

CODESRIA

Bulletin

Number 1, 2020

ISSN 0850 - 8712

Special Issue

Randomised Control Trials and Development Research in Africa

Editorial

This issue of the Bulletin returns to ongoing work theorizing Africa's economic development. It zeros in on the debate on Randomised Control Trials (RCTs) in the design of development interventions for and in Africa. A resurgent area of western intellectual curiosity and policy initiative, RCTs recently attracted renewed attention and unexpected validation with the award of the 2019 Nobel Prize in Economics to

sign of development interventions in Africa, and was lauded by the Nobel Committee for thus making a major contribution to poverty alleviation. This catalysed vibrant debates and rebuttal amongst academics, development practitioners and public policy experts that continues to date, including on social media platforms. The debates centred around the merits of applying RCTs to development thinking in the continent. Consistently, interlocutors have sought to contextualise the literature on RCTs within the historical sociology of knowledge production and dissemination, with an emphasis also being placed on the impact on development outcomes.

Beyond whatever signal the Nobel Prize sent for research and development thinking, the theoretical and ideological assumptions RCTs engender remain problematic conceptually and methodologically. Of course, the use of RCTs in the field of biomedicine, for example, carries enormous value and has led to results that sit at the core of scientific advancement. Not so for economics where critics, even when they acknowledge the importance of experimental as opposed to observational approaches, caution against

the tendency to accord RCTs special status.¹ "Every discipline is constituted by what it forbids its practitioners to do."² At its basic, this injunction that is associated

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Esther Duflo, Abijit Banerjee and Michael Kremer. This trio was awarded the 2019 Nobel Prize for their work in adapting the experimental method of RCTs to the de-



This quarterly Bulletin is distributed free to all social research institutes and faculties in Africa to encourage research co-operation among African scholars. Interested individuals and institutions may also subscribe. Contributions on theoretical matters and reports on conferences and seminars are welcome.

with Nietzsche, limits and represses interdisciplinary creativity. But there are instances where deployment of methodological tools from one discipline to the other is conceptually dangerous and ethically injurious; the application of RCTs in research used to design development intervention in Africa is a particularly pernicious instance.

For some, the application of RCTs in development thinking in Africa represents the new gold standard in conceptualizing research in economic development. For these scholars, replicating RCTs is good science and positioning outcomes of RCT research to influence the design of development policies is laudable. Those who remain sceptical question the design, validity and impact of the methodology and perceive in it echoes of past experimentations that have only served to distract progressive economic thought in Africa. As Grieve Chelwa and Nimi Hoffmann demonstrate in this issue of the Bulletin, RCTs have significant conceptual and design flaws and can be deficient in addressing questions for which they appear well suited to address.³ Chelwa and Hoffmann argue that the design of RCTs do not take the holistic context in which the poor live and end up ignoring the range of factors influencing people's choices. Chelwa, in particular, finds the march of development economics into an experimental field absolutely problematic. The implications of using results from this experimental methodology as a basis of designing public policies for poverty interventions are dangerous at best, given the inadequate appreciation of the overall social context, a context that in real life can neither be 'randomised' nor 'controlled.'

These concerns are not new and have been alive in economics for decades. Using Africa to validate or invalidate medical assumptions or development interventions has a well-documented but problematic past. The history of using blacks or Africans as guinea pigs in experimental medical and anthropological research is known. This has previously been the basis for important ethical questions and concerns, leading most research institutions to lay out broad ethical parameters against which approval by Institutional Review Boards (IRBs) for research involving human subjects has to be based. However, even as application of these rigorous parameters continues, the research industry embeds power relations that find other means to legitimize research interventions and outputs even if these do not adhere to standard ethical norms. This puts IRBs squarely at the centre, especially because some have morphed into gatekeepers that authorise specific, well-funded institutions to conduct research in particular places, while preventing research inquiry into other "special" communities. As Angus Deaton notes, even in the US, for instance, "nearly all RCTs on the welfare system are RCTs done by better-heeled, better-educated and paler people on lower income, less-educated and darker people."⁴ In this Bulletin, Nimi Hoffmann and Seán M. Muller identify and discuss the ethical

problems and the dangerous policy consequences of RCTs. These are particularly manifest in the alarming revelations in Muller's research on RCTs conducted in the field of education in South Africa.

Interestingly, the trust and legitimacy deficits associated with RCTs seems to be largely dependent on their use. Rosaine N. Yegbemey, in the piece on climate-adaptability in relation to the needs of small-holder farmers in the North of Benin, shows that the application of RCTs methodology did present positive possibilities, providing farmers with climate-adaptability in relations to farming needs of smallholder farmers in the North of Benin the positive possibilities of using RCTs. In this specific case, the application of this methodology has proven effective in providing farmers with climate-related information that is useful in influencing adaptation decisions. On the contrary, RCT interventions have proven ineffective when there are major trust and legitimacy issues among the subjects of their experiment. In her analysis based in western Kenya, Marion Ouma illustrates how cash transfer interventions by a private organization in Nyanza, Kenya elicited more mistrust and resistance compared to transfers done by the government. This resistance to RCT driven cash transfers has also been documented in similar cases in other African countries like Malawi and Zambia.

In brief, articles in this issue of the Bulletin assert the need to deepen intellectual reflection on conceptual and methodological questions relating to the deployment of RCTs in the social sciences. The ethical questions the researchers raise require more than a laudatory response. We thank Nimi Hoffmann for proposing the special issue and her assistance in facilitating the publication of these articles. The French version of these articles will be issued later in the year. May this also serve as a reminder of the invitation to any African academic interested in putting together a special issue of CODESRIA Bulletin to please contact the council.

Notes

1. Angus Deaton and Nancy Cartwright, "Understanding and misunderstanding randomized controlled trials," in *Social Science & Medicine*, Vol. 210, August 2018, 2-21
2. Hayden White as cited in Keith Jenkins, *Re-thinking History*, London and New York, Routledge, 1991, preface.
3. Deaton and Cartwright, "Understanding and misunderstanding randomized controlled trials."
4. Angus Deaton, *Randomization in the Tropics Revisited: A Theme and Eleven Variations*, NBER Working Paper No. 27600, <http://www.nber.org/papers/w27600>, p. 21.

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Pop Developmentalism in Africa

This article traces the intellectual history of development economics from its initial preoccupation with the big questions of industrialisation to its current focus on micro-level extra-market and extra-political interventions. Whether it is administering deworming tablets in Kenya or teaching negotiation skills to girls in Zambia, this new approach to development implicitly promises wide-scale transformative development for the adopters of its medicine. I argue that the current policy prescriptions of the field cannot on their own lead to wide-scale transformative development. If anything, the interventions called for by the new development economists are the results and not the causes of transformative development. I call for a more eclectic approach to development economics that largely borrows aspects and ambitions of the field's forbearers but grounds itself in the specificities of individual developing countries.

I have three objectives in this essay. First, I would like to show that the practice of development economics has gone through two major phases over the last 80 years or so from its formal birth in the 1940s. Second, I will argue that the current practice of development economics, as encapsulated in the intellectual labour and policy prescriptions of the 2019 economics Nobelists (Abhijit Banerjee, Esther Duflo and

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Michael Kremer) and their disciples, cannot on its own lead to wide-scale transformative development of the kind we have come to associate with the now industrialised countries. If anything, the kind of development interventions that the trio's work calls for are likely to be the results of transformative development and not its cause. My conclusion here holds, even if Banerjee et al. were to resolve, by some *deus ex machina*, all the myriad concerns about internal validity, external validity and research ethics that have been levelled at their methods (Deaton 2010; Muller 2015; Hoffmann 2019). Lastly, I will argue for a more eclectic approach to development economics that largely borrows aspects and ambitions of the field's forbearers but grounds itself in the specificities (history, politics, etc.) of individual developing countries.

In its initial incarnation in the 1940s, development economics was concerned with the big question of how to fundamentally transform the economies of the then 'Third World'. Albert Hirschman, himself a pioneer of the field, writes that 'development economics started out as the spearhead of an effort that was

to bring all-around emancipation from backwardness' (2013: 69). In this initial formulation, development unequivocally meant sustained increases in income per head. And the vehicle that was to deliver this was industrialisation. The early pioneers (Paul Rosenstein-Rodan, Ragnar Nurkse, W. Arthur Lewis, Kurt Mandelbaum, Albert Hirschman, among others) all agreed that the process of industrialisation required an omnipresent state to not only address market failures, which were said to be endemic in the developing world, but to also engage in entrepreneurial ventures. Much of the intellectual debates at the time were split between those who believed that industrialisation required a 'big push' along a 'balanced growth' path (Rosenstein-Rodan, Nurkse, and Lewis to some extent) and those who believed that 'unbalanced' sectoral linkages were key to industrialisation (Hirschman). These were the halcyon days of classical development economics.

From the early to mid-1970s, a crisis of confidence arose within development economics as a result of the many false industrialisation starts in the developing world. Countries that should have been well on their way to 'take-off' did not do so and those that had taken off crashed only moments after. The growing mathematisation of economics that was well underway at the time meant that the pioneers of development economics, many

of whom were not trained in the new orthodoxy, could not adequately respond to the charge levelled by neoclassical economists that false starts were the result of government-inspired resource misallocations. Further, neo-marxists pointed out that rather than bridge inequalities, as hypothesised by the classical development economists, whatever little industrialisation that had taken place had had the actual effect of deepening intra- and inter-country inequalities. Lastly, many of the efforts towards industrialisation were said to have happened at the expense of political and democratic progress under authoritarian regimes, aspects that had been completely neglected by the first generation development economists.¹ All these factors, according to Hirschman (2013), resulted in the decline of development economics, at least in its classic vintage.

By the 1980s many poor countries were in the grips of economic crises. In the case of sub-Saharan Africa, the World Bank traced the origin of the crisis to the economy-wide distortions that had partly been inspired by the work of development economists (World Bank 1981). The prescription of the World Bank and other allied institutions was, therefore, straight forward: poor countries, especially those in sub-Saharan Africa, needed to structurally adjust their economies in favour of market-based allocations coupled with a minimal role for the state. However, by the 1990s it had become apparent that structural adjustment had been the wrong prescription for the wrong crisis (Mkandawire and Soludo 1998). Many African countries had implemented the requirements of structural adjustment with devastating results, especially for the poor.²

At the start of the twenty-first century, the international financial institutions (IFIs) came to the position that the singular focus on markets inherent in structural adjustment policies (SAPs) had adversely impacted the lives of the poor. Thus, they now required governments in Africa to prepare Poverty Reduction Strategy Papers (PRSPs) that were to articulate how governments would protect the welfare of the poor. However, the IFIs and many in the donor community still held the view that statist policies were to blame for the crisis. These views were heavily influenced by work coming out of the 'neo-patrimonial school' that used an incredible array of epithets ('tribal', 'corrupt', 'cronyistic', 'parasitic', et cetera) to describe the bankruptcy of the African state (Mkandawire 2015).

It is into this milieu that today's mainstream version of development economics was born. The donor community, given the incompetence of the African state, insisted on the direct provision of aid to needy communities. Government involvement in this process, if any at all, was to be kept to a bare minimum. Thus began the era of Non-Governmental Organisations (NGOs) and Project Aid.

Inspired by the Millennium Development Goals (MDGs), donors identified micro-interventions in mostly health and education as these were considered to be important inputs into development. The large infusions into Project Aid, however, required rigorous evaluations to figure out 'what works' (Duflo and Kremer 2008). Knowing what works is not only important for the purposes of accounting for tax dollars on the part of donors, but for the transplanting of this knowledge to other settings in the developing world. Mainstream economists,

armed with the tools of credible causal identification (Angrist and Pischke 2010), were particularly suited for this challenge. Thus they formed a symbiotic relationship whereby donors supplied financial resources and economists provided credible answers as to which interventions worked. And to do this, economists carved out for themselves 'islands of normalcy' in the developing world (i.e. places that were insulated from the neo-patrimonial urges of the state) to run their development experiments with local NGOs as their favoured implementing partners. Development economics was no longer concerned with the large macro-question of how to permanently increase income per head but with micro-level questions around whether certain interventions (mosquito nets, deworming tablets, iodised salt, teaching negotiation skills to girls, et cetera) improved some narrow measure of the poor's welfare.

This reorientation of the field of development economics towards micro-level concerns betrays a complete misunderstanding of what is commonly meant by development. In saying this I am not disputing the claim that some of the micro-level interventions favoured by the new development economists do alleviate some of the poor's hardships (of course I am abstracting here from the still unresolved questions about internal and external validity, et cetera). However, the argument is that the favoured interventions of the new development economists are in no way the sine qua non of development. At best, these interventions act as a feel-good tourniquet meant to temporarily alleviate suffering much like humanitarian assistance. And as Deaton (2013) has argued in

the case of Britain, spectacular improvements in well-being (as measured by, say, life expectancy) in the late eighteenth and early nineteenth centuries followed increases in the general level of incomes in the economy. Increased incomes, in turn, accorded the authorities the resources needed to invest in large-scale public sanitation infrastructure. This is also what has characterised the Chinese experience over the last 30 years. The new development economists are guilty of putting the cart before the horse.

There is a pressing need for development economics to reconnect with its historical ambition and preoccupation of thinking through the mechanisms that are likely to permanently and fundamentally transform the lives of the poor. And as Rodrik (2008) argues, some of the empirical skills in the new development economists' toolkits can be helpful here.³ These will, however, have to be combined with other methods of knowing complete with multidisciplinary approaches (history, politics, et cetera) that dig deep into the experiences of individual countries. Mkandawire (2001) shows that many African countries scored a lot of progress in the first 20 years after independence and mostly on the back of industrialisation. Serious minds are needed to deduce

yesterday's lessons for today's development challenges.

Notes

1. The experience of W. Arthur Lewis as economic advisor to Kwame Nkrumah in newly independent Ghana is illustrative of this point (see Tignor 2006).
2. Van De Walle (2001) argues that structural adjustment policies (SAPs) did not succeed in the case of Africa because many governments did not actually implement them. The careful work of Mkandawire and Soludo (1998) shows that many African governments actually went even further in their implementation of SAPs than what was required by the international financial institutions.
3. See for example Lane's (2019) survey of the 'new empirics of industrial policy'.

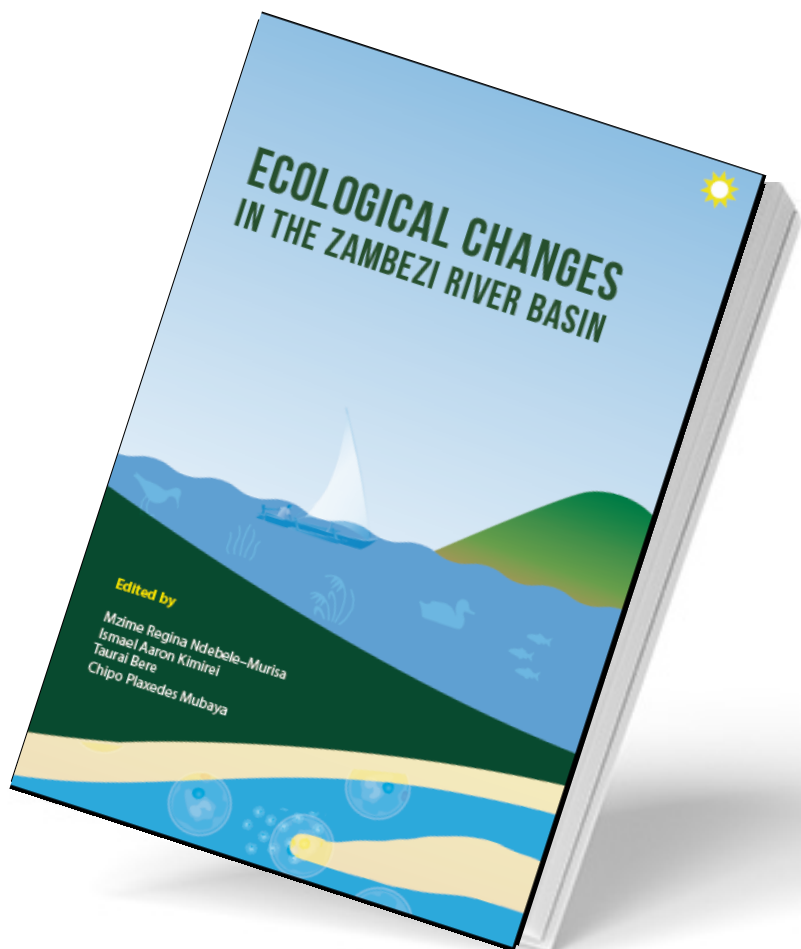
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Book Announcement

Annonce d'un livre



ISBN: 978-2-86978-713-1

Pages: 268

This book provides an analysis of the ecological conditions and ecosystem goods and services of the Zambezi River Basin (ZRB), the fourth largest river in Africa. Various environmental and anthropogenic factors; inclusive of climate, environmental flows, hydrology, morphology, pollution and land use changes among others and their interactions are considered as drivers of the river ecosystems. The impacts of these drivers on aquatic biota, river ecological integrity, and the livelihoods of surrounding communities are analysed within the socio-economic-policy context. The book goes beyond the usual inventories and basic research by using the comparative research method (CRM) in a trans-disciplinary

manner. This CRM analytical approach in this book seeks to interrogate the differences or similarities in socio-economic systems, livelihoods, ecological systems, ecosystem goods and services, their usage and management under the seemingly different cultural, socio-economic conditions expected across borders that are within the ZRB. The multidisciplinary approach also connects the typical ecological research with social dimensions in a holistic manner. The book therefore, provides empirical and research based evidence to support strategic planning and policy development in the wake of ecological changes that nations and indeed regions such as the ZRB are grappling with while seeking to sustainably manage precious river systems.

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Involuntary Social Experimentation: Revisiting the Case for a Moratorium

In 2019 the Sveriges Riksbank Prize (‘Nobel’ prize) in Economics was awarded for the use of experiments to evaluate social policy interventions in former colonies. These social experiments, the Nobel Committee (2019) claims, have ‘helped to alleviate global poverty’ and have ‘the potential to further improve the lives of the worst-off people around the world’. It is striking then, that the award makes no mention of the ethics of experimentation on highly vulnerable people. In this piece, I revisit the evidence I gathered on informed consent in social experiments in former colonies, which suggests that many studies face serious problems with informed consent (Hoffmann 2020). My intention is to show that involuntary experimentation is an important ethical and intellectual issue for Southern scholars.

The argument is composed of four moves. First, I explain how the design of many experiments pose serious obstacles to informed consent. Second, I aim to show that involuntary experimentation on vulnerable people in former colonies is unethical: it violates their personhood, increases the risk of unintended harm, and establishes continuities with colonial experimentation. Third, I engage with objections that informed consent is unnecessary, and that the demand for informed

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consent in social experimentation is an illegitimate infringement on the sovereignty of the state. I argue that these objections are unjustified, and that informed consent is a central component of democratic social policy. As a result, instead of strengthening social policy, involuntary experimentation weakens it. Fourth, I aim to show that the political economy dynamics of social experimentation mean that existing regulatory mechanisms are likely to be ineffective. However, social experimentation is still a relatively low-stakes enterprise compared with medical experimentation, and there is therefore a window of opportunity to develop more effective regulation. In light of this, Southern scholars have a clear responsibility to call for a moratorium on social experiments and to participate in establishing more effective ethical safeguards.

Obstacles to informed consent

I begin by revisiting the evidence on informed consent. The evidence

comes from a systematic review of all randomised controlled trials (RCTs) published between 2009 and 2014 in ‘top economics journals’ that had been previously conducted by Peters et al. (2016). I used their review because it indicates the standards of journals considered to be the most rigorous in the discipline.

In the original article, I focused on experiments conducted in Africa, Asia and Latin America (58 out of a total of 92 studies). To extract information on informed consent, I used a minimalist criterion: participants knew they were in some sort of study before agreeing to participate. This did not require them to know that they were in an experiment, or to know the details of the experiment before consenting to participate. By this criterion, 78 per cent of authors did not discuss informed consent, 12 per cent stated that participants were intentionally left ignorant, and 10 per cent indicated informed consent for some sort of study. However, no study indicated whether participants were explicitly aware that they were being experimented upon. (Table 1). This silence on informed consent, and in some cases explicit denial thereof, suggests that it is considered less important than other elements of the experimental design.

Table 1: Features of experiments in former colonies related to informed consent

	% not stated	% no	% yes	Total
Participants consented to participate in some sort of study	78	12	10	100
Participants aware that they were in an experiment	100	0	0	100
Cluster randomisation	0	36	64	100
Institutional setting	0	76	24	100
Impoverished participants	0	3	97	100
Child participants	0	84	16	100
Addresses design constraints on ability to opt out	100	0	0	100
Approved by university ethical review board	91	0	9	100

However, the experimental design of many of these studies presents serious obstacles to informed consent. One barrier concerns the practice of randomly allocating treatments to clusters, such as schools or clinics. Cluster randomisation often makes informed consent unfeasible (Lignou 2018). This is because it may be costly to leave the service of the implementing agency (such as switching schools), or participants may be locked into the service (such as relying on social welfare), or the service may be the cheapest or most convenient option (such as using the closest clinic). Although 64 per cent of the studies employed cluster randomisation, no study discussed whether participants could not opt out because it was costly to leave the cluster, and how this was addressed.

A second barrier to consent concerns the vulnerability of participants. Sixteen per cent of studies used children as participants, yet only one study explicitly gained the consent of parents. Twenty-four per cent used institutional settings, such as clinics or schools, but no study discussed whether participants believed they would suffer professional consequences if they refused to participate and how this was addressed. And nearly all

the studies involved the allocation of scarce resources to impoverished participants, but no study discussed whether penury compelled people to participate and how this was dealt with.

By design then, it appears that most of the studies in the review had serious built-in obstacles to informed consent. I discuss methodological reasons for suspending informed consent below, but first I consider the ethical implications of involuntary experimentation.

The ethics of informed consent

The suspension of informed consent on vulnerable people is consequential. First, it raises the distinction between treating humans as persons who have a right to participate or not as they so choose, versus treating them as subjects to be manipulated for research purposes. (Barrett and Carter 2010: 520). It is for this reason that informed consent was incorporated into the International Covenant on Civil and Political Rights as one of the ‘inalienable rights of all members of the human family . . . derive[d] from the inherent dignity of the human person.’ (UN General Assembly 1966). This framing casts

the absence of informed consent as a violation of personhood in and of itself, outside of any negative consequences it enables. Seen this way, involuntary experimentation arguably violates the personhood of some of the world’s most vulnerable people – impoverished black and brown people, many of whom are women.

Second, it increases the risk of unintentional harm. If participants are aware of the true nature of the intervention, its risks and trade-offs, they may be able to alert experimentalists to unintended negative consequences. This is important for experiments that allocate critical resources, such as income or healthcare, to impoverished people. Withholding or providing resources to particular groups may harm vulnerable groups or catalyse contestations that are socially destabilising (Acemoglu 2010). The principle is a general one: while each individual has rich, complex and deep knowledge about herself, experimentalists necessarily have sparse and inadequate knowledge about participants (otherwise they would have no reason to study them). Since all social experiments are characterised by information asymmetries between experimentalists and participants, experimentalists do not fully

know what harms they may cause. Insofar as informed consent allows participants the opportunity to reduce experimentalists' ignorance, it plays an invaluable role in reducing the risk of harm.

Third, the suspension of informed consent increases the risk of establishing historical continuities with colonial experimentation.

While many colonial experimentalists hoped to help the lives of the poor and contribute to science, their experimentation was often involuntary and harmful, and had the effect of positioning entire regions as though they were 'living laboratories' in which scientific curiosity and the urge for beneficence could be satisfied (Tilley 2011). Stark

regional asymmetries in authorship heighten this risk (Table 2). Of the experiments conducted in former colonies in the literature review, 84 per cent of lead authors were at institutions in the United States or Western Europe. No first authors were located in Africa or Latin America, and only 5 per cent were in Asia.

Table 2: First author location of experiments conducted in former colonies

	Frequency	Percentage
Africa	0	0
Asia	3	5
Europe	10	17
Latin America	0	0
United States	39	67
World Bank	6	10
Other	0	0
Total	58	100

Dealing with objections

The suspension of informed consent in social experiments is typically a response to the problem of external validity, or the ability to apply the findings outside the context of the study to another place or time (Barrett and Carter 2010). If participants know that they are in an experiment then they may behave differently than they would under non-experimental conditions, so that the outcomes of an intervention might not scale-up to a population. Yet the standard solution in medical research – assigning a placebo – is not possible in most social experiments (Peters, Langbein, and Roberts 2016). Thus, there is a prima facie 'greater good' argument for violating informed consent – it helps ensure the external validity of experiments in order to contribute evidence for more beneficial policies.

With or without informed consent, however, social experiments face serious problems of external validity. The effects observed in the sample are unlikely to be similar to the effects in the population due to general equilibrium and political economy effects (Heckman 1992; Moffitt 1992; Deaton 2010; 2010), while the perception that experiments are non-parametric and theory-free is inconsistent with claims to generalisability (Muller 2015; Deaton and Cartwright 2016). And even if experimental results could generalise to different people or times, this assumes that experiments lead to more beneficial policies than alternative forms of research. This is a counterfactual claim for which no experimental evidence has yet been forthcoming (Chelwa and Muller 2019). Indeed, the role of medical experiments in harmful outcomes, such as the opioid crisis, cautions against

strong claims about policy benefits (Deaton forthcoming). Thus, appeals to external validity do not address concerns about suspending informed consent; instead they simply shift the terrain to even thornier methodological issues regarding external validity and uncertainty.

Defendants of involuntary experimentation might instead argue that the potential harms of social experimentation are trivial in comparison to medical experimentation, and that indeed, the potential benefits of suspending informed consent outweigh its harms. Singer et al. (2019) provide the following analogy to advance this utilitarian argument:

The philosopher Derek Parfit asks whether a person trapped in a collapsing building may break an unconscious stranger's toe in order to save a child's

life. Most agree that ‘using’ the stranger in this way is ethically permissible. Similarly, RCTs have occasionally identified interventions that are tens or even thousands of times more effective than others.

However, this is a poor analogy. The example of the trapped person involves just three people in a tightly circumscribed scenario. Social experiments typically involve large numbers of people in porous and complex social settings. In the systematic review discussed above, the majority of studies had sample sizes ranging from thousands of individuals to millions of households (Peters, Langbein and Roberts 2016). This complexity and scale necessarily involve information asymmetries between experimentalists and participants. Thus, not only are the potential harms not fully knowable, but the potential benefits as well. A utilitarian argument requires some knowledge about the possible harms and benefits of an action in order to weigh them against each other. Yet the very mechanism by which better information about prospective harms and benefits could be derived – informed consent – is ruled out by appeal to the supposedly beneficial consequences. The utilitarian argument against informed consent falters on the grounds of circularity.

Another line of defence is to appeal to the fact that social experiments typically piggyback on existing interventions conducted by governments, NGOs or firms. Thus, if interventions are going to be imposed unilaterally, then social experimentalists may as well gain knowledge from these interventions, which can be used to identify any harms (Singer, Baker and Haushoffer 2019). This echoes a well-established view that all social policy interventions

are experiments, but the point is to make the knowledge from these interventions socially useful. Writing in 1938, for instance, the British social theorist Beatrice Webb argued:

All administration, whether from the motive of profit-making or from that of public service, whether of the factory or the mine, of the elementary school or the post office, of the co-operative society or the Trade Union . . . necessarily amounts to nothing less than ‘experimenting in the lives of other people.’ (Cited in Oakley 2000: 318.)

The difference is that social experiments unveil the cloak of secrecy in government interventions and therefore make useful contributions to knowledge. However, as early proponents of ‘reforms as experiments’ recognised, this does not entail suspending the principle of informed consent, for doing so evades personal responsibility (Campbell 1969; Campbell and Russo 1999). This version of the argument is weak, because it relies on buck-passing.

A stronger version of this argument is that it is inconsistent to require social experimentalists to gain informed consent, when one does not require the implementing agency, and particularly governments, to gain informed consent (MacKay and Chakrabarti 2019; Meyer et al. 2019). This is an important demand for consistency. But instead of waiving the principle of informed consent for experimentalists, as these authors suggest, there are strong reasons to insist on a consistent commitment to informed consent.

The first reason concerns the problem of asymmetrical information. If governments and NGOs ought to ensure the well-being of the people they serve, then informed

consent is an important mechanism by which they can reduce their ignorance of the harms and benefits of social policy interventions. From this perspective, informed consent is an important component of strengthening the ability of governments and NGOs to benefit people.

The second reason applies particularly to governments that claim legitimacy on the grounds that they represent the will of the people they govern. MacKay and Chakrabarti (2019) argue that legitimate governments do not need to gain informed consent in social experiments, because policy programming is part of their ‘right to rule’. On this view:

Individual residents are legitimately sovereign over those spheres of action protected by their right to autonomy, and governments are legitimately sovereign over those spheres of action protected by their right to rule. Provided they respect the limits of their right to rule, governments do not therefore infringe their residents’ rights to autonomy by engaging in policy making. (MacKay and Chakrabarti 2019: 5)

As they acknowledge, this assumes ‘governments and their residents possess mutually exclusive spheres of sovereignty’. This reasoning is manifest in the Belmont Report (National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research 1978), which waives the requirement for informed consent in experiments conducted by the United States government.

Yet this is a thin model of democracy. It implies that democratic participation should be limited to sporadic voting, and is consistent with autocratic governance, so long as autocracy is limited to the period

between elections. It also assumes that an elected government is viewed with equal legitimacy by all residents. But the legitimacy of a government is in part a function of the ways in which it treats its residents. The Belmont Report was published just six years after the Tuskegee Study of Untreated Syphilis on unconsenting black men concluded (Washington 2006). It was followed a decade later by a government-funded trial which forcibly administered a lethal drug to black and Latino orphans who were HIV positive. The trial only concluded in 2001 (Yearby 2016). A blanket assertion about the legitimacy of a government, and its sovereign right to withhold consent, elides struggles over who has the right to be treated as human. In doing so, it fails to take into account the harms a government might inflict on people it implicitly deems to be less than human.

The claim that governments and their residents inhabit mutually exclusive spheres of sovereignty is also at odds with the everyday practice of democratic governance. Governments plausibly derive their legitimacy in part from their openness to residents' participation in policymaking and implementation. This includes consultation with residents regarding prospective law making, residents actively participating in and sometimes resisting law-making through legal challenges and political action, and residents monitoring and evaluating policy implementation in order to hold government to account. These activities are all tied to the spirit of informed consent. Making this principle explicit is not at odds with a government's right to rule. On the contrary, it would appear to embody it.

However, this does not imply a dogmatic and unyielding insistence

on the principle of informed consent. To do so would be to deny historical context and unequal power relations. A government raising taxes on the wealthy is a different matter from the government raising taxes on the poor. We might with reason believe that the protestations of the wealthy have a less legitimate claim on the government than the complaints of the poor, since the former fight for privilege, while the latter fight to survive. Similarly, it is common to relax the requirements for informed consent when conducting experiments to investigate powerful actors who engage in unethical or illegal behaviour, such as racial discrimination or money-laundering (Findley and Nielson 2015).

In both research and governance then, informed consent ideally functions as a baseline principle for protecting the most vulnerable and can only be waived with strong contextual justification. In governance especially, it is a constitutive element of legitimate rule – it is the thread used to stitch together the social compact between government and its residents, out of which a democratic social policy is woven.

Considered carefully then, the violation of informed consent in social experiments is incoherent. Social experiments manifestly aim to make a positive contribution to social policy. Yet, the violation of informed consent undermines the realisation of a democratic social policy. This is the political argument against involuntary experimentation: that it constitutes a *prima facie* threat to democratic social policy.

The case for a moratorium

All of the experiments in the systematic review were published in prestigious journals, and while

none of them mentioned any form of ethical review, they were presumably approved by their universities' institutional review boards. This suggests that the mechanisms for regulating social experimentation are ineffective.

Weak regulation is likely a function of the political economy of social experimentation. Over the last two decades, there has been a dramatic increase in the use of experiments to evaluate the outcomes of social policy interventions in former colonies. One of the key drivers of this increase is the Abdul Latif Jameel Poverty Action Lab (J-PAL), which was founded by two of the 2019 Nobel laureates, Esther Duflo and Abhijit Banerjee. Since its inception in 2003, J-PAL has posted 876 social experiments in 80 countries, where the largest proportion were conducted in African countries (Jatteau 2018).

While J-PAL is not transparent about its finances, by some estimates it received around US\$300 million between 2003 and 2018 (Servet 2018). This funding comes from a range of institutions such as the World Bank, the UK's Department for International Development and the Gates Foundation. And J-PAL's footprint is set to grow with the Nobel Prize, which has not only served to deepen the prestige of social experimentation in general and their institute in particular, but has also allowed the laureates to source an additional US\$50 million in donor funding to extend J-PAL's programme worldwide to institutions and researchers in former colonies (Kremer 2019).

Indeed, it appears that J-PAL has been influential in the World Bank, which has been a key driver of social experimentation, as both a project and research funder, and as a thinktank. In 2005, the

Bank commissioned a research evaluation headed by Banerjee, which condemned the Bank's projects for lacking rigorous impact evaluation (cited in Jatteau 2018). This view was echoed by the Evaluation Gap Working Group (2006), which included authors from J-PAL, and development actors, such as the World Bank and the Gates Foundation. That same year, the World Bank established a dedicated impact evaluation unit (DIME) composed of former J-PAL associates to conduct RCTs. The number of RCTs used in World Bank evaluations subsequently increased from a baseline of zero in the year 2000 to just over two-thirds of all evaluations in 2010 (Bédécarrats, Guérin and Roubaud 2019).

This shift in World Bank policy has accompanied changes more generally in international development policy to focus on results-based management. These guidelines were formulated in the 2005 Paris Declaration on Aid Effectiveness and reiterated by all the major conferences on official development assistance in Accra in 2008, Busan in 2011 and Addis Ababa in 2015 (Bédécarrats, Guérin and Roubaud 2019). Since then, a number of dedicated RCT funding agencies have been established. The Strategic Impact Evaluation Fund was founded in 2007, the Global Agriculture and Food Security Program in 2009, and the Impact Evaluation to Development Impact in 2014.

This suggests that social experimentation has rapidly become a multinational enterprise, one with significant financial and political interests. It is also a high prestige activity, one with ivy league universities in the United States at the centre of the research network (Jatteau 2016). The combination of these

factors has likely helped J-PAL develop a model of policy influence that focuses on driving demand by 'co-creating' experiments with governments, NGOs and funders (Gyamfi and Park 2019). As a result, key institutions, which might have held experimentalists accountable, are no longer at arm's length from the research and their will to enforce ethics may be undermined by a conflict of interests (Hoffmann 2018).

Given these constraints, existing models for regulating experiments are likely incapable of being effective. It is difficult for national entities to regulate multinational industries. It is not easy for poor countries or universities to veto unethical experiments by donors or wealthy Northern universities. And it is challenging to make the case for caution in an international policy context enthusiastically advocating experimentation as *the* gold standard. As such, ensuring ethical experimentation will likely require new models of regulation, which involve Southern scholars and governments working collaboratively.

It is within this context that the experimental economist Sarin (2019) has urged the 2019 Nobel laureates to call for halting all experiments on vulnerable people until effective ethical safeguards are established. This is an important intervention, but it fails to account for the responsibilities of Southern scholars to our societies and elides the role that Southern scholars have played in enabling unethical experimentation. It is our responsibility to insist that experiments in our societies follow rigorous ethical protocols, and we should be at the forefront of ensuring this is enforced. This does not imply that Northern scholars have no responsibility to prevent

unethical experimentation, but it is with our own conduct that I am concerned.

The prospects for more effective regulation of social experiments look bright in comparison to medical experimentation. According to one estimate, between 2007 and 2017, 360 million people participated in a registered clinical trial (Narita 2019). In comparison, only 22 million people were enrolled in social experiments (in disciplines such as economics, political science, and psychology). The sheer scale of medical experimentation suggests that there are substantially greater financial and political obstacles to effective regulation when compared with social experimentation.

In light of this, I believe Southern scholars have three clear responsibilities. First, we have a duty to call for a moratorium on experimentation until effective regulatory mechanisms are established. Second, we have an obligation to understand the constraints on effective regulation. In this regard, we have much to learn from medical scholars, who have long grappled with unethical experimentation. And third, we have a responsibility to resist unethical experimentation and participate in establishing effective ethical safeguards. These social responsibilities flow from our intellectual freedoms, as CODESRIA's community has long-recognised (Diouf and Mamdani 1994). For intellectual freedom is not merely a negative freedom from constraints. It is also a positive freedom to serve. Defining and enforcing the proper bounds of social experimentation is crucial to upholding the dignity of some of our most vulnerable people, reducing the risk of harm, and mitigating continuities with colonial experimentation. It is also a small,

but important step in reclaiming the intellectual project of democratic social policy, and indeed, as Chelwa and Muller argue in this issue, the broader intellectual project of development.

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Randomised Trials as a Dead-end for African Development

Randomised controlled trials (RCTs) are supposedly an important tool in reducing world poverty and contributing to economic and social development. On close scrutiny, the basis for that claim turns out to be remarkably weak. Worse, and in marked contrast to the hype about this methodological approach, there are substantive reasons to believe that the use of RCTs could in fact be *harmful* to the prospects and well-being of Africans and African countries.

This short article elaborates on these concerns as follows.¹ The first section discusses some fundamental methodological limitations of RCTs and the resultant intellectual inconsistency of the proponents of this approach. This shows how the randomista project *is flawed on its own terms*. The second section then discusses how pre-

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existing views and biases, whether ideological or about how social and economic processes work, contradict the framing of RCTs as a neutral scientific endeavour. The final section briefly illustrates these arguments with two important examples from South Africa where, I suggest, the randomista approach has done, and continues to do, significant harm.

An overview of methodological limitations

RCTs are a method for obtaining quantitative estimates of causal

effects, and their use for drug trials in medical contexts is well-known. The deployment of RCTs to address social and economic questions is not straightforward and may even be unethical in many cases (see Hoffmann, this issue). Beyond that, however, one could argue that as a different methodological emphasis in intellectual inquiry, RCTs are 'mostly harmless' (Angrist and Pischke 2009). But what is characteristic of the dramatic increase in the use of this method in economics are assertions of methodological superiority in the policy realm and a deliberate effort to obtain influence (Banerjee 2007; Banerjee and Duflo 2009; 2011). It is this latter project that was recently awarded the 2019 Nobel Prize in Economics (Nobel Media 2019) and the proponents of which I refer to, following others (Ravallion 2009; 2018; Deaton 2010), as 'randomistas'.

The basic argument of the randomista project goes something like this:

1. We need reliable, quantitative estimates of causal effects to make the right policy decisions.
2. The assumptions required by other econometric methods to obtain such estimates are implausible.
3. Simple analysis using an RCT can identify the effects of policy interest without requiring prior knowledge.
4. Therefore, RCTs herald a ‘credibility revolution’ (Angrist and Pischke 2010) in economics and should be prioritised by policymakers seeking simple answers to important questions.

Every component of the argument is contested and has been the subject of substantive criticism, but for present purposes I focus on one fatal contradiction at the core of advocating RCTs for development policy.

Even if RCTs do actually identify causal effects, direct policy relevance requires going from an empirical finding in an experimental population to recommending an intervention in a broader population that is the one policymakers are concerned with.² And the main obstacle to doing so is that the causal effect of an intervention rarely, if ever, exists in isolation: it depends on the characteristics of people and context in which it is implemented. Or in other words: the causal effect of an intervention depends on its *interaction* with other factors. Those factors may vary across time as well as geographical space, so that even in the experimental population there would be a different outcome five years later.

While this is quite intuitive, and was pointed out by the first authors to systematically consider the problem in relation to social experiments (Cook and Campbell 1979), it presents a serious dilemma for randomistas. If the causal effect depends on other factors, then to extrapolate it to a different population requires knowing what the relevant factors are and having data to see how they differ across the two populations. But the methodological argument for RCTs is that assumptions about causal structure, used by non-experimental methods to obtain supposedly causal estimates, are not credible.

This argument can be formalised to show (Muller 2015) that by simply asserting, rather than establishing, that the results of RCTs apply to broader populations the randomistas endorse an assumption with the exact same structure as the one they reject when it comes to non-experimental methods. At best this is intellectually inconsistent, at worst it is fundamentally dishonest – either way it constitutes a fatal flaw at the heart of the randomista project (Muller 2020).

Although the extent of the problem has not been adequately acknowledged – in the sense of refraining from making policy recommendations or promises about policy relevance – some indirect solutions have been proposed.³ One of these of particular interest is that practitioners of randomised trials use their ‘expertise’ to assess when/whether experimental results can be applied in other populations. The invocation of qualitative expertise, not least within a broader stance of imitative scientism, will surely interest many readers who are familiar with the historical disdain shown by economists towards qualitative methods and claims

based on individual expertise rather than ‘data’, ‘models’ and ‘econometrics’. But it also does nothing to resolve the intellectual contradiction, since if qualitative expertise can be used to decide prospects of extrapolation, why could it not also be used to decide prospects of identifying causal effects? Doing so would render not just RCTs redundant but econometric methods more broadly.

Proposing the qualitative expertise of randomistas as a solution to the extrapolation problem also draws attention to another dangerous characteristic of their project: not only are RCTs placed at the top of a hierarchy of evidence, but randomistas seek to place themselves at the top of a hierarchy of knowledge (an ‘epistemic hierarchy’).

Of course, the privileging of economists’ views over others with arguably greater expertise is not a new phenomenon – in the past it has led to accusations of ‘economics imperialism’. However, the promise of simple answers based on ‘scientific experiments’ combined with a well-funded push for influence marks the randomista project out as even worse in this respect than its predecessors.

Ideologically-infused experiments

Being at the top of a hierarchy of expertise is a sure way of securing policy influence, but there is no *prima facie* reason to believe that an American academic running an RCT has more knowledge of a local health system than, for instance, a competent doctor who has worked in that system for decades. Linked to this is a further problem with RCTs that has received little attention, but is profoundly important for developing countries seeking to

determine appropriate strategies and trajectories for social and economic development.⁴ The problem is this: *the very choice of an intervention on which to base an RCT is itself the result of a pre-existing conception of how the world works and how it should work.*

Consider the following example. A policymaker in the Busia district in Kenya is concerned about learning outcomes in state schools because a relatively small proportion of children attain basic competency in literacy and numeracy. The policymaker asks a randomista for assistance in improving outcomes and the randomista, naturally, proposes that an RCT, or multiple RCTs, be run to establish ‘what works’.⁵ But where does the intervention that will form the basis for the RCT come from?

One answer might be to say: ‘let us try something that appears to have worked elsewhere’. But this begs the question, since under the full absurdity of the randomista approach nothing can be said to have worked unless that is verified by the results of an RCT. Thus in the base case the randomista must draw an intervention from the set of interventions *they believe might work*.⁶

From a purely methodological perspective this is interesting because, as noted above, the methodological motive for the randomista project arose from scepticism of *ex ante* causal knowledge. Yet the mere choice of an intervention imposes researcher beliefs in at least three respects:

1. In determining the set of interventions that may work in theory.
2. Determining the subset of 1 that are considered practically feasible.

3. Prioritising the possibilities in order to select a single intervention or, at best, a handful of interventions.

To focus on our chosen example, suppose that either the policymaker or the researcher has evidence of high teacher absenteeism and this is deemed to be a likely cause of poor outcomes. What experimental intervention might one institute? Researcher A who considers public employees in developing countries to be inherently lazy may favour a punitive incentive system based on increased monitoring. If that is practically infeasible because of resistance from teachers or other stakeholders, a reward-based system may be the next best option. On the other hand, Researcher B – who believes that under-resourcing and low-quality work environments negatively affect motivation – may propose an intervention that substantially increases school resources.

Notice that each researcher’s preferred experimental intervention may not even be in their counterpart’s set of possibly, or theoretically, effective interventions. That will also carry over to interpretation of the results of any RCT. Researcher A will interpret RCT evidence of ineffectiveness of a resource-based intervention as merely confirming what they already expected, while Researcher B may interpret it as reflecting the fact that increases in school resources may take time to have an impact. Similarly, B will interpret ineffectiveness of an incentive-based intervention as reflecting the fact that absenteeism is caused by other factors, while A may interpret it as reflecting a need to alter the design of the incentive mechanism.

The broader point is that there is nothing neutral about RCTs: the

interventions chosen for testing are the outcome of decisions by researchers conducting experiments and institutions funding them, and will therefore reflect their preconceived notions of how the world works and what solutions should be considered plausible.

Two examples from South Africa

Two specific examples from South Africa illustrate the salience of these arguments and the dangers of the randomista project for developing countries: the misleading use of an RCT to make the case for an employment tax incentive; and, the contribution of RCTs and their proponents to the continued neglect of systemic contributors to poor educational outcomes. Both examples are discussed extensively in separate articles.

The employment tax incentive: using an RCT to distort the policy debate

In the mid-2000s the South African government invited a group of economists, subsequently known as ‘the Harvard Group’, to advise on the country’s economic policy (Center for International Development 2008). One proposal that emanated from this initiative was for an employment tax incentive aimed at reducing the extraordinarily high national unemployment rate (Levinsohn 2008). Underlying the proposal was a conceptualisation of unemployment as resulting, at least to a significant degree, from the price of labour being too high. That view had long been contested by trade unions, leading to a polarised situation involving academics siding with different vested interests: one side framed unions as seeking to privilege their members at the expense of the unemployed,

while the other side framed business as seeking to destroy collective action in order to better exploit workers. Unsurprisingly, the proposed tax incentive was opposed by trade unions.

The original analysis that had proposed the incentive acknowledged that the question of how responsive employment is to wages is an empirical one and that therefore more evidence was needed to substantiate any incentive and determine its characteristics. Although there was already some evidence that the National Treasury and many academics involved believed that an incentive was desirable, two studies were conducted in order to inform the decision: one was a computable general equilibrium (CGE) analysis (Burns et al. 2010) and the other a randomised trial of a wage subsidy voucher given to job seekers.

The nature of CGE studies is such that they effectively *assume the answer to the primary question* ('would a publicly funded reduction in the wage causally increase employment?') and model the sensitivity of outcomes to other assumptions; in that sense they are rather uninteresting, and unhelpful, for making the main policy decision, and I do not discuss that work further here.

The randomised trial was conducted by academics with links to the National Treasury and funding support from 3ie, which along with the Jamaal Abdul Latif Poverty Action Lab (J-PAL) and Poverty Action, is one of the main international organisations funding the use of RCTs for development research. The 'policy influence plan' submitted to the funder (Unknown 2011) shows that the researchers anticipated unions as an obstacle to the impact of their

findings – clearly expecting a positive result.

The working paper with the details of the study and its findings was only published after Parliament had approved the Employment Tax Incentive Bill (Levinsohn et al. 2014). However, prior to the decision the local researcher running the experiment published a number of articles in the popular press arguing for adoption of the incentive based on the positive findings of the study (Rankin 2012; 2013). The National Treasury also cited the study in its presentation to Parliament. Yet parts of the full working paper that was published later are more cautious about what can be claimed, and scrutiny of the study details shows that the RCT provides little, if any, insight into the core policy question.

Among the reasons why the claim that the RCT findings supported the implementation of the national incentive was false are: that the voucher intervention bore little resemblance to the intended incentive; the experimental population was not nationally representative; additional evidence did not support the claim that a lower wage was the mechanism behind the higher employment rate of voucher holders; and any positive effect could have been the consequence of a competitive effect among workers that would disappear when the intervention was scaled-up.

This example illustrates the points made in the preceding sections. The policy claims based on the RCT were not appropriate given the study's limitations. Furthermore, the researchers showed clear bias in favour of the policy. One even worked for a consultancy company that provided services to labour brokers who would benefit directly from the incentive. Yet by

leveraging the dubious scientism and epistemic authority associated with the randomista project, an RCT was used to endorse a policy that committed the government to billions of Rands of tax incentives for the private sector at a time when it was implementing fiscal consolidation.

RCTs and selective denial of systemic contributors to poor educational outcomes

Many of the studies cited in the 2019 Nobel award concern educational experiments. The vast majority of these are concerned with interventions that either do not materially increase resources available to schools, or – as in the hiring of low paid contract teachers – do so in a manner that undermines the wages or power of incumbent teachers. This follows a longer tradition in the economics of education of denying or downplaying the relevance of fiscal resources (on the basis, incidentally, of econometric findings that are not credible by randomista standards).

Given that South African academic economics is largely an imitative enterprise (Muller 2017), it is unsurprising that both these stances have been reproduced locally. The researchers who currently dominate this policy space in South Africa produced a report on 'binding constraints in education' that did not list resources as a binding constraint (van der Berg et al. 2016). This in a country regularly ranked the most unequal in the world, with high rates of unemployment, poverty and violence, and an education system for black South Africans that until 1994 was infamously designed to produce 'hewers of wood and drawers of water'.

The justification for this remarkably extreme position is two-fold. First, one of the authors previously claimed that South African education expenditure was high relative to other countries (van der Berg 2007) and therefore resources could not be a cause of poor outcomes. Second, in studies done using non-experimental econometric methods the authors and their collaborators apparently failed to find evidence that resources had a significant impact on outcomes. The view that resources are unimportant dovetails with a negative view of teachers, school management and trade unions: that it is not the inequities bequeathed by apartheid that cause poor educational outcomes, but merely inefficient management of adequate resources.

RCTs fit neatly into this stance since, as has been the case internationally, they focus attention on non-structural issues, resource optimisation and deficit models of developing country civil servants. Unsurprisingly then, these researchers and their similarly-minded collaborators in the Department of Basic Education have taken enthusiastically to these methods (even though acceptance of the randomista claim about credibility would render much of their prior work non-credible). Examples include poorly thought-out interventions such as randomly sending study guides to schools in one province and then fishing for statistical significance (Taylor and Watson 2015), along with somewhat more carefully considered larger-scale projects to test teacher training or early grade reading interventions.

While the small group of researchers conducting these studies claim that the extrapolation problem is '[not] serious enough to call the method into question' (Fleisch et al. 2017:

10) it is evident that they do not have a grasp of the fundamental problem outlined above. It perhaps bears mentioning that given the current enthusiasm for RCTs, adopting this research method serves both the researchers' academic publishing aspirations and desire for policy influence, regardless of whether it serves the public interest.

What has been particularly striking about the recent turn to RCTs in South African basic education policy is that educationalists and civil society activists who championed reading interventions for decades were ignored by government. But economists with little, if any, direct knowledge of the education system who advocated early grade reading interventions on the back of 'scientific' experiments rapidly got to the point of having their recommendations reproduced word-for-word in the President's State of the Nation Address.⁷

Linked to this is how researchers who enhance their epistemic status using RCTs are also given more authority to inform policy using different methods to address separate policy questions on which other researchers may have greater, long-standing knowledge and expertise. This further compounds an arguably distorted epistemic hierarchy in which academics and others who draw expertise from research are given almost all the weight assigned to non-political inputs while 'experts of practice' – such as teachers – are ignored except to the extent that their expertise is filtered through the former's research, analysis and anecdote. And all of this, as with the employment tax incentive RCT that misled the public and policymakers, occurs under a broader narrative of 'evidence-based policy'.

RCTs as a dead-end

The main problem with the randomista project for those concerned with development, then, is not its methodological intolerance (Harrison 2013) *per se*. It is that an undue emphasis on RCTs for policy purposes is methodologically unsubstantiated, smuggles in ideological and epistemic bias, distracts from important questions and in doing all this diverts scarce intellectual resources and political will toward projects that will rarely deliver on even their narrow promises. The randomistas appear to be driven by a 'missionary zeal' (Bardhan 2013) that they are the chosen ones to save the denizens of developing countries from poverty with an 'incredible certitude' (Manski 2011) about their findings that is not warranted. And the combination poses a real danger to developing countries that have limited resources to resist a well-resourced project to determine their policies.

Compounding this is that, as noted by many critics, the randomista project focuses both research and policy on narrow questions and interventions that lend themselves to RCTs, rather than on those that are most important for developing countries. The deliberate pursuit of medium and long-term structural change through a process of learning that has characterised the development paths of most now-wealthy nations is outside the scope of the randomista project (see Chelwa, this issue). So, while there has been an attempt by randomistas to frame their stance as one of hope rather than pessimism regarding the prospects of major improvements in developing countries, that is disingenuous. The randomista project is premised, mostly implicitly but occasionally

explicitly, on a fundamental pessimism about developing countries achieving the economic improvements of their predecessors.

Although the connections need to be elaborated in more detail, the randomista project can be seen as an extreme manifestation of imperialistic tendencies among economists premised on dubious claims about economics as a scientific activity. In that light, it is notable that even those who have endorsed the proclamation of a ‘credibility revolution’ have been silent about what this implies for all past policy advice by economists. If indeed it is true that RCTs are required for credible causal estimates then all past policy claims by economists using the implausible assumptions of other methods must have been inappropriate at best, or harmful at worst. The zealotry of the randomista project manifests itself in the argument that the hubris of economists that preceded it was fundamentally flawed but ‘this time is different’. Close scrutiny of the project suggests otherwise: ‘this time is worse’.

For all the above reasons, I adopt a stronger stance than many critics of RCTs: not only will the widespread adoption and reliance on this method fail to yield the benefits promised by the randomista project, it is likely to *hinder* the attainment of long-term improvements in the prospects and well-being of the residents of developing countries. Properly located methodologically and epistemically, RCTs would play at most a small role in informing policy decisions of developing countries. If given the authority and power sought by the randomistas, RCTs will be a dead-end for African development. Whatever factors have hindered the attainment of greater progress in African countries since

independence, there is no reason to believe that RCTs will address, or circumvent, those. The challenge for African countries remains to set out, as other countries have done historically, an alternative path to the new missionary complex that has congealed around the randomista project.

Notes

1. For the sake of brevity and exposition I keep references to a minimum; more extensive references can be found in other work on which the present paper is based (Muller 2014b; 2014a; 2015; 2020) and shorter versions of some of these arguments can be found in Chelwa and Muller (2019) and Chelwa, Hoffmann and Muller (2019a; 2019b).
2. This problem is widely referred to as the problem of ‘external validity’, following Cook and Campbell (1979) who contrasted it with the problem of identifying a causal effect (‘internal validity’). It is also referred to as the ‘generalisability’ or ‘transportability’ problem.
3. In a forthcoming book chapter (Muller forthcoming) I discuss a range of efforts to address the problem, including replication and machine learning, and explain why they are inadequate.
4. There are some cogent critiques of popular conceptualisations of the notion of ‘development’ but I use the term in a broad, relatively unobjectionable manner here to refer to improvement in the well-being and prospects of those within a country – without requiring any particular presumption of what improvement might mean.
5. If the randomistas were to propose some other approach, presumably they would also need to suggest that someone else, who is actually an expert in that approach, be consulted.
6. In some places, randomistas have made much of their consultation with local partners in deciding what intervention to test. Aside from the fact that there is little independent evidence of this, it is quite clear even in such accounts that the researchers

do not agree to run interventions that they believe are unlikely to be effective. Furthermore, the nature of, and rationale for, the vast majority of interventions is evidently economic in nature.

7. And it is perhaps no coincidence that the dominant demographic in the former group were black women while the latter are predominantly white men trained at the university which was the intellectual heart of apartheid.

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Climate Services for Smallholder Farmers Using Mobile Phones: Evidence from a Pilot Randomised Controlled Trial in North Benin

Introduction

Climate services are broadly acknowledged to have the potential to support decision-making and improve resilience to climatic shocks. Nevertheless, providing such services comes with several challenges such as the format, timing, costs, etc. In agriculture, climate services can help farmers to take informed production decisions such as the best timing of farming activities (e.g. sowing or planting and application of fertilisers or pesticides), type of seeds to use, etc. Despite this importance, there is limited high-quality and rigorous evidence on how climate information could be provided to smallholder farmers. Against this backdrop, we tested the impact of climate services for smallholder farmers using mobile phones. We conducted a pilot theory-based experiment, using a randomised controlled trial (RCT) design that involved a treatment group and a control group with randomisation at the village level. Farmers in the treatment group were provided with weather information through a mobile phone Short Message Service (SMS). We used the exogenous variation created by the random assignment to estimate the impact of climate services on the farmers' production decisions and performance.

In sub-Saharan Africa smallholder farming systems that rely on rain-

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fed agriculture remain the main source of livelihoods and food for most of the population. Changes in rainfall and temperature patterns are affecting agro-climatic conditions with important alterations in the growing seasons (Ngaira 2007; Waha et al. 2013), the planting and harvesting calendars (Rosegrant et al. 2008; Waha et al. 2013), and processes such as evapotranspiration, photosynthesis and biomass production (Rosegrant et al. 2008). It is projected that crop yields in West Africa for instance might fall by about 10 to 20 per cent by 2050 due to climate change (Thornton et al. 2002). Net crop revenue could fall further by about 90 per cent by 2100 (Boko et al. 2007). These impacts will exacerbate both food insecurity and poverty issues.

Considering the reduction of climate change impacts, adaptation is now recognised as a key policy option (Kurukulasuriya and Mendelsohn 2008). In agriculture, farmers currently use several adaptation strategies that are well documented in the literature. Common strategies include crop

diversification, the use of short cycle or drought-tolerant seed varieties, crop rotation and farming techniques such as adjustments of the timing of farm operations and the dosages of fertilisers (Abid, Schneider and Scheffran 2016; Assan et al. 2018; Below et al. 2012; Bryan et al. 2009; Hassan and Nhemachena 2008; Hisali, Birungi and Buyinza 2011; Shepherd and Godwell 2019; Twagiramaria et al. 2017; Yegbemey et al. 2013). Yet, the lack of adaptive capacities is one of the major limiting factors in smallholder farming systems (Waongo, Laux and Kunstmann 2015). A good illustration is the lack of relevant climate-related information to inform adaptation decisions. At the scale of the production systems, farmers typically shape their adaptive response to climate change based on their past weather knowledge and experience that form their expectations for future weather. While we strongly acknowledge the importance of farmers' experience and endogenous knowledge, we argue that traditional weather forecast knowledge systems are now challenged with higher and higher levels of uncertainty about future variability. Previous studies (e.g. Roudier et al. 2014; Yegbemey et al. 2014) found that providing farmers with relevant climate information is likely to help them to (better) shape their adaptive response. According to Douxchamps et al. (2016),

adaptation strategies to reduce smallholder farmers' vulnerability to climate variability and seasonality are particularly needed in West Africa. However, there is still a paucity of policy-oriented research exploring innovative interventions to provide smallholder farmers with climate services.

Within the framework of CODESRIA's Making Research Initiative (MRI), we were awarded a research grant (MRI/CTR 7/2017) to conduct a study to explore *ex ante* the impact pathways of a hypothetical intervention which consists in providing smallholder farmers with weather-related information. Additionally, we design a pilot field experiment (i.e. an RCT) to analyse quantitatively the impact of weather forecasts (provided to smallholder farmers through a mobile phone SMS) on self-reported labour costs, yield and income. Our experiment was recently registered with the RCT ID AEARCTR-0005039 in the American Economic Association's registry for RCTs. It is important to note that we wrote two research papers based on the current projects. Both papers are under review for publication by CODESRIA.

Research design

Our intervention consisted in providing climate-related information through mobile phone SMS. Our targets are maize farmers that own a mobile phone and can read French or have someone in their household who can read French. The intervention was implemented by a local NGO, Bureau de Recherche et de Développement en Agriculture (BReDA). Using a mixed-methods approach, we designed a pilot theory-based RCT to test the impact of climate services for smallholder farmers

on their production decisions (i.e. labour allocation) and performance (i.e. yield and income). RCTs are experimental approaches viewed as the most rigorous method to estimate the impact of an intervention when both internal and external validities are met. In a typical RCT, some people/units are allocated at random (by chance only) to receive the intervention whereas some people/units are also allocated at random to not receive the intervention. The former group of people is the treatment group and the latter group is the comparison or control group. The impact is assessed by comparing the average change in the outcome variables of interest (i.e. labour allocation, yield and farm income in our study) between the treatment and control groups.

We conducted field work in six villages of the municipal area of Bembèrèkè in North Benin, West Africa. Villages were selected so that they are similar in terms of the importance of maize farming, production systems, maize production, average farm size, etc. To ensure this, agricultural extension officers were involved in the selection process. Furthermore, a field exploration visit was organised to confirm that the selected villages are actually similar. Following our RCT design, three villages (clusters) were randomly assigned to the treatment group (treatment villages) and the other three to the control group (control villages). Randomisation was conducted through a public lottery attended by representatives from all six villages. A total of 331 eligible and volunteer maize producers were randomly selected in the six villages. Farmers eligibility criteria include: a) farmers should be maize producers, b) farmers should plan to produce maize during the rainy

season of 2017–18) farmers should own a mobile phone with a valid and functional line number, and d) farmers should have the ability to operate (i.e. read SMS) their mobile phone or have someone in the household who can do so. Following our design, farmers in villages assigned to the treatment group received the intervention whereas farmers in villages assigned to the control group received no intervention.

We conducted a baseline survey and an endline survey before and after the intervention respectively. In addition, we conducted a total of seven monthly follow-up surveys to collect monitoring data. Each data collection was designed as a household survey based on semi-structured interviews, using a questionnaire pre-programmed in KoboCollect. Primary data collected include: a) farmers' socio-economic characteristics such as location, gender, age, level of education, household size, main and secondary activities, contact with an extension agent, access to credit, etc.; b) treatment status (i.e. treatment versus control); c) production decisions such as inputs allocation; and d) inputs and output quantities and prices. Before the baseline survey, we conducted an extensive qualitative survey to understand better the possible impact pathways of the intervention.

Impact pathways of weather information for smallholder farmers

We used a qualitative research design based on focus group discussions with smallholder farmers and agricultural extension officers to build a Theory of Change (ToC) of our intervention. By definition, a ToC is a description of

how a desired change is expected to happen in a particular context due to the intervention of interest. Our results support the premise that climate services have the potential to help farmers in taking informed production decisions. More specifically, we find that providing farmers with weather-related information can help them better allocate production resources and eventually record higher yields and incomes. Farmers who enjoy these impacts might end up having better lives through improvements in their livelihoods. Our study suggests that several types of weather-related information can be useful for smallholder farmers. These include rainfall and wind forecasts. There are also several dissemination channels that can be used to provide farmers with climate information, ranging from the social network of the local communities, to information and communication technologies. We show that each dissemination channel comes with both strengths and weaknesses. In that respect, we argue that the best dissemination channel will depend largely on the socio-economic context of the intervention area. Regardless of the socio-economic context of the intervention area, weather-related information needs to be accurate, available in a timely manner, understandable, and easy to use by smallholder farmers.

Impact of weather-related information on labour costs, productivity and farm income

Thanks to our pilot field experimental design, we compared the self-reported labour costs, yield and income between treatment and control farmers. Following the ToC of our intervention, we expect that farmers provided with weather-related information will better allocate their production

resources and therefore record higher agricultural outputs. We acknowledge that our sample size is rather small and to account for this, we used three regression specifications: Ordinary Least Squares (OLS); Generalized Estimating Equations (GEE) model with small sample correction; and Randomisation Inference (RI). The balance tests on the outcome variables and key co-variables at baseline show that the control and treatment groups are well balanced. Our impact estimates suggest that farmers in the treatment group record a lower level of labour costs but higher levels of yield and income. These patterns are consistent with our theoretical expectations. Furthermore, both the signs and the values of the impact estimates are consistent across the three regression specifications but significant with the RI model only (for labour costs and yield) or with the RI and GEE models (for income).

Conclusion

There are several adaptation strategies mostly developed by smallholder farmers themselves or introduced by development agencies, government and/or research institutions. These include new seed varieties, crop diversification, adjustments of the farming calendar, changes in input allocations and off-farm activities. While these adaptations could help adjustment to clear long-term changes in climate, they can show limitations as far as day-to-day weather variability is concerned. Considering that agricultural production is mostly seasonal, we acknowledge that adaptation to climate change is vital for smallholder farmers but adaptation to more uncertain climate variability is urgent.

Through climate services, smallholder farmers can have access to weather information. Then, they can use the information to adjust farming practices. As this will be new to most rural areas, initiatives should engage with farmers to inform the main features of climate services such as the content, language, communication channels, timing, etc. As a matter of fact, our findings suggest that there are several options to design climate services and each option has strengths and weaknesses.

Our field experiment shows that weather-related information through mobile phone SMS has positive impacts on labour, yield and income. Despite the pilot nature of our experiment, the findings will encourage researchers, practitioners and policy makers in their efforts to design and offer climate services for smallholder farmers. Yet, larger experiments are expected to generate more rigorous and high-quality evidence on the impact of climate services.

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Trust, Legitimacy and Community Perceptions on Randomisation of Cash Transfers

Randomised controlled trials (RCTs) are now increasingly used in social policy and development as methods for identifying causal relationships. The recent win of the Nobel Prize in Economics by three development economists working on RCTs indicates ‘the rise of the evaluations and an acknowledgement for the approach to alleviating poverty’. Growing from the fields of medicine and clinical sciences, RCTs are now considered ‘the gold standard’ for evaluation on matters development. As governments and international organisations seek ways to understand causal questions related to development (Chelwa, Muller and Hoffmann 2019a), so has the popularity of RCTs grown. Developing countries constitute the bulk of where development economists and non-governmental organisations carry out experimental evaluations. In Kenya, international organisations including the Abdul Latif Jameel Poverty Action Lab (J-PAL), an NGO associated with the Nobelists, have conducted a number of RCTs in rural and poor urban centres. The Busara Center for Behavioral Economics, a research and advisory firm, is another institution with offices in Kenya conducting RCTs in Africa.

With increased enthusiasm for experimental models, questions and criticisms abound. It is unclear for example how RCTs solve the

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problems of poverty, a claim made during the presentation of the 2019 Nobel Prize. Also, it remains unclear how governments adopt or scale-up policy prescriptions arising from RCTs to national level. Other areas of criticisms include levels of informed consent (see Hoffmann, this issue), conflicts of interest (Hoffmann 2018), and on methodological limitations of the experiments (see Muller 2015). Another level of criticism of RCTs involves the perception of participants in the experiments, an area to which little attention has been paid.

Between 2014 and 2016, GiveDirectly, an American NGO, conducted a randomised experiment which involved giving lump-sum amounts of cash in Western Kenya in Homa Bay and Siaya counties. A one-off unconditional cash transfer of up to US\$1000 was paid to households in the counties through mobile money transfer. Unconditional cash transfers do not require households to perform certain specified behaviour to qualify for transfers. Households and individuals receiving the money

and are at liberty to use it as deemed appropriate. Governments and aid organisations are increasingly adopting cash transfers as poverty reduction instruments, and the use of cash in humanitarian situations in on the rise too. This essay derives from anecdotal sentiments expressed by community members in Oyugis in Homa Bay County, Kenya. On one of my visits to Oyugis, discussions were ongoing in the village about the programme. An aspect that struck me in the conversations was that community members were refusing to take cash from the organisation and urging others not to take money. Why would people refuse to take free money from GiveDirectly when as organisations reports ‘cash transfers have been thoroughly and rigorously shown to reduce poverty and improve lives’? GiveDirectly reports the refusal in a blog on their website thus; “As it turns out these challenges have been common for NGOs working in the area. Other development programs.... have also faced community resistance” (<https://www.givedirectly.org/refusals-in-kenya/>) suggesting that resistance is characteristic of that particular county and not to the programme. However, refusal to participate in such programmes is not peculiar to Homa Bay County, as GiveDirectly reports of a similar incidence in Malawi (see <https://www.givedirectly.org/why-one-village-refused-funds/>) where community members refused

to participate in a cash transfer experiment and only did so after the intervention of the local officials. Having previously worked in social protection and cash transfer policy spaces, my interest was to understand the perceptions of the community that had accepted government cash transfers on the one hand but on the other refused to take cash payments from GiveDirectly. Also, why would people reject large cash transfers, when GiveDirectly was claiming it was overwhelmingly beneficial?

At the fore of these discussions are the community's perception on randomisation, and themes of trust and legitimacy. The first section of the essay is a brief discussion of cash transfers and randomisation followed by a section on the community's perception of randomisation. The subsequent section is on perceptions of trust and legitimacy.

Cash transfer revolution and randomisation: a match made in heaven

Randomisation involves allocation of a treatment to some members of a group and comparing the results of the treatment with a control group – those not receiving the treatment – to determine the causal effect. Randomised experiments conducted in Kenya include a wide range of topics including the evaluation of teacher attendance, the allocation of study materials, the provision of mosquito nets, water treatment pills, and deworming, amongst others. With the rise of cash transfers up the global development agenda, a new frontier of experiments has opened up. The idea of giving cash to individuals and households – conditional or unconditional, through a means test or universal – is considered

revolutionary by its proponents. Cash transfers have been described by some as 'the silver bullet out of poverty' prompting their rise up the development discourse in the global South. The Government of Kenya with the United Nations Children Fund (UNICEF) initiated the Cash Transfer for Orphans and Vulnerable Children (CT-OVC) in 2003 as a response to the HIV/AIDS pandemic. Other cash transfer schemes, initiated with financial support and advocacy of international organisations, including the World Bank and the UK Department for International Development (DfID), are the Hunger Safety Net Programme, the Persons with Severe Disability Programme, and the Older Persons Cash Transfer Scheme. Other African countries have equally adopted cash transfers with each country on the continent now implementing a cash transfer scheme of some sort or other. However, most programmes are not homegrown but initiated through powerful advocacy, soft power and dominant structural mechanisms (Ouma and Adésinà 2019).

With the rise of cash transfers a fresh avenue for randomisation has opened up. Both RCTs and cash transfers are now hegemonic in social policy and development discourse in the global South driven by international organisations and national proponents. Randomisation of cash transfers is now used to evaluate a broad range of aspects from health and educational outcomes, girls' sexual debut, happiness and jealousy, to conflict and violence. As Hoffmann (2020) points out policy experiments are rooted in historical backgrounds of colonial experimentations in Africa. Both cash transfer and policy experiment proponents derive from the idea of the 'white saviour'

with international organisations claiming to provide solutions to the development challenges on the continent. With the popularity of randomisation of cash transfers the interaction of the two can only be described as a 'match made in heaven'.

Community perceptions on randomisation

By randomising, each member of a selected sample in the experiment has an equal chance of selection to receive treatment. To the community in Homa Bay County, the selection of beneficiaries seemed 'random' hence they could not understand how beneficiaries of the cash transfer were selected. Similar to findings on community perspectives from Zambia (Kombe et al., 2019) it was unclear to members of the community if the programme was aid or research. Stemming from the association of the provision of items and cash transfers as aid, there was confusion about the 'eligibility' of beneficiaries. The community's understanding is that cash transfer payments are made to poor and vulnerable households meeting certain criteria like orphanhood, disability or old age. In the community's understanding cash transfers are therefore provided to certain categories of people for their instrumental value of poverty alleviation and prevention of destitution. Expectations were that poverty and vulnerability would mark eligibility and not some 'random' criteria that enabled the selection of those considered ineligible according to the community's standards. The exclusion of those considered the poorest, such as street families, from cash transfers also heightened confusion over the objectives of the cash transfer experiment.

With lack of clarity on the eligibility criteria, members of the community attempted to contextualise the experiment. While selection into the government cash transfer schemes is somewhat clear, based on orphanhood, age and visible disability, with little clarity on the experiment by GiveDirectly, community members branded the programme a devil-worshipping enterprise. To the community, none of the considerations for eligibility for cash transfer made sense, as both the well-off and the poor were included and left out, providing room for speculation that beneficiaries were being enrolled into an evil scheme. It was expected that misfortune would befall those who accepted the money. And unlike in other experiments where those receiving provisions may have negative feelings of jealousy and envy, this was not the case with community members seeking to disassociate from those who had been enrolled in the scheme. In settings such as rural Western Kenya, where norms of neighbourliness, solidarity and community resilience remain strong, randomisation may be counteractive to these social norms.

On trust and legitimacy: ‘the government we know’

Besides the objective of the experiment, community members questioned the provision, in this case, money, and the amount paid out to beneficiaries. In some communities such as among the Luo (the people of Siaya and Homa Bay counties) money offered by strangers is treated with suspicion which further emboldened false speculations of devil worshipping and witchcraft. Often, a stranger giving money ‘just like that’ raises suspicions amongst people. Happenings, like houses burnt in

Siaya County, where others had received cash were linked to the cash transfer experiment. With rumours spreading about the ‘devil-worshipping’ outfit, community members sent word around telling others to reject the money. Distribution of goods often raises questions across communities and the distribution of money is bound to be more contentious especially when information is unclear.

Trust, or a lack thereof, was a factor in the experiment. Although native speakers of Dholuo formed part of the identification and registration team for the experiment, community members considered the organisation foreign. Asked why they would take money from government and not from GiveDirectly, a community member stated: ‘the government we know, but this organisation we do not know’. The statement demonstrates the mistrust community members have with ‘strangers’, be it for experiments or service provision. In addition, it points to the acknowledgement by community members of the role of state in social provision despite the roll-back and erosion of state capacity in social policy provision. While not all members of the community receive cash transfers from the state, the government is the agency they interact with in some form of the other – through law enforcement, collection of market tax, provision of educational and health services, or for relief through cash or in-kind transfers.

Manufacturing legitimacy – resources and expertise

With globalisation, policy spaces are now proliferated with more actors – both national and transnational. Along with the multiplicities of actors in the

policy arena are questions of legitimacy. Legitimacy concerns those mandated through law or norms to carry out certain activities and initiatives. National actors like politicians have veto powers mandating them to participate in policy processes while government works hold bureaucratic mandates. While international organisations, unlike domestic actors, may lack veto power or bureaucratic power to carry out some programmes, organisations like GiveDirectly draw their legitimacy from the resources they hold. In the case of the cash transfer experiment, the resources they bring into the policy space are finance and knowledge. Support and financial resources from heavyweight organisations in development like USAID, and the Bill and Melinda Gates Foundation provided additional legitimacy to GiveDirectly.

In addition, their international orientation from developed countries is perceived by some to be a marker of expertise. The resources they possess allow them to penetrate and bypass government bureaucracies to conduct experiments, engage in policymaking processes and even in some cases implement policies on their own with disregard for existing structures. Like other social policy experiments, GiveDirectly eschewed government structures and institutions and instead set up separate structures. Interaction and recognition of other government cash transfer schemes are minimal in papers or reports of the organisation. The idea of ‘directly giving cash’ is depicted as novel. The ‘creation’ of novel ideas further provides international organisations with the legitimacy of expertise.

Besides disregard for existing institutions, another mechanism

that experimenters use to enhance their legitimacy is a process of depoliticisation (Ouma and Adésinà, 2019). The process involves keeping away political elites from the programmes by portraying the policy experiment as technical. Like organisations that promoted the adoption of cash transfers in Kenya, GiveDirectly limited interaction with political elites and sought to keep them out of the experiments. The guise is that bringing politicians into the programmes will mess up the programmes as they will fall under patronage politics (Mkandawire, 2015). However, such assertions aim at delegitimising the role of politicians in policymaking decisions while providing space for international organisations to conduct experiments with disregard for political economy realities. By avoiding interaction with considerations related to political economy – which matter in policymaking – policy prescriptions arising from the evaluations can sometimes be less meaningful (Das 2020). Also, by depoliticising social policy experiments, policy uptake or scale-up is compromised since it is politicians that allocate budgets to programmes and policies. As Drèze, (2020) notes, while evidence is a scientific matter, policy is a political decision and therefore inherently political. Depoliticisation therefore is counterproductive to experiments aimed at informing policy decisions.

Moreover, even as experiments are conducted to inform policy uptake, it is unclear from RCTs in Kenya how and to what extent the government has adopted recommendations from the policy experiments. NGOs cannot scale-up programmes to the national level, and experiments, while conducted by NGOs, are

expected to be scaled-up or adopted by governments. From the GiveDirectly experiment in Siaya and Homa Bay counties, it remains unclear how the experiment would inform policy considering the government was already providing regular cash payments to various categories of the population. Furthermore, controlled experiments present a skewed interpretation of reality making it difficult for governments to draw policy lessons. Experiments by international organisations and NGOs are conducted in near perfect conditions which are artificial constructed (Ravallion 2020). They may involve the expenditure of large amounts of money, expensive technology, well trained staff and other resources not at the disposal of governments. Scaling-up or adopting policy prescriptions from the experiments present challenges to governments. Considering the amount of transfers made in Homa Bay and Siaya counties, it would be impossible for the Government of Kenya to match the amount in the experiment. And as government agencies in the experiments are limited, policy uptake from the experiments may not interest policymakers. Moreover, policy prescriptions from the experiments may be misaligned to national plans or be inimical to national social policy needs (Hoffmann 2018).

Additional thoughts

For long Africa has been a site of all sorts of experiments. Africa offers an ideal location for experiments due to structural deficiencies in regulation on research protocols which organisations conducting experiments may exploit to conduct experiments that may be harmful or unethical (Hoffmann 2018), and for experiments that cannot be

conducted in countries where the promoters of the RCTs originate. Sometimes the experiments have little to do with the sites where they are conducted but present researchers with opportunities for publication. Experiments testing whether giving cash to the poor makes improvements to their lives, for example, are *a priori*, as improvement in the financial means of a household mostly enhances quality of life. Of course, people are happier when they receive cash (Egger et al. 2019) – and this needs no randomised controlled experiment to demonstrate.

As Kabeer (2020) notes, publications related to RCTs rarely report crucial details about the challenges and what deviates from research design. This brings into the discussion questions about the transparency of the policy prescriptions that arise from the experiments – prescriptions which may be harmful to well-being in Africa (see Muller, this issue). Besides the lack of veto power discussed above, most international organisations engaged in development work on the continent, particularly on RCTs and cash transfers, also lack legitimacy with the people. Their actions may therefore undermine social norms of reciprocity and community resilience (Adésinà 2011), as most of experiments conducted draw from policy prescriptions which seek to strength individual resilience rather than build on existing community support and resilience. Success for the experiments relies on personal testimony of change, and ‘before and after’ narratives (see <https://live.givedirectly.org/>) rather than changes to structural barriers that perpetuate poverty.

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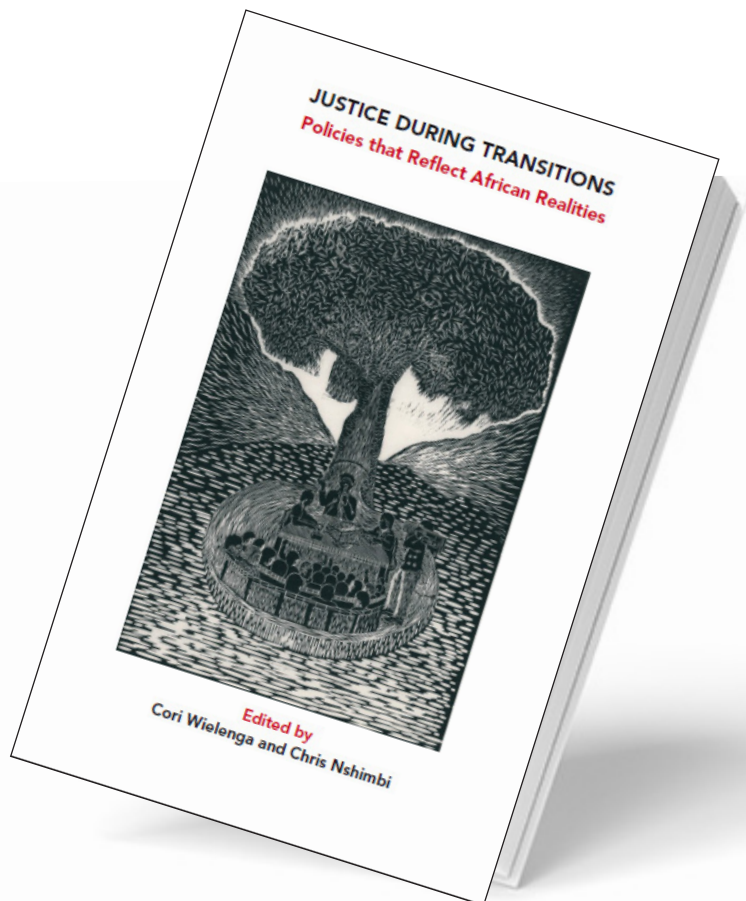
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ISBN: 978-2-86978-755-1

Pages: 252

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CODESRIA would like to express its gratitude to the Swedish International Development Cooperation Agency (SIDA), the Carnegie Corporation of New York (CCNY), the Andrew W. Mellon Foundation, the Open Society Foundations (OSFs), Oumou Dilly Foundation, Ford Foundation and the Government of Senegal for supporting its research, training and publication programmes.

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