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Methods for Credible Evaluation of Programme Stimulus Effects in South Africa

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Abstract
In response to the effects of the COVID-19 pandemic, in 2020-21 the South African government instituted various economic relief and stimulus programmes. These included substantial increases to social grant disbursements, and a large new targeted jobs programme. Potential “spillover” or “multiplier” effects of these programmes are an important part of prospective policy evaluation, and are of particular interest during a period of fiscal constraints. This review considers approaches for quantitatively evaluating stimulus effects of government programmes, with particular reference to the South African pandemic response. After discussing the general approach of random and quasi-random programme evaluation methods, we review the existing international literature evaluating stimulus effects of jobs programmes and cash transfers. We highlight key lessons in terms of methods, data requirements, and the necessity of rigorous local evaluations. We also describe the South African programmes, and discuss local data sources. We suggest that the social grant top-ups and jobs programme present an exciting opportunity to credibly measure programme stimulus effects in South Africa.

Keywords: Public employment; spillover effects; programme evaluation; South Africa

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Résumé

En réponse aux effets de la pandémie de COVID-19, le gouvernement sud-africain a mis en place divers programmes d'aide économique et de relance en 2020-2021. Il s'agissait notamment d'une augmentation substantielle des versements de subventions sociales et d'un vaste nouveau programme d'emploi ciblé. Les retombées potentielles de ces programmes constituent un élément important de l'évaluation prospective des politiques et présentent un intérêt particulier en période de restrictions budgétaires. Cet examen examine les approches permettant d'évaluer quantitativement les effets de stimulation des programmes gouvernementaux, en particulier la réponse à la pandémie en Afrique du Sud. Après avoir examiné l'approche générale des méthodes d'évaluation aléatoire et quasi-aléatoire des programmes, nous passons en revue la littérature internationale existante évaluant les effets de stimulation des programmes d'emploi et des transferts monétaires. Nous soulignons les leçons clés en termes de méthodes, d'exigences en matière de données et de la nécessité d'évaluations locales rigoureuses. Nous décrivons également les programmes sud-africains et discutons des sources de données locales. Nous suggérons que le programme de bonification des subventions sociales et de création d'emplois offre une occasion intéressante de mesurer de façon crédible les effets de stimulation du programme en Afrique du Sud.

Mots-clés: Emploi public; externalités ; evaluation des programmes; Afrique du Sud

1. Introduction

In response to the severely negative economic and social costs of the COVID-19 pandemic and mandated lockdowns (Jain et al. 2020), in 2020 the South African government put in place a number of programmes designed to alleviate the shock. Key amongst these were 1) the mass disbursements of additional social grant funding – new grants and increases to existing grants – to the value of over R50 billion to 18-24 million people over May 2020 to April 2021[1] and 2) targeted jobs programs creating over 400,000 temporary jobs to the value of R13 billion. With the multiplier effects of these social programmes being of particular interest in a period of fiscal constraints, these disbursements present a unique opportunity to evaluate the stimulus effects on local economies of large income injections. In this paper, we selectively review existing economics literature on empirically identifying the effects of economic stimulus, and suggest approaches for evaluating the stimulus effects of grant and jobs programme over the pandemic. This review is intended to be useful for both policymakers and researchers.

By stimulus effects, we mean economic effects over and above the immediate effects on the direct recipients of programmes. That is, while it is usually safe to assume large benefits to recipients of social grants or participants in jobs programmes, and even to their households, we are interested here in the economic effects on *non-recipients*.[2] We frequently refer to this kind of effect as a "spillover", as the programme goes on to affect those other than the directly targeted. The classic mechanism we have in mind is a *local demand multiplier*: 1) the recipients of programme income (whether a social grant or public job) use the income to buy more goods and services from their local businesses; 2) these business owners use the additional income either to increase their own consumption expenditure, or to expand and hire additional workers who can now spend more themselves; 3) this additional expenditure further stimulates the local economy via the same channels; and so on. The extent to which local demand multipliers are expected to lead to sizable spillovers depends on a number of context-specific factors, including the proportion of income which is spent rather than saved, whether firms increase profits by increasing prices rather than output, and how easy it is for firms to hire additional workers at the prevailing wage. It is therefore important to empirically *measure* stimulus effects in particular contexts.

Whether stimulus or spillover effects occur via the local demand multiplier or other mechanisms, the "multiplier" is a standard measure of the size of these effects regardless of the mechanism. It can be defined as the *change in total real income divided by the total additional income injected* for a group of people. In this review, we focus on groups of people in local economies, such as at the municipal level, for example. A multiplier greater than 1 means that incomes in a local area rose by more than the income injected into the area (in this case by government). A multiplier can also

[1] These are approximate numbers; for example the totals depend on whether CSG caregivers or children are counted as recipients. See Section 4.

[2] There is an existing South African literature evaluating these direct effects on recipients, which we briefly reference in Sections 3.1.4 and 3.2.2, though it is not a focus of this paper. Credible evaluations of these direct effects will usually assist when it comes to evaluating effects on non-recipients. The second-order effects of these direct effects can also be important: for example jobs-programs are expected to help recipients find jobs in the private sector by providing work experience, while South African old age pensions may facilitate job search for co-resident prime-aged adults. This latter mechanism is discussed in Section 3.2.2.

be less than 1, for example if businesses raise prices in response to higher demand for goods, which could actually *decrease* the real income of those in the area.

The stimulus effect or multiplier is potentially a crucial part of any programme evaluation. An example which we review later is the evaluation of India's jobs programme by Muralidharan et al. (2017), which finds that 90% of the resulting increase in income for local residents came from increased *private sector* income in response to the programme, rather than the jobs programme payments themselves. This extraordinary case highlights the potential for stimulus to radically change the cost-benefit analysis of a government programme. At the same time, we will also show that effects of cash transfers and jobs programmes clearly depend on implementation details and local contexts. In some cases, these programmes may have led to increased prices and goods rationing (Filmer et al. 2018). In developing knowledge about stimulus and spillover effects in South Africa, there is no substitute for high-quality evaluation in our local context. This review aims to facilitate these kinds of studies, providing a launching board for research towards credible estimates of local multipliers.

In our discussion of evaluation methods we focus exclusively on quantitative evaluation, and almost exclusively on experimental and quasi-experimental programme evaluation methods. This should not be taken to imply that these are the only credible methods for evaluating the stimulus effects of these job and grant programmes.[3] Rather, these are approaches which we believe have particular strengths when it comes to credible programme evaluation, and which should be part of a package of methods used to evaluate local stimulus effects. We therefore seek to explain the appeal (and some weaknesses) of these methods, and present considerations for feasible and credible evaluations of this type in South Africa. It is our hope that consideration of other methods for evaluating stimulus effects (by those better-versed in those methods than ourselves) will take place in due course.

This review proceeds as follows. The next section outlines the logic of random and quasi-random quantitative programme evaluation methods, and is aimed at readers who are not familiar with these approaches. Section 3 is the mainstay of this paper, and reviews the stimulus effects literature. We focus on sub-literatures on jobs programmes and social grant transfers, and then briefly review macroeconomic approaches to estimating multipliers. Two workshops were held in the lead up to this review, and we integrate insights from the speakers throughout this section. Section 4 provides institutional details on the presidential employment stimulus jobs programme, as well as the additional social grant support provided during the first year of the pandemic. Section 5 provides a short discussion of the types of data that may be useful in an evaluation of local multipliers associated with these interventions, drawing from the existing literature. Section 6 concludes.

[3] See Muller (2020) for a critique of a special privileging of random and quasi-random methods for policy development, with a particular focus on Randomised Control Trials.

2. Approaches to quantitative programme evaluation

2.1. Random and quasi-random treatment assignment

Consider a quantitative evaluation of the effects of South Africa's state Old Age Pension. One could think of many outcomes of interest which are appropriate for the point we make here – health, labour supply, effects on household members, etc – but for our purposes consider an evaluation of the effects the pension has on poverty rates of recipients. A naive approach using survey data would be to simply compare the poverty rate amongst pension recipients against the poverty rate of non-recipients. Results for this kind of comparison are shown in Table 1, and they show that poverty rates are *higher* among Old Age Pension recipients than non-recipients. However interpreting the results from this analysis as the *causal effects* of the pension would be wrong. The South African pension incorporates a means test, and is only available to people who are least 60 years old. So pension recipients are typically older and poorer than non-recipients (amongst other differences, see below), and comparing outcomes between these groups reflects these pre-existing differences as well as the effects of the pension. This is why such an analysis wrongly suggests that the pension *increases* poverty among its recipients.

Table 1: Poverty rates by household OAP receipt (black Africans)

	OAP	Non-OAP
Food poverty rate (H0)	17.76%	16.23%
Upper bound poverty gap (H1)	23.98%	18.35%

Notes: Table shows poverty rates of black Africans depending on whether an Old Age Pension (OAP) is received in their household in 2017, using NIDS Wave 5. H0 refers to the headcount ratio while H1 refers to the poverty gap. Inflation-adjusted poverty lines ("food" and "upper bound") are defined per Statistics South Africa (2019). Calculations use the NIDS post-stratified weight. This table is intended to be illustrative of a method and not to be used for substantive conclusions about the poverty effects of the Old Age Pension.

The key problem here is that the comparison group (non-recipients) is *systematically different* from the recipients, so differences in outcomes reflect things other than the effect of the pension. To perform a programme evaluation one wants to find or construct a comparison group (the "untreated" or "control" group) which is similar to the policy recipients (the "treated group") *except* for the fact that the treated group receives the policy intervention (the "treatment"). In this case, differences in outcomes between the treated and control group reflect the *causal effect* of the treatment (the policy). In empirical microeconomics, this typically means "treatment" is either *randomly* or *quasi-randomly* assigned.

Random assignment normally occurs as a part of a "randomised control trial" (RCT), where researchers randomly choose which individuals or groups get the policy intervention and which do not. The effect of the policy (the "treatment effect") is then quite simple to identify: one just compares outcomes of the treated group against the outcomes of the control group. In the example above, this would mean randomly choosing people to receive grants, and then comparing poverty rates amongst grant recipients against those of non-recipients. Differences between these groups are now only due to the causal effect of the grants, as random allocation means there are no other systematic differences between the groups.

However for many kinds of policy evaluations a randomised control trial is either impractical or unethical.[4] In these cases we can sometimes perform an evaluation based on *quasi-random* assignment. In these settings, called "natural experiments" (contrast with *researcher-created* experiments as above), we rely on quirks in the ways actually-existing policies are designed or implemented, which allow us to compare outcomes for two groups which are very similar, but where one group gets treated and other other does not. As noted above, one cannot identify the effects of the pension by simply comparing pension-recipients to non-recipients, because these groups are different in many ways. However because people only become eligible for the pension when they turn 60, it might be possible to identify the effect of the pension by comparing outcomes of 59-year-olds against those of 60-year-olds. Figure 1 presents such a comparison, showing poverty rates by age – and that there is a clear discontinuous drop in poverty rates between ages 59 and 60.[5] The idea is that with such a small gap in age the two groups are essentially the same, *except* for the fact that 60-year-olds receive the pension. If this is true (and it might not be), treatment is then *as-good-as-random* (or *quasi-random*) between the two groups, and differences in the outcomes of the two groups identify the effect of the policy. In the case of the Old Age Pension, this approach allows us to see its poverty-reducing effect. Other examples of policy "quirks" which can cause natural experiments are implementation delays (so that some people receive the policy early while similar people receive it late) or geographic variation along administrative boundaries (so that some people receive the policy while their similar neighbours do not). Different types of natural experiments are discussed throughout Section 3.

Randomisation or quasi-randomisation is often crucial for credible quantitative policy evaluation because it is *usually* the case that the treated group is different from the control group in important ways. Governments do not usually choose policy recipients randomly, but do some kind of targeting, or recipients themselves choose to participate based on their needs and personal circumstances.[6] For example: social grants are targeted to the vulnerable, public employment programs are targeted to the unemployed, and government stimulus is targeted to poorer regions. In all of these cases, those who are directly affected by the policy are quite different from those excluded, and we cannot determine the effect of the policy by aggregate comparison. Programme evaluation methods which use random or quasi-random variation have come to increasingly dominate empirical microeconomics in the last 30 years (Angrist and Pischke 2010), and it is predominantly these methods which we discuss in this review.[7]

2.2. Modeling versus random & quasi-random variation

[4]The ethical issues inherent in RCTs are complex and perhaps have received insufficient attention as the method has become widespread in Economics. In our view these issues need to be taken seriously when considering whether an RCT would be an appropriate programme evaluation method. For discussion of some of the ethical issues, see for example: Deaton (2020), Hoffmann (2020) and Evans (2021)

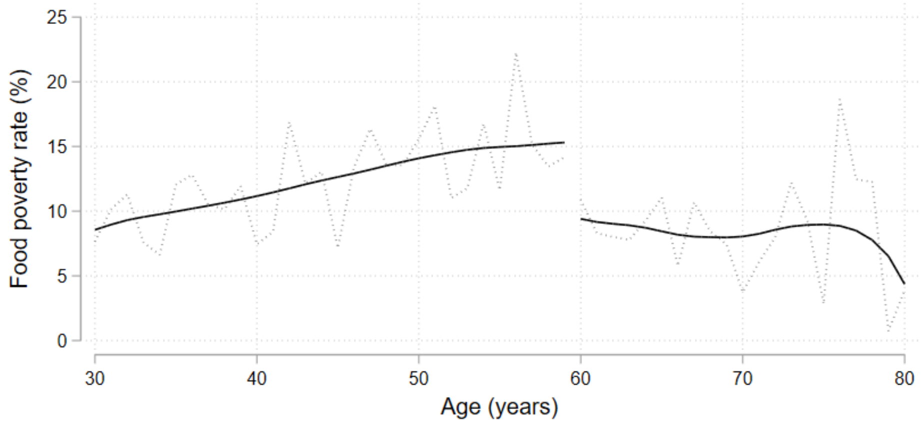
[5]Our Figure 1 very closely follows the approach used by Ranchhod (2006) to evaluate the labour supply effects of the Old Age Pension. Our method here is however a very simplified version of his analysis, and is purely for illustrative purposes. Readers should not use Figure 1 to make conclusions on the actual poverty effects of the Old Age Pension – doing so would require a much fuller analysis and discussion.

[6]Governments may implement *universal programs*, which present their own policy evaluation challenges because of the difficulty of finding a control group.

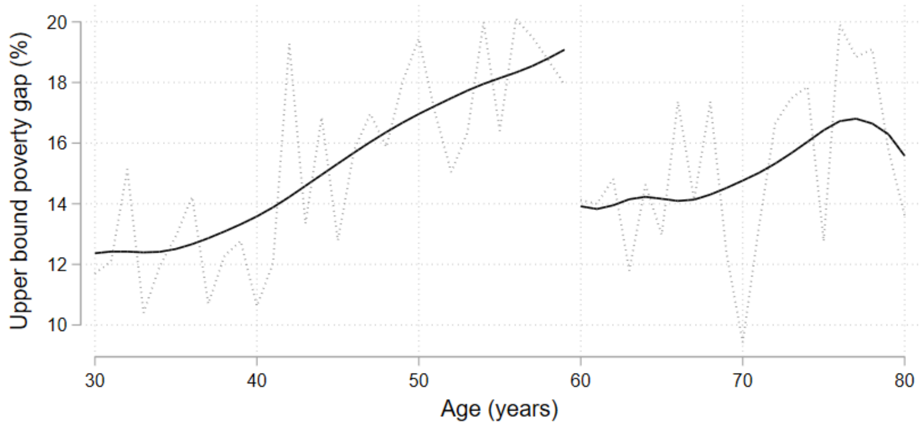
[7]Angrist and Pischke (2010) have, not uncontroversially, called this the "credibility revolution" in empirical Economics.

Figure 1: Black African poverty rates by age; illustrating discontinuity at 60

(a) Headcount ratio at food poverty line



(b) Poverty gap at upper-bound poverty line



Notes: Figures show black African poverty rates by age in 2017, using NIDS Wave 5. Actual poverty rates per age are shown using the dashed line, while LOWESS is used for the solid smoothed lines on either side of the age threshold of 60, with the Old Age Pension eligibility threshold. Inflation-adjusted poverty lines are defined per Statistics South Africa (2019). Calculations use the NIDS post-stratified weight. This figure is intended to be illustrative of a method and not to be used for substantive conclusions about the poverty effects of the Old Age Pension.

One alternative approach is to try to statistically *control* or economically *model* differences in the treatment or control groups. For example, in the case of the state Old Age Pension, we noted that pension recipients are typically older and poorer than non-recipients. We often observe individuals' ages and incomes, so could hold these factors constant in a regression. But it's likely that this wouldn't be sufficient: compared to non-recipients, pension-recipients will likely have different education, assets, health, family structure, geographic location and marketable skills, amongst other things which might have a bearing on the outcome we're interested in. We can control for some of these factors to the extent that we observe them, but we are very unlikely to observe all of these factors, and often it is quite easy to think of fundamentally unobservable factors which can affect outcomes (such as marketable skills). Controlling for observables or imposing strong economic assumptions on the unobservables (economic modelling) is one way to perform programme evaluation, and it has the benefit that the researcher does not have to perform an RCT or find policy "quirks" which allow a natural experiment. These modeling approaches often allow researchers to ask more wide-ranging questions, but may come at the cost of a less credible answer, which can be sensitive to the various economic assumptions imposed.

It is important to note, however, that while random and quasi-random methods can provide highly credible answers about the *particular programme under review* ("internal validity"), answering the question of how well these results can be transported to other contexts ("external validity") requires further assumptions (Muller 2015; Deaton and Cartwright 2018). This is often taken to be a weakness of RCT methods in particular, but in fact applies to all kinds of empirical programme evaluations, including quasi-random "natural experiments", purely descriptive methods, and qualitative approaches (Deaton 2020). These issues may however be particularly important in RCTs, which are usually evaluations of small-scale, geographically-bound interventions, rather than evaluations of national programmes. For example, results on cash grants in rural Siaya County in Kenya may be different in other parts of Kenya, let alone South Africa.[8] This Kenyan study would however not be made *invalid* by this issue: it can tell us about something important in the setting studied, and can be informative about the plausibility of theoretical mechanisms. But we must still be careful when applying its lessons to other contexts. Per Deaton (2020, p. 10): "External validity is about how a study is used."

2.3. Evaluating spillover effects

The same issues discussed above apply when specifically interested in spillover effects, as we are in this review. However spillover effects, defined as effects on those who are not *direct* recipients of the policy, do present additional complications.

Firstly, the same issues about needing (quasi-)random variation apply. If evaluating public employment scheme spillovers on the privately-employed, for example, one cannot simply compare outcomes for all privately employed people in areas which have public employment against outcomes of all privately employed people in areas which do not. Even if the programs are not targeted to the privately-employed, we can expect privately employed people in areas with public employment to be systematically different from those in the unaf-

[8] On the other hand, questions about external validity also can apply to the so-called "local average treatment effects" identified in natural experiments. Does the effect of South African state pensions on 59 year-olds and 60 year-olds discussed above also tell us about the effect pensions have on outcomes for 70 year-olds? Or how pensions would affect 40 year-olds?

fected regions: the former live in areas which are likely to have a higher unemployment rate, may have fewer employment prospects, and may have different kinds of jobs, education, healthcare provision etc. Again, we need to find a *comparable* control group. One method is random assignment: randomly provide public employment in some districts and not others, to try make sure that the districts are not systematically different, and then just compare private employment across the districts. However, apart from the ethical issues noted above, there can also be complex technical issues associated with these RCT approaches, such as the size of evaluation "units" (e.g. district council, local municipality, village), the number of units, the density of treated individuals within these units, and the stimulus size, which all affect the researcher's ability to credibly identify spillover effects. An alternative to random assignment is to look for natural experiments, where the credibility of such studies hinge on the plausibility that assignment truly is sufficiently "naturally" random, i.e. unrelated to any potential confounders. Analogous technical requirements need to be satisfied in this case too, even when a natural experiment has been identified.

Secondly, we should note that multiplier and spillover effects can be *particularly* difficult to evaluate with random and quasi-random methods. These methods rely on comparisons between distinct "treatment" and "control" groups, with the controls giving us an idea of what outcomes would have been for the treated group if they hadn't actually received the treatment. In order for this to work, the "control" group cannot themselves be affected by the policy, directly or indirectly – they need to represent the outcomes in a counterfactual world where the policy wasn't implemented. But in our case, the spillover effects we are interested in are precisely the effects of the policy on those who are *not* directly affected by the policy, but who are *indirectly* affected due to interactions with the directly treated. We *expect* some people who are not direct policy recipients to be affected by the policy, so we cannot automatically use all non-recipients as the control group. Instead, we need to take care to separately identify a subset of non-recipients who may receive indirect effects of the policy, and a separate "truly-untreated" group who are not directly nor indirectly affected, who can then be used as the control group

To give an example: consider a cash transfer received by individuals in District A and not received by individuals in District B.[9] Districts A and B are otherwise identical, and they border each other. If we think there may be spillover effects of the cash transfer on neighbouring districts – perhaps individuals from District A buy goods from District B, so the cash grant increases expenditure and incomes in District B – then District B is not a valid control group, as they are also affected by the policy (indirectly). In order to evaluate the effect of the policy in District A, we need *another* comparable non-treated group which is plausibly *not affected* by the policy directly nor indirectly, perhaps a District C which is sufficiently geographically distant from A that we do not expect them to experience spillover effects. Crucially, we also need a District C to evaluate the size of the spillover effects in B, which is the main object of interest in this review. If we want to evaluate the size of the spillovers, we need a control group not affected by the spillovers. In practice, the boundaries (geographic or otherwise) of who is likely to be affected by spillovers can be subject to debate. Credible research will show that results are not overly affected by reasonable choices in this regard.

[9] For simplicity in this example, assume that all individuals in District A receive the cash transfer. However if some didn't, they would be part of the "spillover" group with individuals in District B (see below).

2.4. Does this matter in practice? A case study of US minimum wages

It is of course legitimate to wonder whether these methods really make a practical difference when it comes to programme evaluations. A leading example of the value of these methods is research on the effects of the US minimum wage. The necessary context is that while there is a US national minimum wage, it is only rarely adjusted, and most minimum wage changes are at the state-level. One approach to evaluating the employments effects of minimum wage increases is to compare the evolution of employment in all states which have minimum wage changes against all states which do not. This approach, which uses all cross-state minimum wage variation, has tended to find negative employment effects (Neumark and Wascher 1992, 2007).

However starting with Card and Krueger (1994), the current minimum wage literature has tended to take a "natural experiments" approach, where the researcher carefully chooses a control group which is expected to be appropriate for the treated group which receives a minimum wage increase. Card and Krueger (1994) compared employment at restaurants close to the New Jersey-Pennsylvania border before and after New Jersey raised their minimum wage. The idea is that because of their geographic proximity, these restaurants are similar *except* for the New Jersey restaurants having a higher minimum wage. Card and Krueger (1994) find that the minimum wage did not decrease employment and in fact may have *increased* it. Dube et al. (2010) extend the Card and Krueger (1994) natural experiment approach by using a *border discontinuity design*. Essentially, they combine many Card and Krueger (1994) natural experiments by looking at state-level minimum wage changes *across the US* (not just New Jersey versus Pennyslvania), but they only compare employment between counties (small sub-state regions) which border each other but are in different states.[10] Like Card and Krueger (1994), Dube et al. (2010) find no adverse employment effects.

The problem with using all cross-state minimum wage variation as in Neumark and Wascher (1992, 2007) is that the treatment and control states may not actually be comparable. This is exactly what Dube et al. (2010) argue, saying that employment trends in minimum-wage-increasing and constant-minimum-wage states were fundamentally different in the period under consideration by Neumark and Wascher (1992, 2007). Dube et al. (2010) get around this by restricting their attention to neighbouring counties, where labour markets are arguably sufficiently similar, allowing them to isolate minimum wage effects. This debate was not settled by Dube et al. (2010), and the US minimum wage is still a highly active research area. However high-quality evidence (e.g. Cengiz et al. 2019) has tended to confirm that if there are negative employment effects of the minimum wage, they are small. This has represented a sea-change in how US economists understand the minimum wage, against both theoretical and early empirical expectations. This shift was only possible via the "natural experiments" approach we have outlined above.

[10] See Section 3.3.2 for more information on this type of design.

3. Literature Review

3.1. Jobs stimulus

Public works or jobs programmes aimed at providing low-income households with a safety net are pervasive throughout the world.[11] Governments typically provide jobs that pay low wages to otherwise unemployed workers, with the aims of labour-intensive community asset creation, providing a permanent buffer for low income households, or tiding over periods of crisis. In this section, we begin by reviewing the mechanisms through which jobs programmes alleviate poverty, and then we discuss evaluations of jobs programmes with a focus on India (which has the world's largest programme) and South Africa.

3.1.1. Mechanisms

The motivations for jobs programmes include individual-level outcomes such as providing a basic income, mitigating temporary shocks, and acting as a bridge towards permanent employment. At the community level, which is relevant for evaluation of stimulus effects, jobs programmes aim to provide an income floor through employment opportunities, to create public assets, and to stimulate spending in the local economy (through a local demand multiplier effect, as discussed in Section 1).

The potential for jobs programmes to provide an income floor for the local labour market is explained in Basu et al. (2009). The wage level of these jobs form part of a worker's outside option; that is, there is no reason for a worker to accept a wage in the private sector below the wage paid in a jobs programme. Of course, this depends on other factors too, such as the relative quality and accessibility of the jobs, but the net effect is to raise the private sector wage. We would expect these effects to be strongest in *guaranteed* employment programmes, where the worker always has the outside option of public employment. However even when employment scheme jobs are rationed and workers only have some *probability* of being selected for the public job, the chance of getting a public job still improves their outside option to some extent. Of course if the public employment scheme is very small relative to the scale of unemployment, we may not expect any appreciable effect. If these public employment schemes are relevant in a particular setting, they should be particularly important for the informal sector, as an alternative to minimum wage regulation which cannot be enforced.

An important concern is the effects on employment. An effective minimum wage may crowd out private employment, by destroying jobs that would have paid lower wages. Basu et al. (2009) highlight that this depends crucially on the competitiveness of the labour market. Under monopsonistic competition, where employers have the ability to set wages, employers need to *increase* the wage to attract more workers, meaning that the cost of employing another worker is equal to that worker's wage *plus* the increase in wage for existing workers. For example, an employer may be paying such low wages that workers

[11] Subbarao et al. (2012) list programmes in Argentina, Bangladesh, Cambodia, Djibouti, Ethiopia, India, Liberia, Nepal, Sierra Leone, Rwanda, and Uganda, amongst others. Beegle et al. (2017) note that 39 out of 48 countries in Sub-Saharan Africa have public works programmes.

living further away would actually lose money on net through transport costs, or worker turnover may be high at that low wage as workers constantly switch to better-paying jobs. Monopsonistic employers employ fewer workers than in competitive markets. However, an income floor for monopsonistic employers would make the cost of employing another worker equal to the wage only (since all workers are paid the floor, no wage increase is required), counter-intuitively making it *less* costly to employ another worker, and thereby incentivising monopsonistic employers to *increase* employment. This effect has limits: when the income floor is too high (relative to productivity), then private employment will be crowded out.

In summary, the stimulus effects of a jobs programme may operate through raising the income floor, which would increase the general level of wages and has unclear effects on employment, through local spending multiplier effects (as discussed in the introduction), or through raising productivity through public asset creation.

3.1.2. Indian studies

We begin with evaluations of India's National Rural Employment Guarantee Scheme (NREGS). Besides being the largest public employment programme in the world, the major studies in the literature on jobs programmes focus on NREGS (Imbert and Papp 2015). NREGS was phased in between 2006 and 2008, where 600 million rural residents became eligible for 100 days of guaranteed employment involving labour-intensive public infrastructure projects typically in the off-season of agricultural work. Annually, 0.5% of India's GDP is allocated to the programme which provides work for over 50 million rural households.

Perhaps the most rigorous study to date is Muralidharan et al. (2017), one of the authors of which (Karthik Muralidharan) we hosted at our second workshop on the presidential stimulus. Working with the state government of Andhra Pradesh, they randomized the order of smart-card rollouts across 157 districts (63,000 people each) as an at-scale RCT. Compared to the existing system of payment via government post offices after paper-based authentication, the bank-partnered biometric smartcard system substantially improved NREGS in many ways, such as increasing program earnings and access to work, and reducing leakage and payment delays (Muralidharan et al. 2016). While this study only evaluates a *reform* to NREGS, Muralidharan et al. (2017) argue that the smartcard reform brought NREGS closer to what the NREGS designers initially intended, as evidenced by the improved outcomes above.

They find credible spillover effects in two ways. Firstly, they find a large increase in income for participants (13%), but crucially only 10% of this increase comes from NREGS income. 80% of the remainder comes from increases in earnings from the private sector, which is a stimulus or spillover effect rather than a direct programme outcome. Secondly, they compare average private sector wages in *treated* districts, *untreated* districts with many *treated* neighbours, and *untreated* districts with no treated neighbours. Relative to the last group (which is plausibly unaffected by the reform), private sector wages rose by 6.5% in treated districts and by 4.1% in untreated districts with many treated neighbours. Several other studies of NREGS corroborate the income floor effect on private sector wages, including Imbert and Papp (2015), Azam (2011), and Berg et al. (2012).

Most intriguingly, Muralidharan et al. (2017) find *employment* increased by 20% in the private sector relative to the control (untreated districts with no treated neighbours). This employment finding is unusual in the literature: Imbert and Papp (2015) find a *contraction* in private sector employment, and Zimmermann (2020) finds no significant effect either way. Muralidharan et al. (2017) defend the credibility of their unusual findings in three ways. Firstly, they provide evidence consistent with the mechanisms highlighted in Basu et al. (2009): areas with more monopsony power (as measured by land-holding concentration) recorded the largest employment increases. Secondly, they note that the negative employment effect found by Imbert and Papp (2015) includes self-employment, whereas they focus on wage employment; a decrease in low-productivity self-employment may be *desirable* if coupled with more extensive wage employment. Thirdly, they note that the studies providing conflicting evidence rely on the phased rollout of NREGS, when implementation was weak and may have had different effects. Indeed, Muralidharan et al. (2016) show large gains from more effective payment systems for NREGS, including an earnings increase of 24% at very little cost.

In sum, the evidence from India's NREGS suggests that private sector wages (including the informal sector) were raised substantially. There are strong indications that employment *increased*, due to monopsony power, though there are conflicting results in the literature. However, as Karthik Muralidharan noted in the workshop in response to a question about whether such positive employment effects would be likely in the South African case, such effects depend on the local labor market dynamics and there is no substitute for direct evaluation.

3.1.3. Other international studies

Another study provides evidence of potential negative spillover effects: Beegle et al. (2017), who study Malawi's public works programme. In response to an over-subscribed programme, jobs were randomly allocated across and within villages – allowing similar comparisons to those explained in Muralidharan et al. (2017). While food security of households who did participate in the programme did *not* change, the authors record a *decrease* in the food security for nearby households who did not participate but who reside in the same village.[12] It is difficult to know what to make of this, and the authors present evidence that is *inconsistent* with a number of possibilities including labour market tightening (as discussed above), low take-up, statistical power or price inflation. The likely candidates are that these outcomes reflect *relative* food insecurity, since the measures are subjective, or perhaps that treated households bought up limited stocks of food, resulting in shortages for other households. Overall, the study serves as a cautionary note on possible negative effects, as well as the importance of giving detailed attention to spillover effects in experimental design – which requires some understanding of local labour and retail markets.

Berhane et al. (2014) evaluate Ethiopia's Productive Safety Net Programme (PSNP), which was introduced in 2005 in response to chronic droughts. PSNP reached more than 7 million participants in 2015 (7% of Ethiopia's population), making it the world's second largest public works programme, and it has an annual budget of about $500 million (about R7 billion). The

[12] As explained in section 2.3, the comparison group here is households in a completely different village with no treated households, i.e. "District C".

payments are small at less than $1 per day of work. Matching participants who have participated for many years with participants who participated for only one year, the authors find a modest increase in food security for participants as well as productive assets. An earlier study by Gilligan et al. (2009) also found increases in food security, *provided participants actually received payment* (there was no aggregate effect without making this restriction). They found no evidence of faster asset growth, but this may be due to a short period of evaluation at the early stages of the programme. In general, there is limited evidence on the stimulus effects of community asset creation.[13]

3.1.4. South African studies

South Africa's Expanded Public Works Programme (EPWP) began in 2004. Its most recent third phase from 2015 to 2019 had a target of 5 million job opportunities, having reached its previous target of 4.5 million from 2009 to 2014. The Community Works Programme (CWP) was modelled on India's NREGS and was intended as a more labor intensive programme, typically providing 2 days of work per week up to 100 days and with a minimum labour to total cost ratio of 65%. In 2013, CWP reach 204,000 participants.

In general, there is limited evidence on the spillover or stimulus effects of jobs programmes in South Africa. McCord (2004) studies two predecessors to these public works programmes: Gundo Lashu in Limpopo and Zibambele in KwaZulu-Natal, both initiated in 2000. As a useful local example of data collection with similarities to Muralidharan et al. (2017), she randomly sampled 6,000 programme participants for interviews, and complemented this with a qualitative study through participant focus groups. However, as McCord notes, the study lacks a baseline survey, and her design is forced to rely on a constructed control group of rural residents from the 2003 Labour Force Survey. In terms of direct effects on participants, she finds improvements in income and food security at the time of employment, but only one third said these improvements were long lasting. A finding suggestive of spillovers is that two thirds of participants purchased food locally, although the focus groups indicated the local business expansions induced by this higher demand disappeared soon after. These results highlight that stimulus effects may quickly dissipate.

A common problem with existing studies of South African jobs programmes is that they rely on participant outcomes without a comparison group, or on simulations, meaning that the causal effects of the programme remain unclear. One example is an EPWP presentation in 2017 (EPWP 2017) which reports that 47% of the participants in the programme in 2011 indicated that their financial situation had improved after the programme. Using the Quarterly Labour Force Survey for 2015, they also report 65% of participants in EPWP over the previous 12 months had found employment of some kind. For both these sets of results, the effect of EPWP on participants is unclear without knowing outcomes for a credible comparison group.

An example of a simulation is Stanwix and Van der Westhuizen (2012), who use household survey data for 2007 to simulate the impact of expanding the CWP. They assign CWP jobs in proportion to the unemployment rate in each district, and simulate outcomes with varying numbers of jobs and days worked. They find potentially large effects on poverty, though

[13] Ranaware et al. (2015) and Aggarwal et al. (2012) report positive effects of public works from India's NREGS, though their methodologies focus on successful cases.

little impact on inequality. McCord and Van Seventer (2004) also simulate programme effects, relying on industry linkages to predict an output multiplier of 1.2 for labour intensive programmes. Almost all of the income effects for the lower quintile households, however, are driven through direct employment, implying smaller spillovers for low income local economies driven through industry linkages. The advantage of such simulations is that they can trace out mechanisms and the associated magnitudes. The disadvantage of such simulations, however, is that they do not account for anything outside that which is modelled, for example other mechanisms of spillovers or programme implementation.

Overall, the need for well-designed evidence regarding previous jobs programmes in South Africa, together with the promising results from studies in other countries reviewed above, both highlight the high returns to a rigorous evaluation of the presidential employment stimulus.

3.2. Grants stimulus

Cash transfers are an important form of income support in countries throughout the developing world. In South Africa, roughly the poorest 40% of households get the majority of their income from such transfers in the form of government social grants (Leibbrandt et al. 2012). There is a *very* large literature evaluating various effects of cash grants, both locally and internationally, and we focus on a few types of studies. In general, we restrict our attention to studies which use the random or quasi-random assignment methods discussed in Section 2. In the international literature, we only review studies which evaluate the multiplier and spillover effects of cash transfers outside the recipient household. We are not aware of any such studies in South Africa, so when reviewing the South African case we (very briefly) discuss the literature on labour supply effects of Old Age Pensions on prime-aged co-residents.

3.2.1. International literature

Likely the most comprehensive analysis of cash transfer multiplier effects in the international literature is Egger et al. (2019). The authors randomly allocate a one-time (donor-funded) transfer of USD $1000 to over 10500 households across 653 villages in Siaya County, a rural area in Western Kenya. This is a very sizable transfer, constituting about 15% of local annual GDP in the treated areas during the peak 12 months of the programme, and corresponding to 75% of mean annual household expenditure in recipient households.[14] Not only do the authors randomize transfers across villages, but also across "sub-locations" (groups of 10-15 villages). While all poor households in randomly selected "treatment" villages receive the cash transfer, some sub-locations are randomly chosen to have a higher proportion of treated villages than others. The advantage of this approach is that it allows the researchers to identify multiplier effects of the policy on non-recipients. The core idea is to compare effects on non-recipients which are close to treated villages against effects on non-recipients

[14] Egger et al. (2019) note that "Although small in relation to overall Kenyan GDP in 2015 (0.1%), locally this is a larger relative shock than most government transfer programs, e.g., the ARRA programs studied in the recent US stimulus bill fiscal multiplier literature." The unusual size of the shock is one additional reason to be cautious when attempting to export these results to other contexts: there may very well be non-linearities in responses to fiscal stimulus.

far from treated villages. If being close to treated villages increases income or consumption of non-recipients, this is evidence of multiplier effects.

This is exactly what the authors find, and unusually detailed surveying of households *and* businesses means they can credibly identify a particularly rich causal story. First, they find large positive impacts on consumption expenditure and holding of assets for households that receive transfers, as would be expected. They do not find notable changes in recipient household labour supply. They then show that local enterprises in areas with greater concentration of treated villages experience increases in total revenues, in line with the increased expenditure from grant-recipient households. In turn, these businesses increase their wagebill, and have somewhat higher profits. They then show that non-recipient *households* which are close to recipient households (in their village or nearby villages) *also* increase their consumption – and crucially this increased consumption seems to come from increased labour income, likely because local enterprises are now employing more people and paying higher wages. They therefore seem to identify a classic demand-side multiplier mechanism, whereby recipient households spend money in the local economy, which causes businesses to increase wagebills, which transmits into increased expenditure from these employee households. Interestingly, they find at most minimal effects on input and output prices, suggesting some "slack" in these markets. They can use their results to estimate a local multiplier, which ranges between 2.5 and 2.8 depending on the method used – very large relative to comparable estimates from the US (see Section 3.3.2).

While it has a number of unique methodological advantages over other papers in the literature, the Egger et al. (2019) findings are of course the results of one study in a very particular context. In a village-level RCT in Mexico, Angelucci and De Giorgi (2009) also find that the consumption expenditure of non-recipient households increases in response to transfers to recipient households, but do not detect a demand-side multiplier effect like in Egger et al. (2019). Instead, they suggest that increased non-recipient expenditure is driven by inter-household transfers from recipient to non-recipient households, as part of informal insurance and credit market activities. Like Egger et al. (2019) and Angelucci and De Giorgi (2009), Cunha et al. (2019) (analysing a different village-level RCT in Mexico), find that cash transfers have at most small impacts on prices in aggregate, but they find that these transfers increased local prices more in poorer and geographically isolated villages – as do Egger et al. (2019). Filmer et al. (2018) find large impacts on prices of certain nutritious foods in response to a village-level cash transfer RCT in the Phillipines, so much so that the programme notably increased stunting among non-recipient children (nutrition of recipient children improved).

Overall, there are probably still too few studies of cash transfer multiplier to be able to conclude robustly about likely effects in contexts other than those directly studied. It should be noted that the lack of evidence for a consumption multiplier in in Angelucci and De Giorgi (2009) is perhaps not a particularly strong finding. Though they find no statistically significant effects of transfers in treated villages on hours, earnings, sales or prices, they do not discuss the precision of these results. More concerningly, they note that goods markets are fairly integrated across their sample, and it will be fairly common that one store will serve both treated and control villages. In this case, their village-level randomisation approach (comparing effects on ineligible recipients in treated villages to ineligible recipients in control villages) will be insufficient, as ineligible recipients in control villages may also be affected by the cash transfer general equilibrium effects at their local store. Egger et al. (2019) avoid this by ensuring there are some randomly selected regions which are sufficently far from treatment villages. When it comes to price effects specifically, the Cunha et al. (2019), Egger et al. (2019) and Filmer et al. (2018) studies seem to suggest that some price increases are perhaps to be expected, perhaps particularly in remote areas, but the price effects in Filmer et al. (2018) are much larger than in Cunha et al. (2019) and Egger et al. (2019). In general, the heterogeneity of contexts and methods emphasizes that these studies may not all be measuring exactly the same quantity. Apart from differences in outcomes potentially being driven by heterogeneous contexts and differently-sized fiscal injections, it is only Egger et al. (2019) who set out to specifically estimate a multiplier, and who use a design which distinguishes between "directly treated", "indirectly treated" (spillover) and "fully non-treated" (control) groups. In designing an evaluation and comparing to the existing literature, one needs to be clear as to which spillovers one wants to assess.

3.2.2. South African evidence

South African social grants are explicitly targeted to the poor via means-testing, and we are not aware of cash transfer RCTs in the local setting. Instead, researchers have used natural experiments, exploiting policy details such as the age cut-offs for grant eligibility, or looking at "events" which affect grant receipt in a household, such as pensioner deaths. As far as we know, nobody has used this method to examine the potential local multiplier effects of South African social grants. However there is a large literature showing that when it comes to direct effects on recipients and their households, social grants reduce poverty & inequality (Woolard and Leibbrandt 2013), increase investments in education and child welfare (Case and Deaton 1998; Duflo 2003), and improve gender equality (Ambler 2016). They also have various effects on household composition and remittance behaviour (Jensen 2004; Woolard and Klasen 2005; Hamoudi and Thomas 2014)

A literature which has attracted particular attention is the effect that Old Age Pensions have had on co-residents' labour supply. While this is probably not a classic "multiplier" mechanism, changes in labour supply can have broader effects as in Muralidharan et al. (2017). We therefore provide a very brief summary of this literature.

Bertrand et al. (2003) use the pension age-eligibility thresholds to examine the labour force participation of prime-aged adults who are co-resident with the elderly who are just above and just below these age thresholds. Finding that prime-aged adults co-resident with the pension-eligible elderly have fewer working hours, they conclude that the pension reduces prime-aged labour supply. Posel et al. (2006) argue that the Bertrand et al. (2003) analysis does not sufficiently account for the extended nature of South African households and migrant labour. They reproduce the Bertrand et al. (2003) analysis but include *non-resident* household members in the analysis, and the negative labour supply effect disappears. The idea is that pension receipt by the elderly can finance job-search of prime-aged adults, or relieve childcare constraints for prime-aged adults (especially women).

Ardington et al. (2009) use longitudinal data (which follow households over time) to examine how migration and labour supply decisions are affected by changes in household pension receipt. They find a small increase in the employment of prime-aged adults when pension-receipt begins in a household, with these adults especially likely to be labour migrants. They conclude that their evidence supports Posel et al. (2006). However Abel (2019), following a similar method but with national data, finds that social grants unambiguously *decrease* prime-aged adult labour supply, and finds no evidence that grants reduce the childcare constraints of prime-aged adults. Ranchhod (2017) focuses on deaths of pension recipients, and finds that a combination of household compositional and employment changes mean that a higher proportion of working age adults are engaged in employment after the loss of a co-resident pensioner, as well more potential caregivers coming into the household while the number of dependents decreases. Abel (2019) exploits the pension age-eligibility criteria to show that gaining or losing elderly household members who are just shy of the pension age does not affect labour supply.

While the overall picture is still contested when it comes to labour-supply effects of pensions on co-resident prime-age adults, it worth highlighting a point raised by Abel (2019): despite pervasive normative assumptions to the contrary, from a welfare perspective there is nothing necessarily undesirable or inefficient about social grants allowing people to withdraw from jobs they would prefer not to do.

3.3. Macroeconomic evidence on multipliers

3.3.1. Calibrated models and time-series approaches

The fiscal multiplier is a macroeconomic concept, and the vast majority of attempts to empirically estimate this quantity have come from the empirical macroeconomics literature, using calibrated models or various time-series methods. Daniel Riera-Crichton, a research economist at the World Bank, presented on macroeconomic methods at our second workshop in advance of the development of this review, and this subsection draws from his presentation.[15]

Fiscal multiplier estimates from calibrated models start with building a theoretical, general model of a macroeconomy, which has space for context-specific *parameters* which affect the model, and which will be different for particular countries. The model is "calibrated" by substituting in plausible values of these parameters for the country under study, and from there it is quite straightforward to see what the model predicts about the size of the fiscal multiplier in that context. The difficult and contentious parts of this process are the theoretical model used to understand the macroeconomy (the best model for the macroeconomy has been a subject of vigorous dispute since at least the beginning of modern macroeconomics) and also what parameter values to use for the calibration.[16] Fiscal multiplier results can be sensitive to these assumptions, with little consensus on the "correct" approach. Internationally, various kinds of New Keynesian Dynamic Stochastic

[15] Daniel Riera-Crichton of course cannot be implicated in our interpretations of his remarks, nor our views about the methods discussed. In addition, he presented in his personal capacity, not as a representative of the World Bank.

[16] The correct values of these parameters are often far from obvious, with New Keynesian DSGE models often requiring estimates of "deep" and difficult-to-observe quantities such as the intertemporal elasticity of substitution or the subjective discount factor.

General Equilibrium (DSGE) models dominate this approach, while in South Africa Computable General Equilibrium (CGE) models have often been used. It is worth mentioning that the type of employer-specific labour-supply constraints discussed by Muralidharan et al. (2017) in Section 3.1.2 are usually *not* included in these types of models, with the result that these models preclude the Muralidharan et al. (2017) job-program results *by assumption*. Storm and Isaacs (2016) similarly note that the *assumptions* of standard CGE models also guarantee that these models produce negative employment effects from minimum wage increases, in contrast to what is found from the high-quality empirical evidence discussed in Section 2.4.

Time-series approaches rely on using changes over time for a particular country (usually) to identify relationships between variables. To give a highly simplified example, if a country increased it's fiscal spending by X in period t, one can estimate a multiplier by seeing how GDP changed in period t and following periods. The problem is that countries usually increase fiscal spending in response to local economic conditions, such as a recession, so that GDP is likely to be affected by factors other than the increase in fiscal spending, making it difficult to identify a causal effect of spending. Three approaches have typically been used to address this: 1) structural vector autoregressions (SVARs), which rely on strong and potentially restrictive timing or sign assumptions when it comes to how economic variables depend on each other (Blanchard and Perotti 2002; Mountford and Uhlig 2009), 2) "narrative" methods, which rely on qualitatively evaluating the reasons for fiscal policy changes, and only using those episodes unrelated to local economic conditions (Romer and Romer 2010), and 3) natural experiments, which use increases in spending driven by factors considered unrelated to local economic conditions, such as defense spending caused by wars (Barro 1981; Ramey and Shapiro 1998).

3.3.2. Sub-national cross-sectional multipliers

A new literature has also emerged which uses *sub-national cross-sectional* variation in spending to identify *local* multiplier effects, using natural experiments and data more similar to those discussed elsewhere in this review. Chodorow-Reich (2019) provides a comprehensive review. Rather than directly estimating national-level multipliers, analyses in this tradition use the intra-country unevenness of fiscal spending to identify the effects of stimulus by comparing regions which quasi-randomly received more fiscal spending to areas which received less. The key requirement is finding some factor ("instrument") which is sufficiently correlated with local fiscal policy but which is itself independent of local economic trends. A new sub-literature evaluating the stimulus impact of Unemployment Insurance (UI) payments in the US (following dramatic temporary expansions during the Great Recession and the Covid-19 pandemic) provides useful examples of approaches which could be applicable in South Africa.[17]

Boone et al. (2021) use a *border pair discontinuity design*. They exploit the dramatic increase in UI duration during the Great Recession, with variation in the increase across US

[17] As discussed below, these approaches require geographic policy variation which is more prevalent in the US than South Africa, due to the US's federal system. We do expect some geographic policy variation in South Africa nonetheless, either due to differential policy at the provincial or local government level (or space-based national policies), or because of uneven policy implementation.

states. Using county-level (similar to district councils) monthly data, the authors compare outcomes across all counties that border each other but are in different states. For example, while two nearby counties may share similar labor and product markets, one may be in a state that increased UI duration a lot while the other may have negligible change. It is particularly important that the comparison county has a similar unemployment rate (as implied by the shared labor market), since UI duration is correlated with higher unemployment and presents a potential confounder. In contrast to micro estimates which generally find a negative employment effect from raising UI duration, the authors find no significant employment effects at the macro level. They attribute this to multiplier channels such as the classic aggregate demand multiplier as well as jobs rationing.[18]

Casado et al. (2020) use a *shift-share instrument*. They analyze the $600 UI boost in place in the US from March to July of 2020 during the pandemic. Focusing on one state (Illinois), they exploit variation in the total stimulus across counties to evaluate the effect of the UI stimulus on economic activity as measured by bank transactions. To avoid concerns that the initial level of local demand affects unemployment and so the relationship between county unemployment and economic activity may reflect prior patterns, they use a shift-share instrument; that is, using the base period share of each industry in each county, they project the county unemployment rate based on the growth of the *national* unemployment rates in the composite industries. This method isolates the aspect of the UI stimulus associated with *national* industry trends rather than *local* factors such as demand, allowing the authors to evaluate the total impact on economic activity purely due to the stimulus, without worrying about UI being affected by local confounding factors. The authors find that eliminating the UI boost would result in a large decrease in spending.

Other examples of natural experiments used in the literature include Nakamura and Steinsson (2014), who use regionally-varying responsiveness to US defense spending by state, depending on the existing concentration of defence contractors; Hausman (2016), who uses variation in the geographic distribution of World War 1 veterans interacted with a large, one-time veterans bonus payment in 1936; and Corbi et al. (2019), who exploit discontinuities in the formula mapping local population to transfers from the federal government in Brazil.

3.3.3. Comparing the estimates

The aggregate macroeconomic multipliers estimates discussed in Section 3.3.1 are typically different quantities to those which come from the cross-sectional approaches in 3.3.2. Chodorow-Reich (2019) argues that sub-national cross-sectional multiplier should be understood as usually providing a rough lower-bound for a particular kind of national multiplier: the closed economy, no-monetary-policy-response, deficit-financed multiplier. On this basis, after collating and reviewing various estimates, Chodorow-Reich (2019) concludes that the cross-sectional literature suggests a value of about 1.7 for this quantity in the United States. Ramey (2019), in her review of fiscal multiplier estimates in developed countries, notes how different methods discussed in 3.3.1 have resulted in different ranges for fiscal multipliers.

[18] The jobs rationing mechanism assumes a labour market with substantial search frictions. Imagine that each job has a queue of workers: then longer UI duration for some workers just means others go to the front of the queue. The net number of jobs stays the same even if fewer workers are searching for work during their UI coverage period.

For multipliers on general government purchases, most methods result in estimates in the range 0.6-1, though the specific country context is important. For tax rate change multipliers, narrative methods suggest quite large estimates, with magnitudes around 2 or 3, while calibrated and estimated New Keynesian DSGE models suggest magnitudes lower than 1.

Van Rensburg et al. (2021), a team of South African Reserve Bank economists, review existing macroeconomic estimates of government expenditure multipliers in South Africa, and produce their own estimates using a macro econometric model. In their literature review, they show how existing estimates of this multiplier vary widely depending on method, assumptions, and time-period examined, ranging from frequently below 1 and "small" to 2. Their own estimates suggest that the South African fiscal multiplier has declined from 1.5 in 2010 to approximately zero in 2019.

Clearly, multiplier estimates depend both on the particular kind of multiplier being estimated and the method used, even aside from the period and context. Daniel Riera-Crichton, in his presentation at our second workshop, noted that evidence on the size of *social transfer* multipliers is particularly scarce, especially in developing countries.[19] In his own work with coauthors (Bracco et al. 2021), time series methods suggest that the social transfer multiplier is 3-times higher in Latin America (0.9) than in developed economies (0.3). Calibrating the Bracco et al. (2021) two-agent New Keynsian model to South Africa results in a social transfer multiplier of between 1.24 and 1.32.

The sensitivity of these methods and importance of local context all point to the value of credible evaluations of cash transfer and jobs programs multipliers in the South African setting.

3.4. Selected summary

In Table 2 we summarize the key evidence on jobs programmes, social protection grants and macro-economic multipliers. Despite the importance of evidence on stimulus effects, there is very little evidence in South Africa, and many of spillover mechanisms are only just starting to be explored internationally.

4. Policy details

This section summarizes the pandemic programmes we think are most amenable to quantitative evaluation, starting with the past round of the presidential employment stimulus (October 2020 to March 2021) and then considering the additional social protection grants of May 2020 to April 2021. A key challenge is that evaluation requires detailed knowledge of the programmes, and it would be necessary to fill these gaps before developing a credible

[19] Social transfers are payments to individuals from government social insurance and social assistance programmes. The main types of transfers are unemployment benefits, cash transfers to the poor (such as South Africa's social grants), and public pensions (Bracco et al. 2021). They include ongoing social protection programmes as well as emergency relief responses.

Table 2: Summary of key papers in stimulus literature

Authors	Programme evaluated	Method	Stimulus results
Jobs Programmes			
Muralidharan et al. (2017)	NREGS, India	RCT	Increases of 7% in private wages, 20% in private employment, and 13% in overall income
Imbert and Papp (2015)	NREGS, India	Staggered adoption	Increase of 5% in private wages, decrease of 1.5% in private employment
Beegle et al. (2017)	MASAF PWP, Malawi	RCT	Some evidence of negative effect on food security
Berhane et al. (2014)	PSNP, Ethiopia	Matching	Modest asset creation after persistent payments
Social grants			
Egger et al. (2019)	GiveDirectly, Kenya	RCT	Non-recipient consumption increased 13%, increased wage-labour earnings, increases in business revenue
Angelucci and De Giorgi (2009)	Progresa, Mexico	RCT	For every 100 pesos transferred, non-recipient consumption increases by 11 pesos
Filmer et al. (2018)	PPPP, Phillipines	RCT	Significantly increased prices, increases stunting among non-recipient children
Bertrand et al. (2003)	Old Age Pension, SA	Age discontinuity	Decreased employment of co-resident prime-aged adults
Ardington et al. (2009)	Old Age Pension, SA	Fixed effects	Increased employment of household prime-aged adults, through increased migration
Macro multipliers			
Chodorow-Reich (2019)	Local cross-sectional	Literature review	Lower-bound for closed economy, no-monetary-policy-response, deficit-financed multiplier of 1.7
Ramey (2019)	Government purchases	Literature review	Aggregate multiplier range 0.6-1
Ramey (2019)	Tax rates, narrative methods	Literature review	Multiplier absolute value of 2-3
Ramey (2019)	Tax rates, NK models	Literature review	Multiplier absolute value < 1
Boone et al. (2021)	UI, USA	Border discontinuity	No evidence of decrease in employment (rationalized by multiplier effects)
Casado et al. (2020)	UI, USA	Shift-share instrument	Large increase in spending

Notes: UI stands for Unemployment Insurance.

evaluation design.

4.1. Presidential employment stimulus

In April 2020, the president announced a R100 billion allocation to a jobs programme. R13 billion of this was allocated towards a number of programmes between October 2020 and March 2021, aiming to provide 700,000 "opportunities". Details are available publicly on the website of the presidency (Presidency 2020, 2021). As summarized in Table 3, these opportunities ranged from the creation of new jobs and the retention of at-risk jobs, to grants supporting the livelihoods of subsistence farmers. By February 2021, 600,000 opportunities had been supported, 360,000 of which were jobs that were new or previously vulnerable. We highlight four programmes as potentially good candidates for evaluation based on the size of the stimulus.

The largest programme is the school assistants programme operated through the Department of Basic Education. 200,000 educational assistants (helping with learning) and 100,000 general assistants (helping with school infrastructure maintenance) are in posts across all provinces at the time of writing. In total, the programme aims to create 345,000 opportunities (300,000 new jobs) with a budget of R7 billion. Recruitment occurred through the zero-rated SAyouth mobi site and was strongly oversubscribed, with 850,000 applications and selection based on skills.

Two other employment programmes look promising for evaluation. The Department of Social Development is supporting over 80,000 Early Childhood Development (ECD) workers by providing up to 6 months of R750 monthly top-up payments. Amongst other parts of the programme, 25,000 workers will be supported towards compliance with Covid-19 precautions. The Department of Environment, Forestry and Fisheries (DEFF) runs environmental upgrading work through the Expanded Public Work Programme, which the Presidential Employment Stimulus is supporting through making up for cut-backs as well as new employment. Nearly 15,000 new jobs will address backlogs of waste in communities (about R400 million), another 15,000 jobs are allocated towards water source rehabilitation (about R500 million), and around 16,000 jobs will conserve reserves, forests and parks (about R750 million). It is unclear how many of these latter two categories of jobs are new or retained.

In each of these mass employment programmes, the following information would be crucial (in order of priority):

- *What is the total wage bill by location and date?* Even better would be information on appointments by location, date, duration and wage. An analysis of local stimulus effects cannot happen without this data. We understand these data should be available for the DBE school assistants programme.
- *What are the details of the participant selection process? What data do we have on rejected applicants?* For example, if some applicants were rejected based on criteria not related to potential earnings, a credible design would match selected applicants with very similar rejected applicants (or applicants just-accepted over a threshold with applicants just-rejected below the threshold). Given the turbulence of the pandemic

labour market, we would otherwise struggle to predict how spending and employment would have evolved without the programmes. A programme evaluation needs to be able to construct a credible comparison group.

- *How do payments happen and were they on time?* As Muralidharan et al. (2016) emphasise, the stimulus impact can hinge on the actual payment process. As an example, we understand that payments to DBE school assistants may have been delayed in KwaZulu-Natal (KZN), which could provide scope for an analysis which compares stimulus outcomes in areas in KZN against comparable areas in other provinces which did receive payments on time.

Finally, the DALRRD support to subsistence farmers involves 35,000 vouchers of between R1,000 and R9,000. These are sizable grants with potentially large stimulus effects. Encouragingly, it seems a database has been compiled recording details about the farms and the disbursements. It would be crucial to understand and perhaps access this database. For example, does it record the location, grant size, commodity produced for each farmer? What about rejected applicants? As above, we need details on how candidates were selected and rejected for the programme. We understand there have been challenges in the implementation of this programme, including that much of the total budget was not allocated and that suppliers tended not to return the full value of vouchers to claimants in goods.

One way to fill the gaps in information highlighted above would be to pursue relationships with the relevant departments implementing the programmes (DBE, DALRRD, DSD and DEFF), or the recruitment agencies (Harambee).

4.2. Additional social protection grants

A month after the first lockdown was announced, several additional grants were put in place and existing grants were topped-up, to alleviate the COVID-19 poverty impact as several million jobs were lost (Jain et al. 2020). This represented an enormous expansion to the social protection system, with grants directly claimed by 18 million adults[20] or nearly half of the South African adult population. As summarised in Table 4, the most important of these were the top-up to the caregiver grant and the new Social Relief of Distress grant. In total, approximately R54 billion in top-ups and new social grants have been disbursed over May 2020 to April 2021.

The Caregiver grant was disbursed through systems which were already in place for the Child Support Grant (CSG), which is the largest single social grants payment programme in the country. Each of the approximately 13 million children have a registered primary adult who receives the cash payment of about R440 on his or her behalf, making a total of about 7 million caregivers. In May, the CSG was simply topped up by R300. From June to October however, a flat R500 payment was given to caregivers regardless of the number of children they received the CSG on behalf of. Together, these payments totalled R22 billion. Credibly evaluating the impact of these CSG top-ups is complicated by the simultaneous negative

[20] Excluding the children for whom the caregiver grants were ostensibly intended.

Table 3: Summary of presidential employment stimulus programmes

Department	Programme	Opport.	Budget (million)	Progress (as of February 2021)
Basic Education	New education assistants and support to vulnerable posts	345,000	R7,000	More than 300,000 assisstants currently in posts in every province
DALRRD	Income support for subsistence farmers	75,000	R1,000	Issued 35,000 vouchers of b/n R1,000 and R9,000. Verification near complete.
DEFF	Employment in environmental work	50,000	R1,983	
Social Development	R760 p/m (6 months) for ECD, compliance and retained social workers	111,000	R589	Physical site verifications ongoing.
Other		113,000	R1,849	
Transport	Provincial road maintenance	37,000	R630	Work has begun in all provinces except KZN and FS. 4600 jobs.
DSAC	Support for sectors	34,000	R665	Serious allocation challenges
Cooperative Governance	Municipal infrastructure maintenance	25,000	R50	Rollout due in financial year 2022, to support 15 municipalities.
DTIC	Incentives to global business services	8,000	R120	6500 jobs supported to date
Health	Expanding nurses	5,500	R180	Recruitment has begun from unemployment databases
Science and Innovation	Graduate programme support	1,900	R45	
Public works and Infra.	Emp in water and energy	1,600	R159	1875 participants recruited
Total		694,000	R12,421	

Notes: Abbreviations – DALRRD (Department of Agriculture, Land Reform and Rural Development), DEFF (Department of Environment, Forestry and Fisheries), DSAA (Department of Sports, Arts and Culture), DTIC (Department of Trade, Industry and Competition).

Table 4: Summary of stimulus through additional social grants (as of May 2021)

Additional grant	Amount	Recipients (million p/m)	Stimulus (billions p/m)	No. of months	Total stimulus (billions)
New grants					
Social relief of Distress	R350	5.7	R2.0	12	R24
Caregiver	R500	7.2	R3.6	6	R22
Top-ups to existing grants					
Old Age Pension	R250	3.7	R0.9	6	R6
Disability	R250	1.0	R0.3	6	R2
Other grants	R250	0.7	R0.2	6	R1
Total		18.3	R7.0		R54

Notes: The caregiver grant was initially disbursed as a R300 top-up per child to the Child Support Grant in May, which on average was equivalent to R500 per caregiver. However, because the caregiver grant does not depend on the number of children, the flat R500 payment in subsequent months gave less per child when the caregiver was responsible for many children (and more per child to caregivers responsible for one child). Due to significant delays in roll out, the Social Relief of Distress grant was disbursed primarily from August 2020 to March 2021 (often with multiple payments). Source: SASSA (2021).

shocks. For example, the COVID labour market shock decreased employment, including for caregivers. With school suspended, caregivers also had to provide additional meals (to replace free meals associated with the National School Feeding Scheme) and time in carework was drastically increased (Casale and Posel 2021).[21]

The Social Relief of Distress (SRD) grant had a very similar total disbursement of R24 billion, and at the time of writing the grant has been paid for 12 months to about 6 million unemployed claimants per month. However, the SRD grant was implemented very differently compared to the caregiver grant. Firstly, the grant was only disbursed to those who do not receive other grants (as verified in the SASSA database) and who were not employed (as verified using formal employment records). Secondly, applications were first solicited and vetted, resulting in delays before the first substantial number of grants, intended to be paid in May, were actually paid by August. More information on characteristics of individual SRD applications, approvals and rejections from SASSA would assist in an evaluation. This is especially important for the SRD as a new grant, as opposed to the Caregiver grant for which we can project receipt based on past data.

Overall, the combination of programmes over the last year presents several fertile and exciting possibilities for credible evaluations of the impact of local stimulus in South Africa – conditional on filling the key gaps in policy detail and data outlined above.

5. Data

In this section, we provide a description of data used in the stimulus effects evaluation literature, as well as potentially useful data for evaluations of the policies described in the previous section. We summarize some examples in Table 5.

The vast majority of empirical papers, using non-experimental methods or natural experi-

[21]The flat Caregiver grant may allow for a credible comparison between caregivers with one child compared to many children, implying different subsidies per child.

ments, rely on existing household surveys.[22] South Africa has a wealth of surveys to draw from; however, high frequency survey data collected *before*, *during* and *after* the programmes' periods are not easily available. The most obvious candidates for household surveys are Statistics South Africa's Quarterly Labour Force Surveys (QLFS) and the National Income Dynamics Study – Coronavirus Rapid Mobile Survey (NIDS-CRAM). However the sample size in NIDS-CRAM is too small for credible local-level district analysis, which is likely a constraint in QLFS too. In addition, due to COVID-19 precautions, both surveys were conducted by phone which has raised sample representivity concerns.[23] A potentially exciting candidate dataset is the population census, which is piloting in July of 2021 and would allow analysis at the local level, but which is only being fully rolled out at a later date, still to be announced. Ideally, we would want the survey to be as close as possible to the stimulus programme period. Stats SA's monthly production statistics are also useful, but are not publicly available at the local economy level.

Studies using experimental methods often carry out their own new surveys, as in Egger et al. (2019) and Muralidharan et al. (2017). These surveys are designed to elicit information on particular evaluation-specific quantities of interest, such as asking about reservation wages or surveying businesses to test for demand effects. In addition, original surveys are timed to suit the intervention, which avoids problems of recall or testing for stimulus effects that may have dissipated by the date of collection of an existing survey. On the other hand, a representative randomly sampled survey capturing pre- and post-treatment outcomes may be expensive to implement. The DBE's school assistants programme surveyed those who applied (accepted and rejected) through the SAYouth mobi site, which is a data source that may capture some of the benefits associated with new surveys. Nevertheless, there would be major returns to a well-designed survey intended specifically for evaluating the policy interventions described in Section 4: in addition to being better than any of the existing surveys in terms of timing, mechanism testing and statistical power for local economy effects, it would also offer advantages over the alternatives of administrative and private data discussed below – especially in terms of tracking the informal and cash-based local economy.

We draw attention to two further categories of data. Firstly, administrative data can be crucial in providing precisely measured details of the intervention being studied (including actual amounts transferred, which may differ from the stated amounts), as for example in Casado et al. (2020) and Boone et al. (2021), who use Unemployment Insurance administrative records. In South Africa, annual matched firm-worker tax records have been available to researchers in recent years, via a secure data facility. The drawbacks in our context, however, are that informal workers and firms are not observed, and that there will be a lag in the availability of the 2021 financial year tax data.

Secondly, even non-representative private sector data may be useful if combined with other sources and transparently discussed. Chetty et al. (2020) build a database combining data from credit transactions, stock market financial records, worker payroll and job posting firms,

[22]From the papers reviewed in this paper, this includes the evaluations of grants in South Africa (Abel 2019; Ardington et al. 2009; Ranchhod 2006; Jensen 2004; Posel et al. 2006), the PSNP jobs programme in Ethiopia (Berhane et al. 2014; Gilligan et al. 2009), and the Unemployment Insurance boost in the US (Boone et al. 2021).

[23]For example, the employment results of the NIDS-CRAM surveys for the fourth quarter of 2020 diverged dramatically from the QLFS, highlighting potential problems in either/both surveys (Bassier et al. 2021).

and Google mobility records. Mbiti and Weil (2016) analyze transfers from three outlets of Kenya's mobile money platform M-Pesa, observing each transfer and its amount. In each case, it becomes important to compare the data to a representative baseline survey. It may be possible to reach an agreement with large private sector firms, for example large formal retailers which track consumption expenditure (such as Spar) or mobile companies which track airtime purchases (such as Vodacom).

In particular, bank transaction data have proven crucial in a number of studies. Outside of South Africa, Casado et al. (2020) track debit and credit transactions in their study of pandemic economic activity, Ganong and Noel (2019) monitor bank outflows including by category of consumption, and Muralidharan et al. (2021) track the bank balances of farmers receiving cash grants in India. Besides providing highly-powered statistical analyses since bank records are typically linked to large populations, bank records are often also available at high frequency.

In South Africa, a potential source of bank transaction data is BankservAfrica, which is an automated clearinghouse, managing interbank switching, clearing and settlement. It is a private company owned by South Africa's private banks and regulated by the South African Reserve Bank (SARB). BankservAfrica collects transaction-level data on inter-bank transfers that they process, which includes a substantial part of but not all interbank transfers in South Africa. While the SARB has access to their transaction-level data by law, BankservAfrica does not make the data publicly available, and ownership of the data rests with the banks. BankservAfrica does however aggregate some data which they publish as economic indicators (for example, the BankservAfrica Economic Transactions Index). The interbank transactions which BankservAfrica observes include interbank EFTs, interbank point-of-sale credit and debit card transactions, and interbank ATM withdrawals. In these cases they observe individual transactions over time, which are attached to particular accounts (creating panel data). In addition, they observe all payments from SASSA (which can be used to identify grant recipients and payments in the data) as well as from each government department (which can be used to identify employment stimulus programme recipients and payments).[24] The major disadvantages of the data are that they do not observe *intra-bank* transfers (including if a person draws cash from their own bank's ATM), cash transactions (which excludes most of the informal economy), or account/transaction location at more detail than the city and province. Overall, this is a potentially rich source of data, though the extent to which it is accessible or amenable to analysis is at this stage unclear.

These data sources should not be viewed in opposition to each other. Different data sources complement each other, by shedding light on different samples and providing sensitivity tests for the conclusions.

[24] By "identify" we mean allow researchers to distinguish between recipients and non-recipients. In both cases the individual anonymity of grant and employment stimulus recipients would be retained.

6. Conclusion

This review provides background for an evaluation of the stimulus effects of the Presidential Employment Stimulus Programme and social grant top-ups associated with COVID-19 and implemented over 2020-2021. By stimulus, we mean the indirect effects on non-recipients, and we have in mind channels such as the demand multiplier that operates through the local shopkeeper of the programme recipient.

We place emphasis on quantitative approaches using quasi-random variation in programme receipt, and discuss the evaluation design considerations which make such approaches more or less credible. As an important caveat, we highlight that there are other approaches we do not address here, but that could add important insights, such as qualitative methods. These complementary approaches could be particularly valuable in our context where it is difficult to quantitatively evaluate spillover mechanisms when they occur in the cash economy, due to data constraints.

We review key studies in the international and local literature, and show that there have been diverse effects across different contexts and programmes. We draw out substantive and methodological lessons for prospective studies of the local context. Importantly, we note that high-quality studies on South Africa's Presidential Employment and social grants programmes will rely on detailed knowledge of policy design and implementation details, and we list several questions that will be crucial to have answers for when designing an evaluation. Finally, we describe some of the data available for these evaluations, noting their strengths and weaknesses. In our view, the most promising directions here are to carry out a new survey specifically designed for this evaluation, and/or to use high frequency transaction-level bank data from BankservAfrica.

Notwithstanding these important methodological and data concerns, the Presidential Employment Stimulus Programme and social grant top-ups present an exciting opportunity to credibly measure stimulus effects – and improve our understanding of the South African economy and policy environment.

Table 5: Examples of data used to evaluate stimulus effects

Dataset	Design	Key variables	Notes
Existing survey			
Muralidharan et al. (2017)	Census cross sections of households and enterprises, post treatment	Income, education, assets, number of employees	Complemented by original surveys, see below
Census (SA)	Pilot July 2021, full census tbc	Location, employment	Timing of full census may be too long after stimulus programmes
NIDS-CRAM (SA)	Phone-based adult panel of randomized sample, five rounds 2020-2021	Wages, employment and rich covariates	Small sample size not suitable for local disaggregation, concerns about representivity
QLFS (SA)	Adult panel of randomized sample, quarterly	Wages, employment and rich covariates	Phone-based sampling in 2020; larger sample then NIDS-CRAM, but similar concerns
New survey			
Egger et al. (2019)	Household survey (baseline and follow-up)	Econ. activity, income, expenditure	Conducted own census to create sampling frame
Egger et al. (2019)	Enterprise survey (baseline and follow-up)	Profits, revenues, wagebill, inventories	Conducted own census to create sampling frame
Egger et al. (2019)	Surveys of commodity prices in local markets	Prices of 72 products	Monthly data, 61 spatialy disaggregated markets
Muralidharan et al. (2017)	Randomized panel of NREGS recipients (baseline, follow up)	Income by source and household employment	
SAYouth.mobi (SA)	All online applicants to DBE school assistants programme	(unclear)	Includes rejected applicants
Administrative			
Casado et al. (2020)	Unemployment Insurance records	Location, industry, date, wage	
SARS tax (SA)	Matched firm-worker tax records	Wages, job transitions, sales, profits	Formal sector only. Availability of 2020 data unclear; annual.
Private companies			
Ganong and Noel (2019)	JPMorgan Chase Bank account transactions	Monthly household spending and income	Representivity (cross-check with other sources)
Bankserv (SA)	Interbank account transactions	Amount and recipient	Intra-bank and cash transactions unobserved
Chetty et al. (2020)	Combines private credit transactions, payroll, and publicly traded firms	Spending, business revenue, disaggregated employment	
Mbiti and Weil (2016)	M-Pesa mobile transfers from 3 outlet locations	Transfer timing and amount	

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