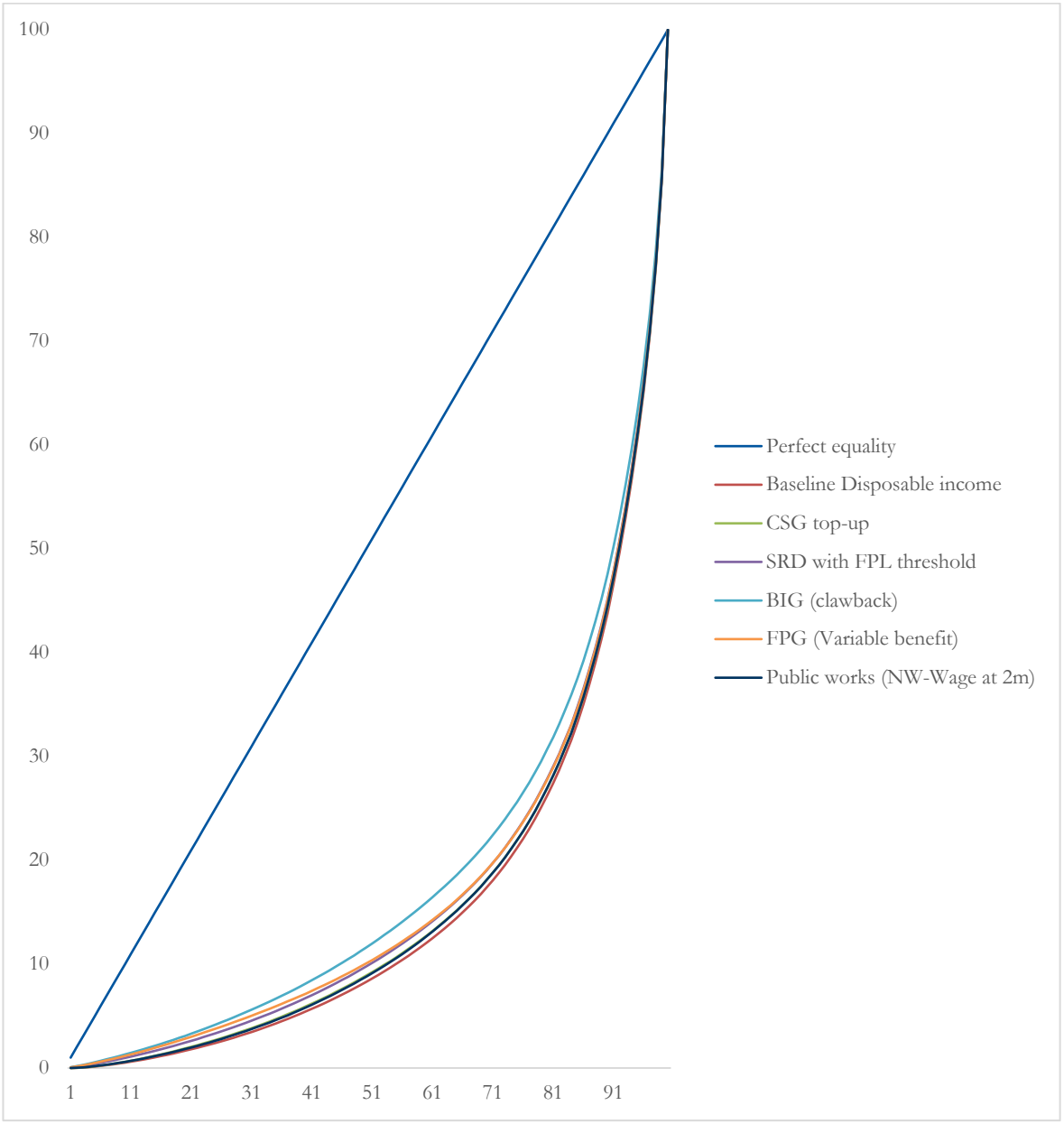
Scenarios	Average value to individual, per month		Total beneficiaries (millions)		Original budget						R45 billion budget					
	Orig.	R45b	Direct	Indir.	FPL Headc.	FPL Gap	FPL Eff.	UBPL Headc.	UBPL Gap	UBPL Eff.	FPL Headc.	FPL Gap	FPL Eff.	UBPL Headc.	UBPL Gap	UBPL Eff.
CSG top-up: increased grant size	623	729	7.0	30.0	22.0	8.3	29.5	51.0	24.2	71.4	19.8	7.4	26.9	50.0	22.9	70.1
CSG top-up: increased size & threshold	624	698	7.3	30.8	22.0	8.3	25.2	51.0	24.2	61.1	20.7	7.7	23.8	50.4	23.4	60.4
SRD Emergency	350	398	9.5	31.8	21.3	7.3	32.2	50.9	23.5	65.3	20.9	7.0	31.6	50.6	23.2	65.0
SRD Youth	350	283	13.4	38.8	20.3	6.4	30.3	50.1	22.6	62.5	21.1	7.0	31.2	50.6	23.3	62.9
SRD full take-up	350	176	21.5	48.7	17.3	4.5	28.2	48.7	20.6	60.1	21.2	7.0	31.0	50.6	23.3	61.5
SRD at FPL threshold	350	225	16.8	43.0	18.0	4.6	35.1	49.8	21.2	68.7	20.6	6.4	37.4	50.8	22.9	69.4
SRD at NMW threshold	350	190	19.9	46.7	17.3	4.5	30.4	48.9	20.6	64.6	20.9	6.8	33.2	50.6	23.1	65.9
SRD at NMW threshold (R624)	624	190	19.9	46.7	8.9	1.8	24.9	45.2	16.5	62.1	20.9	6.8	33.2	50.6	23.1	65.9
BIG at NMW threshold	624	151	25.0	50.6	7.7	1.4	20.8	42.6	15.0	57.1	21.2	7.2	29.6	50.4	23.3	62.7
BIG	624	111	34.2	57.6	7.6	1.4	15.2	41.8	14.9	42.4	22.2	7.9	22.2	50.7	24.0	47.4
BIG (clawback)	597	131	28.8	54.3	7.6	1.4	18.9	41.8	14.9	52.6	21.5	7.4	27.1	50.4	23.5	58.3
FPG two-step	1 413	758	5.0	20.0	-	-	53.6	51.2	18.0	92.9	16.9	3.3	67.4	51.8	21.8	94.1
FPG variable benefit	1 001	774	4.9	19.8	13.1	2.8	55.8	51.5	20.5	94.0	15.7	4.0	60.3	51.8	21.8	94.4
FPG top-up to FPL	1 221	758	5.0	20.0	0.1	0.0	62.1	51.5	19.1	92.9	18.0	2.7	73.8	51.8	21.8	93.9
Public works (EPWP-Wage at 1m)	795	3 782	1.0	4.6	24.6	9.6	26.4	52.1	25.7	56.0	23.1	9.1	10.5	50.1	24.4	39.5
Public works (EPWP-Wage at 2m)	795	1 897	2.0	8.5	23.9	9.1	24.4	51.8	25.2	54.0	22.1	8.6	15.9	50.4	24.0	47.6
Public works (NW-Wage at 1m)	1 446	3 782	1.0	4.6	23.9	9.3	21.3	51.7	25.3	52.8	23.1	9.1	10.5	50.1	24.4	39.5
Public works (NW-Wage at 2m)	1 446	1 897	2.0	8.5	22.6	8.7	18.9	51.0	24.4	50.5	22.1	8.6	15.9	50.4	24.0	47.6

Note: [1] CSG average monthly value of grant is value of top-up amount received by caretaker. [2] When we constrain the budget, to be the same, only the number of participants makes a difference for the public works programmes, not the size of the original wage. As a result, the public works programmes with 2 million participants now have the smaller wage of R1 897 per individual, and the programmes with 1 million participants have a larger wage of R3 782 per individual. [3] The scaled-FPG two step is more efficient than the scaled-FPG variable benefit. [4] The SRD at the NMW threshold - whether the original grant size was R350 or R624 - now collapses into the same variant with a grant size of R190.

Source: authors' calculations based on LCS 2014/15, QLFS 2020 Q1, and QLFS 2021 Q1.

E Lorenz curves

Figure 18: Lorenz curves



Source: authors' calculations based on LCS 2014/15, QLFS 2020 Q1, and QLFS 2021 Q1.