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Universal Basic Income in the Developing World

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Keywords

basic income, cash transfers

Abstract

Should developing countries give all of their citizens enough money to live on? Interest in this idea has grown enormously in recent years, reflecting both positive results from a number of existing cash transfer programs and dissatisfaction with the perceived limitations of piecemeal, targeted approaches to reducing extreme poverty. We discuss what we know (and what we do not) about three questions: what recipients would likely do with the incremental income, whether this would unlock further economic growth, and whether giving the money to everyone (as opposed to targeting it) would be wise.

1. INTRODUCTION

Do we really need to know what would happen if the extreme poor were given a basic income?

One could argue that we do not. One of the central goals of development economics has been to understand how to raise the incomes of people who are poor. A sustainable program for universal basic income (UBI) does that by definition. Asking whether the effects are good or bad amounts to asking whether we should be trying to raise the incomes of the poor in the first place.

The reality, however, is that much of the spending on development is on issues like nutrition, health, and education, which may or may not be the priorities of the people that it is intended to help. This is partly because many of the taxpayers in both rich and poor countries who ultimately pay for these programs, as well as many of those who implement them, worry that if they simply gave away money, then the recipients would use it in ways that they do not like. In the most pessimistic predictions, they might become dependent on the transfers, or waste them on alcohol, revelries, or other frivolous needs. Pragmatically, then, the case for or against UBI must include the evidence on how the money would be spent.

Of course, even if you accept that raising incomes is a goal and that a basic income tautologically achieves it, you need not support UBI. There could be other ways of raising incomes that are more cost-effective—that is, better ways to get people a dollar than simply giving them a dollar. Some people may be unable to pursue income-generating opportunities because of a missing market for credit or insurance or because of the psychological burdens of poverty itself. UBI may be more or less effective than alternatives at relaxing these constraints to growth. On a grander scale, large-scale infrastructure investment of the type popular in the early days of development could be a more effective way to raise incomes.

Even if basic income is the best solution for some people, it is not obvious that it is universally best. A targeted basic income could be a better use of scarce resources. This in turn depends on the effectiveness of targeting.

This review, then, examines what we know and what we do not about these three issues. We do not take on a fourth question, which is whether it would make sense to replace many of the existing major silos of government development spending—nutrition, health care, and education, for example—by a single transfer, allowing people to buy these services on the market. This is mainly because, while some developing countries like India and Pakistan have a large private sector in health and education, in many other countries, this is not currently an option. As a result, while we know that, in some countries, the private sector actually provides quality comparable to the public sector at a lower cost, this may not be true elsewhere. We thus consider UBI as an incremental antipoverty intervention.

To anticipate what people would do with incremental income and whether this would align with the goals of policy makers and taxpayers, we draw on the large body of high-quality evidence on the impact of cash transfers in developing countries. The evidence from these studies has been very positive, with little evidence of the negative outcomes mentioned above and positive effects on a very wide range of outcomes—education, health, entrepreneurship, and so on. However, the specific results also vary greatly within and between studies, highlighting the fact that UBI is unlikely to be cost-effective at achieving any particular narrow policy goal.

To examine how UBI might affect the growth of income from other sources, we draw on the large literature in development economics on the constraints that limit investment and risk taking and how to relax them. We consider both financial constraints, such as imperfections in the markets for credit or insurance, and psychological constraints, such as hopelessness or limited bandwidth. For any particular constraint, there is a plausible argument, and typically some evidence, that UBI could help relax it. At the same time, it seems unlikely that UBI would be the most cost-effective

way to relax any particular constraint. If we knew that the issue constraining Abhijit was a missing credit market, for example, then intervening directly in those markets should do more. The growth argument for basic income as we see it is precisely that we know very little about what constrains Abhijit specifically, as opposed to Paul or Tavneet, who may face different problems, or about how to precisely target the interventions to each of them that will best alleviate their constraints. UBI might make sense because it will usually help a bit and will never entirely miss the mark. Moreover, of course, the opportunity cost of basic income as a growth intervention is low, since the bulk of the fiscal outlay is simply a progressive transfer.

Finally, we consider the merits of universal as opposed to targeted income transfers. This is a radical idea in the context of today's social protection schemes, almost all of which are targeted. The usual (and inarguable) point is that resources are scarce and that, all else being equal, it is beneficial to concentrate them where they will do the most good. However, universality is administratively simpler, a virtue especially for states with limited capacity, and may have benefits in terms of incentives and social cohesion. Given the evidence on informational and implementation issues with de facto targeting, we argue that there may be a stronger case for universality than is often thought.[1]

We focus throughout on the effects of UBI on those who receive it. Of course, any policy proposal to implement UBI (or any other intervention, for that matter) must also specify from where the money will come. The net impact of the policy is then the sum of effects on net recipients and net payers. However, because the options for funding UBI are so varied—one could raise revenue from any number of incremental taxes, cut spending from any number of existing programs, or run larger deficits, for example—there is no simple answer to the costs of a UBI policy. It would all depend on the details. Specific proposals will need to be evaluated, drawing on what we know about specific costs (e.g., the effects of a higher income tax). Ghatak & Maniquet (2019) provide further discussion of these issues.

We also focus exclusively on developing countries; separately, Hoynes & Rothstein (2019) consider UBI in developed countries. In many ways, the issues are, of course, quite different. Interest in UBI for wealthy countries has been motivated in part by fears of pending labor market disruptions due to automation and artificial intelligence, rather than concern with eliminating extreme poverty. Wealthy countries with more developed financial markets arguably face fewer constraints on productive investment and growth, and their governments have the capacity to target benefits more finely and reliably, weakening some of the arguments for simplicity that we consider in this review. That said, in other ways, developing countries already look like one possible future for the developed ones: Few people hold stable full-time jobs, and many work a variety of part-time gigs instead; as a result, public policy has never been based on an assumption of universal full-time employment. Perhaps in this there is something that the rich countries can learn from the poor.

The rest of the review is organized as follows. Section 2 reviews existing evidence on the impacts of cash transfers in developing countries and its strengths and weaknesses as a predictor of the potential effects of UBI. Section 3 discusses constraints on further income growth and the merits of UBI in relaxing them. Section 4 discusses the pros and cons of universality relative to finer-grained targeting, and Section 5 offers some concluding observations on the political economy of UBI pilots to date.

[1] Grosh et al. (2008, p. 85) articulate what we take as the mainstream view when they write that, "for the most part, programs can focus resources on the poor to a moderate or high degree without incurring unacceptably high errors of exclusion and administrative, private costs, and/or incentive costs, although not all do so. Factoring in judgments on social and political costs is harder, partly because their metrics are so different, and partly because discussions about them are often more polemical than quantitative, but the widespread and increasing interest in targeting from policy makers suggests that these costs are not preclusive."

2. WOULD A BASIC INCOME DO WHAT FUNDERS WANT?

In the developing world, large-scale basic income schemes could plausibly be financed from (some combination of) repurposed foreign aid, repurposed domestic tax revenue, or new tax revenue—although they would place serious demands on any of these sources. Consider the set of low-income countries (LICs) as categorized by the World Bank. From 2012 to 2016, the median amount of purchasing power parity (PPP)-adjusted per capita Official Development Assistance received in these countries was $77, and the 90th percentile was $210 [based on the authors' calculations from OECD.Stat (**https://stats.oecd.org/Index.aspx?datasetcode=TABLE2A**, accessed August 14, 2018) and the World Development Indicators (World Bank 2019)]. In comparison, median and 90th-percentile PPP-adjusted tax revenues in 2016 were $286 and $464, respectively [based on the authors' calculations from the World Development Indicators (World Bank 2019)]. If we include both LICs and lower-middle-income countries, these figures become $616 and $1,522, respectively. Clearly, funding anything close to UBI would require a substantial share of available resources. Some countries undoubtedly have scope to raise additional revenue, but in general, LICs raise substantially less revenue as a share of GDP than wealthier ones, likely because their capacity to do so is limited (Gordon & Li 2009).

A decision to fund UBI from any of these sources would involve complex processes with many stakeholders. Beliefs about the impacts of UBI would likely play an important role. Technocrats might need to be convinced to hand over money to the poor that they otherwise could have used to design and implement projects in their own areas of expertise and motivation—projects designed to achieve narrower goals for health, education, nutrition, and so on. Taxpaying voters would need to be convinced to support handouts despite commonly held concerns that they would reduce recipients' own initiative or trigger self-harmful behaviors such as drinking or drug use. Polling suggests, for example, that voter support for spending foreign aid on cash transfers drops if even small amounts would be spent on alcohol (Talbot & Collin 2016).[2]

2.1. What We Know

Strictly speaking, there is very little evidence on the effects of UBI in developing countries. We are aware of only three schemes that can truly claim to be UBI in a developing country: one for two years in nine villages in the Indian state of Madhya Pradesh, one in two villages in Namibia, and Iran's nation-wide cash transfer introduced in 2011 to offset the withdrawal of food and fuel subsidies. Only the first of these has been experimentally evaluated. All other existing experimental evaluations study transfers that were different from UBI in some other potentially consequential way. We emphasize two dimensions in particular. First, existing transfers have not been universal but rather targeted, both to subsets of households (through means testing, ordeals, conditions, etc.) and to specific adults within those households (often the female head). Second, existing transfers typically last for relatively short time periods, as opposed to the long-term commitment envisioned by UBI advocates. Both of these differences could lead to important differences in impacts. These questions remain open; an experiment being conducted by the nongovernmental organization (NGO) GiveDirectly in Kenya, which we are currently evaluating, will be the first to our knowledge to examine the effects of transfers given to all adults within selected communities and over more than a decade.

[2]Relatedly, Prather (2017) finds lower support for aid in cash than in kind among US and UK voters. Ikiara (2009) and Moore (2009) discuss the importance of perceived impact for the political viability of cash transfer programs in Kenya and Nicaragua, respectively.

That said, there is a lot of useful information to be garnered from analyses of existing cash transfer programs in developing countries. With the most current available data as of 2018, the World Bank identified 552 million people living in the developing world who receive some form of cash transfer from their government [based on the authors' calculations using data from Ivaschenko et al. (2018)]. While none of these schemes were (to our knowledge) labeled as UBI, they all shared the common and crucial feature that recipients were given the freedom to do what they wanted with their money. Many transfers (particularly in South and Central America) were paid out only if certain conditions were met, but many others (particularly in Africa) were not (Ivaschenko et al. 2018). In some cases—pensions, for example—these transfers have a structure (size, frequency, and duration) quite similar to UBI payments, although they are not universal.

What have we learned from the evaluation of these schemes? We do not attempt a full synthesis, as the literature is vast, and others have recently reviewed it (e.g., Bastagli et al. 2016). Rather, we highlight two basic themes that bear on the policy discussion of UBI.

First, evaluations generally have not found the negative impacts that many feared. Reviewing evidence on temptation goods, Evans & Popova (2017) find that transfers had on average reduced expenditure on temptation goods by 0.18 standard deviations. In other words, far from blowing their transfers on alcohol and tobacco, recipients appear to drink and smoke less. This finding in no way diminishes the seriousness of substance abuse as an issue for the poor, but it does suggest that lack of money may be a cause of substance abuse rather than a constraint on it. For the case of dependency, Banerjee et al. (2017b) review studies that measure the effects of transfers on labor supply, perhaps the most concrete measure of recipients' efforts to improve their own lives. From the recipients' point of view, of course, time is valuable, and spending that time earning money comes at a real cost; it might be very good for them to be able to substitute some unearned for earned income. That said, Banerjee et al. (2017b) find no systematic evidence that transfers discourage work.

Second, evaluations have found a great diversity of positive impacts. For example, a partial list of outcomes affected in a positive way in one study or another, according to Bastagli et al. (2016), includes income, assets, savings, borrowing, total expenditure, food expenditure, dietary diversity, school attendance, test scores, cognitive development, use of health facilities, labor force participation, child labor migration, domestic violence, women's empowerment, marriage, fertility, and use of contraception. Which particular outcomes changed varies substantially from experiment to experiment, and of course, this variation in average treatment effects across experiments understates the underlying variety in individual effects within them. Experiments let us estimate mean effects within (sub)populations but cannot tell us how any particular person spent their transfer, since we cannot observe that person's counterfactual spending.

This variety implies that recipients value the flexibility that cash transfers provide: They reveal a preference for many different things. It also implies that UBI is unlikely to appeal to a technocrat seeking cost-effective ways to increase any particular, narrow outcome. If recipients spend 20% of transfers on health care, for example, then there will likely be some intervention that spends 100% of its budget on health and is more cost-effective at improving it. The technocratic argument for basic income would instead be that its impacts on a variety of different outcomes were collectively a good buy.[3]

[3] Concretely, suppose that a group of technocrats are each responsible for increasing a different outcome, and that they currently have available a targeted intervention for each outcome i that increases that outcome by $\{\overline{\tau}^i\}$ units per dollar and has no effects on other outcomes $j \neq i$. Now suppose that giving away money has some impact on every outcome, given by $\{\tau^i\}$ per dollar. It may be the case that cash is not cost-effective for any particular outcome, in the sense that $\tau^i < \overline{\tau}^i$ for all or most outcomes i, in which case no one technocrat

2.2. What We Do Not Know

None of the existing studies has experimentally evaluated long-lasting UBI. The impacts of such UBI could be different in (at least) three important ways from the impacts of existing cash transfer schemes.

2.2.1. The importance of being universal. First, universality could matter. The coverage of typical programs today is far from 100%, so that a nationally universal basic income or even one that was universal within poor regions of a country would be a substantial departure from the status quo. We calculate that the average country in the World Bank's ASPIRE database of LICs and middle-income countries covered 11% of its population with some form of cash transfer (Ivaschenko et al. 2018).[4] In Kenya specifically, where our evaluation is set, just 5.3% of all citizens were participating in government-funded cash transfer schemes as of 2016, despite the fact that Kenya is a regional leader in the use of cash-based social safety net programming with its National Safety Net Program [based on the authors' calculations from the ASPIRE database (Ivaschenko et al. 2018)].

Mechanically, broader coverage could change the average impact of cash transfers by changing the identity of the average recipient. Generally speaking, existing programs have tended to target the poor or vulnerable—widows, the elderly, low-income parents, and so on. The average recipient of UBI would thus tend to be less poor and less vulnerable than the recipients of existing programs. To forecast how exactly this would affect program impacts, we could potentially adjust existing analyses by reweighting each observation by an estimate of each household's (inverse) propensity to be treated, in an effort to give equal weight to impacts on all sorts of people. This would be a useful exercise, but it would inevitably yield only very imprecise estimates of the impacts on people who rarely qualify for the targeted programs and no information at all about those with no chance of qualifying at all. Thus, examining impacts on entire populations is important to understand the effects of UBI.

More subtly, universality could change impacts if there are interactions between the effects of one's own and one's neighbors' (basic) income. Many proponents of UBI believe that this is the case (e.g., Widerquist 2018). We can say little about this at the moment, as very few studies have simultaneously varied individual treatment status and the overall intensity with which communities are treated and then tested for interaction effects. Work in progress by Egger et al. (2018) and McIntosh & Zeitlin (2018) will generate some of the first evidence on this point, and we expect it to remain an open area for future work.

The prospect of broader eligibility also increases the importance of understanding general equilibrium effects. Presumably, the more people are eligible, and the more money flows in to any area, the larger are the potential effects on prices and wages.[5] There is relatively little evidence to date on how transfers affect local markets. This is certainly in part because many experiments have been conducted at the individual rather than community or market level, but also because the transfers involved have been small in proportion to the local economy. The mean cash transfer program in the ASPIRE database targeted 11% of the population and delivered transfers equal to 18% of recipient household expenditure, so that a typical program transfers an amount less

would choose to use it to achieve her objective. However, it may still be cost-effective in the aggregate if the technocrats could agree on a way of dividing up the cost of the transfers such that they all get a good price per unit of outcome—that is, if there exist weights $\sum \omega^i = 1$ such that $\tau^i \geq \omega^i \overline{\tau}^i$ for each i.

[4]The population-weighted average is lower still, at 9%.

[5]To be precise, the price effects of interest would depend both on cash flowing in and, potentially, on cash flowing out in the form of taxes, to the extent that the UBI is financed through taxation. In low-income areas or to the extent that UBI programs are financed using foreign aid, the effects of inflows would predominate.

than 11% × 18% = 2% of local GDP (less, since recipients tend to be poorer than the average resident).

The work of Cunha et al. (2019) is one exception; they find evidence that periodic small transfers raised food prices in the most remote communities in rural Mexico and not in less remote ones. Work in progress by Egger et al. (2018) randomizes relatively large amounts of money, equivalent to roughly 15% of annual GDP in treated areas, as lump sums and will quantify effects on a wide array of prices, as will our own UBI evaluation. More evidence on this point would certainly be valuable, but given the significant amounts of money required to generate this evidence, it may need to wait until the scale-up of future government-funded initiatives can be evaluated.

2.2.2. Intra-household allocation. Second, delivering UBI to each adult in a household (which is what many basic income proposals contemplate) could have different effects from delivering the same amount of money to a single adult representative of the household (which is how most existing cash transfer programs function). If the unitary model of the household were correct, then, of course, this distinction would not matter, but there is ample evidence that many households are nonunitary in consequential ways (Chiappori & Donni 2009). Individualizing transfers could thus have important effects, both by changing who decides how those particular dollars are spent and by changing the outside options that each family member holds and thus their influence on various other decisions.

That said, there is only a little direct evidence on whether it matters which adult receives a cash transfer. In large part, this is because policy designers have typically required that transfers go to mothers whenever possible, so that there is little variation to study.[6] What evidence exists does suggest that the identity of the recipient matters but not that one type of recipient is unambiguously better than others. For example, Duflo (2000) finds that pension payments to grandmothers but not grandfathers led to anthropometric gains for granddaughters in the same households in South Africa, while Akresh et al. (2016) find in Burkina Faso that, if anything, transfers to fathers had larger impacts on children's health, while also yielding greater investment.

One specific open question is how receiving individualized transfers would affect the status of women, particularly where it is low to begin with. There is some evidence that cash transfers delivered to women on behalf of their households can increase measures of empowerment and reduce domestic violence (see, for example, Hidrobo et al. 2016 and the references therein), but it is largely unclear how the effects of transfers that follow the individual even if she (or he) leaves the household might compare.

2.2.3. Duration and longer-term planning. Third, committing to deliver basic incomes for a long period of time could have effects that differ from the effects of shorter-term transfers themselves. This is true for (at least) two reasons. First, the basic logic of intertemporal smoothing suggests that recipients may be less likely to save a marginal dollar today if they expect to continue receiving transfers far into the future. Second, the expectation of income support in the future could increase recipients' tolerance for risk taking today in settings where they are poorly insured and close to subsistence.

Eligibility for the programs studied in the literature varies but is typically relatively short. Some programs impose a hard requirement that participants graduate after a few years [e.g., Chile's Chile Solidario (Barrientos 2010)], while others reassess eligibility every few years to remove those who no longer meet poverty criteria. Programs such as conditional cash transfers (CCTs) aimed at

[6] A 2012 review identified just eight high-quality studies examining differences in causal effects by gender, for example (Yoong et al. 2012).

families with school-age children typically provide payments only while those children are enrolled, e.g., for up to 12 years if primary schooling begins at age 6 and secondary schooling ends at age 18. Programs aimed at preventing hunger or extreme suffering often operate only during shocks. Pensions are perhaps the exception: These are essentially UBI payments for the elderly. However, the labor supply and other decisions of the elderly are likely to be quite different from those of others.

To understand whether and how duration matters, a key issue is separating the effects of current transfers from the effects of anticipated future transfers. Our ongoing evaluation of basic income in Kenya will generate some evidence on this point; in one arm, the transfers will last for two years, while in another, they will last 12, so that differences in outcomes during the first two years of the study can be attributed to the anticipation of future transfers in the latter group. We are not aware of other studies that similarly vary duration; a few studies examine how effects evolve over the fixed, common duration of a given transfer scheme (e.g., Baird et al. 2011).

3. CONSTRAINTS ON THE POOR

The current trend in economics is to try to connect all interventions to some narrative about growth, even when it is obvious that it is a stretch. This is unfortunate, both because it blinds us to other priorities and other narratives that may be more compelling and because, for the most part, we know very little about how to make growth happen. That said, as we argue in this section, there are good reasons why UBI may actually have something important to contribute to the growth process in poor countries.

The reason has to do with the varied constraints that poor people face. Some cannot save or borrow the capital needed to finance a productive investment, an education for a child, etc. Some face risks for which there is no functional insurance market. Some grapple with psychological constraints—lowered aspirations or the taxing effects of scarcity. We see this variation reflected in the program evaluation data, where the same intervention often seems to work for some and not for others.

UBI is unlikely to be the most cost-effective way to alleviate any one of the underlying constraints on investment. However, we currently know little about which constraints bind for whom, or how, in practice, to target interventions to the specific people who need them. This is what makes UBI, which does not try to step on any specific lever, interesting as a progrowth policy. We can add to this that the costs of UBI are relatively low, in the sense that the fiscal outlay largely goes toward progressive transfers, which have intrinsic value, unlike the costs of many targeted interventions (e.g., training), which represent a pure opportunity cost and are wasted if the intervention is given to the wrong person. Finally, one can think of UBI as a point of departure from which recipients could tailor benefits to their specific needs—opting to receive several transfers in a single tranche, for example, if saving is their personal challenge.

3.1. Constrained by Lack of Credit?

Several strands of research suggest that lack of access to capital constrains some more than others.

At the macro level, one of the biggest shifts in growth thinking over the past 15 or so years comes from growing evidence that resources are misallocated in developing countries. Marginal products of capital (as well as labor and land) are not equalized, contrary to what we expect in a fully efficient economy (see, e.g., Banerjee & Duflo 2005, Jeong & Townsend 2007, Alfaro et al. 2008, Bartelsman et al. 2008, Buera et al. 2011, Restuccia & Rogerson 2008, Hsieh & Klenow 2009). Hsieh & Klenow (2009) structurally estimate marginal products using firm-level data from India,

China, and the United States and show that the marginal products are much more dispersed in India and China. There are various possible reasons for such results, but the most obvious theory is that some firms are (differentially) credit constrained and therefore underinvest relative to others.

At the micro level, an extensive literature on the returns to capital suggests that they are widely dispersed and very high, in some cases over 100% a year (Banerjee & Duflo 2005). There is also strong prima facie evidence of inefficiencies in the financial sector in most developing countries. Borrowing interest rates in the nonbanking sector are often extremely high (for several examples, see Banerjee 2003). Bank loans are cheaper, but access to them is severely limited—according to the most recent financial inclusion insights surveys, 12% of individuals in India, 8% in Bangladesh, 8% in Kenya, 7% in Nigeria, 5% in Tanzania, and 5% in Uganda had a bank loan at the time of the survey (Suri & Jack 2016). The gap between lending rates of the banks and their deposit rates, which measures intermediation costs, also tends to be much higher in developing countries than in the developed world.

Yet these data coexist with estimates from randomized trials of microcredit showing that borrowers on average do not use loans to grow their businesses. This holds across seven randomized controlled trials (RCTs) in six countries spanning urban and rural and low- and middle-income settings, a result confirmed by the meta-analysis carried out by Meager (2016). Does this mean that the small firms served by microcredit are not credit constrained, perhaps because they have low marginal products and therefore do not want more capital? That would be consistent with the fact that, in several of the microfinance RCTs, take-up of credit product was less than 20%.

Heterogeneity is also evident within the microcredit evaluations themselves. Banerjee et al. (2015) find no impact on average but a strong positive impact on the business earnings of those borrowers who owned a business before microcredit was introduced. This impact, in fact, grows over time, even though the control group was itself treated after 18 months, so that after six years, the treatment group's revenues had more than doubled relative to controls (Banerjee et al. 2017a). It appears that the entrepreneurs who chose to set up a business without access to relatively cheap credit were very different from those who only did so when microcredit was available. The presence or absence of such high-return entrepreneurs in the experimental sample may explain why some studies find a positive impact, whereas others do not.

Grants also seem to have (widely varying) effects on investment. In some cases, transfers increase investments in education even when they are not conditional on attending school (e.g., Benhassine et al. 2015). In some cases, households use grants to invest in durable assets such as housing (Haushofer & Shapiro 2013). A growing literature examines the impacts of grants on the capital stock and profits of microenterprises. The resulting direct estimates of the marginal product of capital are often large and positive, which is consistent with there being credit constraints. For example, de Mel et al. (2008) report that a capital grant of $200 to microentrepreneurs in Sri Lanka generates a return on capital of 5.85% per month. Fafchamps et al. (2014) find an even larger effect in Ghana of a cash grant of $120 to microentrepreneurs: Sales revenues go up by 15% per month. McKenzie & Woodruff (2008) report returns that are astronomically high (over 20% a month) from a similar experiment in Mexico where microentrepreneurs were given $140 grants. Bryan et al. (2014) gave a cash grant of $7 to potential migrants in drought-ridden areas of Bangladesh and report that the earnings and consumption of their families improved substantially. Blattman et al. (2014) report on an experiment in Uganda where young men were given grants of approximately $400 per head as a part of a group and find large and durable effects on earnings two and four years after the intervention. The effect is no longer detectable after nine years, as the control group catches up by saving and accumulating, but the gap persists for long enough that it is meaningful to include in a discussion of constraints on business expansion (Blattman et al. 2018).

In contrast, several apparently very similar studies of cash grants find no evidence of an impact on earnings [including those of Karlan et al. (2014) on farmers in rural Ghana, Karlan et al. (2015) on tailors in urban Ghana, and Berge et al. (2015) on microfinance group members in Tanzania] or find short-run effects that then fade [including those of Hicks et al. (2017) in rural Kenya and Brudevold-Newman et al. (2017) among young women in Nairobi]. Haushofer & Shapiro (2018) report on a three-year follow-up of the GiveDirectly cash transfer experiment, and they report that, while within-village estimates of impact are similar to those after nine months (Haushofer & Shapiro 2013), between-village estimates are significant only for assets, and not for earnings or expenditure. As they discuss, the difference between the within-village and between-village estimates must reflect either negative spillovers or differential sampling or attrition across experimental groups.

Why is there such variation? No doubt some of this is due to sampling variation, but some is probably also due to systematic differences within the study populations. For instance, it has been argued that women in many countries face a very different set of constraints in becoming self-employed than do men. De Mel et al. (2008) find no evidence of a positive effect of a cash grant on women, although there is a large and positive effect on men. Differences in innate talent of entrepreneurs is another important factor. This may vary based on the outside opportunities available in various locations. When employment opportunities are better, those who self-select into entrepreneurship are likely to be better suited to the occupation. There is increasing evidence that this can be a very major factor. For example, de Mel et al. (2008), in their cash grant experiment, find that returns vary between 0% and 45% per month; Fafchamps et al. (2014), also mentioned above, report returns between 0% and 30% per month. Hussam et al. (2017) gave randomly chosen entrepreneurs in India a $100 cash grant and estimated the impact on their earnings. The estimated returns vary between 0% and 28% per month.[7]

When we take all the data into account, it seems likely that credit constraints bind for some and not for others. Where they do, UBI potentially provides a source of capital to relax the constraint. Indeed, there is some evidence that existing social protection cash transfers have been used to finance productive investments. In Mexico, for example, recipients of Oportunidades cash transfers invested an estimated 26% of those transfers, raising their long-term consumption (Gertler et al. 2012). In Zambia, a pair of unconditional cash transfer programs raised earnings by 59% of the transferred amount, a sizeable multiplier (Handa et al. 2016).

At the same time, a stream of small payments is probably not the best way to structure a cash transfer if the goal is to finance investment. If someone found it easy to save up small amounts to get the lump of cash needed to make a major investment, then they would not often be credit constrained in the first place. Many pieces of evidence suggest that saving is in fact hard for some people: They join clubs to help commit to saving more (e.g., Anderson & Baland 2002), for example, and prefer to receive commercial payments in larger tranches (e.g., Casaburi & Macchiavello 2019). In the first village to receive UBI transfers from GiveDirectly, some recipients created a savings club to reverse engineer their streams of small payments into larger lump sums. This suggests that UBI could be more effective at alleviating credit constraints if recipients had the option to turn it into a commitment device by, for example, asking to receive several payments lumped together into one larger tranche.

[7]Note that, in some cases, the recipients of these grants already held substantial assets—$8,000 on average in the experiment of Hussam et al., for example—begging the question of whether they could really be constrained solely by a lack of credit. To reconcile this with the experimental impacts, one must believe that, at that level of asset holdings, the recipients are still slowly saving their way toward the efficient scale.

3.2. Constrained by Lack of Insurance?

For some, lack of insurance markets may be a binding constraint. Entrepreneurs may shy away from investing borrowed or owned capital because they want to avoid exposing themselves to business risk.

Evidence that uninsured risks distort investment and production is mixed. A long line of papers use observational data to investigate this (e.g., Morduch 1990, Rosenzweig & Wolpin 1993). Cole & Xiong (2017) review this evidence in the context of agriculture. However, these observational studies suffer two potential issues. First, the presence of risk generates a whole range of institutional adjustments within the villages (Townsend 1995) and even across villages (Rosenzweig & Stark 1989). What we observe in the data, therefore, is the residual effect of the risk after many of these adjustments, which may miss a large part of the true cost of risk. Second, the identification is mostly cross-sectional, which makes it difficult to control for a variety of unobserved factors. One exception is the work of Cai (2016), who uses a triple-difference estimation to show that government-subsidized crop insurance in Jiangxi province in China raises the take-up of the high-return but high-risk tobacco crop.

Early experimental work on insurance was plagued by low take-up rates (which may themselves suggest heterogeneity in the need for insurance, among other factors). However, there are now several studies that look at the impact of (often highly) subsidized insurance on production decisions in agriculture. These include the work of Mobarak & Rosenzweig (2013) in India, Cai et al. (2015) in China, Karlan et al. (2015) in Ghana, and Cole & Xiong (2017). The first two studies find evidence that insurance causes a switch toward crops that are more profitable but higher risk. The second two find evidence that insurance leads to higher investment. The study of Bryan et al. (2014) on migrants in Bangladesh finds that an insured loan, where the repayments are canceled if the migration project fails because of excess rainfall, has positive effects on migration that are similar in magnitude to those of a cash grant of the same amount. However, as we would expect, there is considerable heterogeneity in the response to insurance. Karlan et al. (2014) find much larger effects on those who are willing to buy insurance even at high prices, who also tend to be more risk averse. Cole & Xiong (2017), in contrast, find that the effect is concentrated among the more educated farmers.

Even where insurance does stimulate risky investment, it is not clear that we should think of (objective) risk as the only or primary constraint. As an extreme example, take the Karlan et al. (2015) study that finds both an effect of insurance on investment by farmers and no such effect from a substantial cash grant, with or without the insurance. They argue that this is consistent with a model where the farmers are not credit constrained, at least relative to their rather limited appetite for investment, and the insurance that they deliver is more or less perfect. While this is undoubtedly correct, it is also possible that the risky investment in agriculture is a dominated choice for those farmers who ideally want to invest in something outside agriculture (fixing up their homes, for example)—after all, the authors report that the extra risk taking actually led to a loss, although they are skeptical of the revenue data used in that computation. With the cash grant, they choose that alternative investment. With the insurance, the only way to properly benefit from it is by taking on extra risk, even if they would perhaps have preferred to devote the extra household labor used on the farm to the alternative project.

For those whose investments are limited by uninsured risk, the question is whether higher (basic) incomes would offset this. It seems obvious that someone worried about what they will eat tomorrow cannot afford to take risks, while someone whose basic needs are met for the foreseeable future might. It is certainly logically possible that higher incomes could reduce risk aversion (with constant relative risk aversion preferences, for example), although, to generate substantial

underinvestment, this story must still explain why the person could not save their way to a point where they could tolerate the risky investment. However, the empirical bottom line is that we still know very little about the relationship between income and risky investment.[8] This remains an open question for research on UBI and related schemes.

3.3. Constrained by Psychology?

External constraints such as missing markets for credit or insurance have been development economists' traditional bread and butter. But what if some people face constraints within their own minds?

For some, poverty may simply tax the mental and emotional bandwidth needed to think through important decisions. Mullainathan & Shafir (2013) develop this perspective, drawing on studies of people who are poor in terms both of money and of time or other resources. For example, farmers in Tamil Nadu, India exhibit diminished cognitive performance immediately before harvest, when they are poorest, relative to after the harvest, when they are flush with cash (Mani et al. 2013). Mechanisms like these can generate poverty traps in which the poor remain poor because they cannot afford lumpy bandwidth-conserving investments that the rich make (Banerjee & Mullainathan 2008).

For others, the constraints on investment may involve what they want to and believe they can plausibly achieve—reflecting their hopes, aspirations, confidence, perceptions of self-efficacy, etc.[9] Life on $1.90 a day is emotionally draining, laced with perennial frustrations and the occasional crushing setback. Why then make the sacrifices needed to eke out pennies each day and save toward an abstract and uncertain better life decades away? To do so would take remarkable grit (Banerjee & Duflo 2012).

One clue that these things matter comes from an attempt to directly change aspirations among subsistence farmers in Ethiopia (Bernard et al. 2018). Researchers showed the participants a set of four 15-minute documentaries in which families like theirs describe how they were able to meaningfully improve their lives by working hard and making various routine investments in farming, microenterprise, etc. The films intentionally did not endorse any particular course of action or communicate information about expected returns. After six months and after five years, households exposed to these films reported substantially higher aspirations for their futures. More surprisingly for such a light-touch intervention, they also increased investment in education, assets, livestock, and improved agricultural inputs.

The so-called graduation approach is another way to get at these issues. These programs involve an asset transfer but also a considerable amount of hand-holding and one-to-one training. The goal is to prepare people who have enough confidence in themselves to pursue their life goals. Graduation programs have been shown to have durable effects on earnings in multiple countries, beginning with the work of Banerjee et al. (2015), who report on separate RCTs in seven countries, out of which they find positive effects in six after three years. Banerjee et al. (2016) report on the seven-year follow up of the graduation program in India and find continuing positive impacts. Bandiera et al. (2017) evaluate the same program in Bangladesh and find that the treated are richer and own more assets after both four and seven years.

Balboni et al. (2019) use the data from the graduation program to estimate the relationship between total assets at the household level just after the asset transfer and assets four years after the

[8]A classic work is that of Binswanger (1980), who estimates an insignificant, positive relationship between income and willingness to take financial risks.

[9]Lybbert & Wydick (2018) review the history of these concepts.

intervention and argue that the relationship is consistent with the presence of a poverty trap. They estimate that the intervention was able to lift approximately two-thirds of the treated population out of the poverty trap, but those who had very few assets to start with did not make it. They argue, based on this evidence, that the relatively large size of the asset transfer in the graduation program was important for its success. This observation therefore connects back to the discussion above about whether UBI should come in a few large tranches or many small ones.

Balboni et al. (2019) do not take an explicit stand on the underlying mechanism behind the success of the graduation program, but in their model, they treat the asset transfer as the key step. However, in the context of the graduation programs, there is some evidence that the hand-holding was also an important part of the package. Banerjee et al. (2017b) compare a graduation program with a pure asset transfer and with the provision of a savings opportunity in Ghana and find that only the former durably raises earnings. For now, at least, it seems that the hand-holding and training are integral parts of the graduation model.

This could be because the graduation program aims to target the poorest of the poor. Many of these people have never had an opportunity to be self-reliant in their lives, and the hand-holding is important in giving them the confidence to try. This also suggests one potential reason for the gender gap in the effects of a cash grant—perhaps poor women are more likely to lack the confidence to succeed as a result of the various societal pressures on them. The seven-year follow-up of the graduation intervention in India finds evidence that the beneficiaries were starting to move beyond the original businesses that they were helped to set up and into other lines of trade. This seems indicative of growing confidence and optimism.

For those who are already in business and/or have a settled occupation, however, the future may feel less exciting. They may have already investigated what it would take to break into the middle class and realized that, while there are several relatively small things that would earn them high returns, the path to building a sizable business remains extremely arduous given that they have no access to the kind of capital and risk capital that it would take to make it. Therefore, they may be indifferent to opportunities that are not going to be transformative. This may be the reason why marginal product varies so much: Most people do not care where they stop trying. They are in a pessimism trap.[10]

Can this model be consistent with the findings about impacts of the various interventions discussed above? A grant or a grant and insurance might break this pessimism trap by encouraging the entrepreneurs to try again and thereby start on a rapid growth trajectory [Banerjee & Mullainathan (2010) develop these ideas formally]. However, it may also lead them to try half-heartedly, feeling obliged to take advantage of the opportunity that was just laid in front of them but also feeling sure that the intervention would not be enough to get them into the middle class. If that is the case, we might see them going a small distance and then no further.

UBI could be transformative for people bound by internal constraints. Not having to worry about making ends meet could free up the mental and emotional bandwidth needed to focus on getting ahead or reset hopes and beliefs about the future. Whether this is true is, of course, still to be seen. However, evidence from other cash transfer programs does suggest that they can profoundly affect mental health for some. In Kenya, larger GiveDirectly transfers reduced levels of the hormone cortisol, a biomarker for stress that has been linked in other studies to myopic decision making (Haushofer & Shapiro 2013). In a particularly poignant example, cash transfers in

[10]One piece of evidence consistent with this view is the work of Hsieh & Klenow (2014), who compare the trajectories of Indian, Mexican, and US firms over time and show a striking pattern: The US firms either shut down after a few years or become enormous. The Indian firms remain the same size more or less from birth until they shut down many years later.

Indonesia reduced suicide rates by 18% (Christian et al. 2019). Surely transfers that affect the desire to live can also affect how people choose to live.

4. TO TARGET OR NOT

A central question about UBI is whether universality is in fact efficient. For any given budget, is it better to spread those resources evenly or to give larger amounts to the poorest? Of course, fixing a budget implicitly sidesteps the (thorny) question of the cost and benefits of changing that budget via changes in tax policy, cuts to other programs, larger deficits, etc. Hanna & Olken (2018), as well as Ghatak & Maniquet (2019), provide useful related review and analysis of some of these issues.

We also largely abstract from the details of targeting. There has been a large literature on different approaches and their effectiveness, ranging from by far the most common, a proxy means test (PMT),[11] to community targeting,[12] to self-selection.[13] A detailed discussion of the relative merits of these approaches is beyond the scope of this article; Ravallion (2016) and Hanna & Olken (2018) provide excellent reviews.

We suspect that universality has several underappreciated benefits, and targeting has several underappreciated limitations. We review three main issues.

First, even in a state with strong capacity to target transfers to households with certain characteristics, it is not clear whether doing so increases their overall impact. Work quantifying the relationship between impacts and targeted characteristics is limited, as is work on the potential disincentive effects of targeting. It is also unclear how effectively targeting poor households succeeds in targeting poor people, taking into account the unequal distribution of resources within households and redistribution of resources across them.

Second, universality could reduce administrative costs. Targeting well requires data collected repeatedly, given that significant shares of people in developing countries change their poverty status from year to year. The administrative costs of universality are likely to be far lower, especially given the ongoing investments that emerging-market governments are making in digital ID and payment systems.

Third, universality could improve the political economy of redistribution. Government capacity to implement nuanced targeting schemes is often limited, particularly in the poorest areas, where it is most important to get it right. In cases like these, making eligibility universal may have a modest effect on the realized incidence of benefits while at the same time substantially reducing the scope for corruption and other abuses of power. Broad eligibility could also help build political bases of support for sustained redistribution.

Overall, we suspect that universality deserves more serious consideration in many contexts, and that the kinds of targeting that will typically make sense will be relatively simple—tying benefits to geography, for example, or using small hurdles to disincentivize the very wealthy from opting in. That said, this is a fundamentally challenging question given how hard it is to obtain rigorous evidence on the causal effects of different targeting regimes. Besley & Kanbur (1990, p. 80) wrote that "policy analyses and research programs utilizing [micro] data, and addressing the issue of targeted versus universalistic schemes are now pressing." They were right then and, we feel, still are today.

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[11] In this approach, households are surveyed to enumerate their assets, these responses are used to estimate incomes, and transfers are then given to those below a given predicted income.

[12] This approach refers to simply asking the community who is poorest (see, for example, Alatas et al. 2012).

[13] In this approach, participating in the program involves bearing some cost that makes participation relatively attractive to the poor. Workfare programs such as the National Rural Employment Guarantee Scheme in India are a common example.

4.1. Costless Targeting with Unlimited State Capacity

Suppose first that targeting would be implemented by a perfectly efficient and functional state, without any internal issues of performance, incentives, or administrative cost. This is clearly unrealistic for developing countries but helps segment the various issues around whether to target.

Targeting the poorest has the obvious advantage of transferring resources to people for whom the marginal value of money is highest, since they have the least money. Yet things are less clear in a world of imperfect markets. As we describe above, in worlds where markets are imperfect, recipients of transfers may use them to relax binding constraints on growth, for example, by purchasing assets that they otherwise could not have financed or starting businesses that they did not have the capital for. In this case, the welfare impacts of the transfers depend on variation in the opportunities and constraints that each person faces, as well as variation in their baseline standard of living (and thus their marginal utilities). It could well be optimal to make transfers to someone a bit better off in a community if this will enable them to make a transformative investment in that community. Existing analysis of optimal targeting has largely ignored this issue, so we know little about whether targeting the poor actually optimizes impact on poverty.

If interactions between households are important, then optimal targeting becomes even more complicated. Transfers to people embedded in existing networks of mutual support may be redistributed within those networks and generate a final distribution different from the one on paper (e.g., Angelucci & De Giorgi 2009). As we discuss above, many basic income advocates argue that the effect of entire communities being in it together will be greater than the sum of the effects on individuals treated in isolation. For example, targeting may create bitterness and social division that might reduce willingness to contribute to the financing of local public goods (Kidd & Wylde 2011); universality could have the opposite effect.[14] More generally, if own and neighbor's treatment statuses are substitutes, then this strengthens the case for targeting, while if they are complements, then it weakens the case. As of yet, we know very little about these interactions.

Targeting may also alter the political viability of basic income schemes—there is a large theoretical literature showing this, and reviewing it is beyond the scope of this article (see, for example, Casamatta et al. 2000, Cremer & Roeder 2015). A program's beneficiaries typically form the core constituency willing to fight for its effective implementation or its continued existence. If only a small number of the poorest, most disadvantaged and disempowered members of a society benefit from a program, it may be hard for them to exert much influence over its future. This is one reason that some in India, for example, have fought for the expansion of eligibility in means-tested programs such as the Public Distribution System, arguing that it would benefit the poorest to include somewhat better-off people who are influential enough to push back against the corrupt middlemen who looted the system in the past (e.g., Dreèze & Khera 2010). Consistent with this argument, Klasen & Lange (2016) show, using data from the Chinese urban Dibao cash transfer program, that a 1% increase in the share of people who get benefits increases the budget available for the program by one-third of a percent. Using fixed budget arguments for targeting may therefore underestimate the benefits of universality.

Finally, targeting may create disincentive effects. As we discuss above, targeting a basic income relocates where the disincentive effects may fall (although, of course, they may be of small magnitude). Any redistributive scheme can create disincentive effects for some part of the population, depending on who pays and who receives. For example, in a UBI scheme, disincentive effects would be concentrated among the people who are marginal contributors to the scheme, likely the upper and upper-middle classes. If, instead, the basic income transfers were targeted to, say, the

[14]Related, Walker (2018) finds no effect of targeted lump-sum transfers on public good provision in Kenya, rejecting economically meaningful magnitudes.

bottom quintile, then this would lower taxes at the top but create a new disincentive effect to move out of the bottom quintile. Fundamentally, then, the question of targeting is one about where in the distribution of incomes to locate the disincentive effects and the magnitude of any such disincentive effects. Unfortunately, we know very little about the incentive effects of targeting regimes in developing countries, in contrast to work on public finance in developed ones, for which this has been a major focus. They certainly exist; Imbert & Papp (2019) find that the regional targeting of India's employment guarantee to rural areas has discouraged rural-to-urban migration, for example (which may be good or bad, depending on whom you ask). Similarly, Banerjee et al. (2018) look at the impact of asset-based poverty targeting of a program in Indonesia on the purchase of specific assets (televisions and SIM cards) and find a temporary increase in misreporting of these assets but no reductions in sales of these assets, implying that disincentive effects may be small. More work in this area would be of great value.

Relatedly, there is also a small but growing literature on measurement error and/or strategic misreporting of consumption and asset measures. On the pure measurement error and recall issues, for example, Beegle et al. (2012) show that this error can be significant and worse for illiterate and poorer households. Similarly, Silverio-Murillo (2018) uses data on Progressa in Mexico and shows that 10% of households are poor according to the husband's accounting of its assets but not according to the wife's, and 8% vice versa. On strategic misreporting, Boozer et al. (2009), using a small sample in Ghana where spouses were separately asked about consumption, show that there are some expenditures that each spouse knows nothing about, which has implications for measuring both static poverty rates and dynamic transitions out of poverty. Similarly, Stecklov et al. (2018) use a field experiment to show that, when surveys are incentivized, households report 15% fewer assets (with no difference in assets observable to the surveyors), which the authors interpret as households wanting to appear poorer.

4.2. Costly Targeting

Actual targeting is, of course, neither perfect nor costless. The administrative costs of targeting can be quite high, especially if eligibility lists are regularly updated, and if the differentiation that they provide between poor and nonpoor is far from perfect.

Any method of targeting yields both exclusion errors (poor households that are deemed ineligible) and inclusion errors (nonpoor households that are deemed eligible). Hanna & Olken (2018) clearly show the trade-off between the exclusion and inclusion errors for Indonesia and Peru: To reach 80% of the truly poor people (i.e., an exclusion error of 20%), there will be an inclusion error of 22–31%. Of course, keeping the budget the same, to reduce the exclusion error, you can increase the number of people that get transfers, but they will all get a smaller amount. Similarly, Brown et al. (2016) look at nine countries in Africa and show that transfers using a very simple demographic scoring system or a basic income do almost as well as a PMT in reaching the poor. Exclusion rates remain high at 25%, and the targeting methods are particularly bad at reaching the poorest. Finally, Klasen & Lange (2016) conclude that there are small differences between targets based on demographics and geography and targets based on more complex asset-based measures, but both make poor proxy objectives, as they do not relate closely enough to poverty effects. The exclusion errors inherent in targeting also pose a horizontal equity issue (especially if the rate of exclusion correlates positively with the depth of poverty), as those who have the same conditions may not be treated the same. Universality avoids this.

Most targeting measures in developing economies focus on household poverty and not individual poverty, since measuring individual poverty using consumption data is extremely hard and costly to do reliably. However, focus on the household implies one important assumption: that the poorest individuals live in the poorest households. If this is not true, then the individual-level

exclusion errors may be significantly higher than the error rates mentioned above. There is a small literature that looks at this question; Brown et al. (2017) provide a good review. They also use nutrition and wealth data from 30 countries in Africa to show that approximately 75% of undernourished children and underweight women are not in the poorest quintile of households. Half are not in the poorest two quintiles. Similarly, using data from Senegal that have a somewhat individualized measure of consumption, De Vreyer & Lambert (2016) show that nonpoor households house one-eighth of all poor individuals. If a UBI scheme were to enroll not only all households but all adults within these households, as is often proposed, then it would avoid having to worry about this distinction.

Targeting also comes with costs. Since the share of individuals engaged in the informal sector in developing countries, either as informal workers (not on contract) or in small self-employed microenterprises, is high, the government has virtually no information on which households are poor and which are not. It therefore has to collect this information in some way (as mentioned, the PMT is the most common), and that process in and of itself has costs.

Hanna & Olken (2018) state that the costs of targeting are quite small in Indonesia and Peru: $42 million every three years, with annual costs of $1.1 million, in Indonesia, and $10.8 million, with annual costs of $1.1 million, in Peru. Kidd et al. (2017) give a good review of targeting and discuss some of the costs. For example, the 2009 PMT survey in Pakistan cost $60 million, the PMT survey in Indonesia cost $60 million in 2011 and $100 million in 2015, and Kenya's Hunger Safety Net Program spent approximately $10 million to survey only 380,000 households (4% of the population).

As Kidd et al. (2017) also point out, it is rare for PMT surveys to be repeated. For example, Pakistan's last PMT was in 2009; Indonesia had a four-year gap between PMTs; and in Mexico, in some areas, registration for their CCT program (Oportunidades) was not repeated for over a decade.

This makes accurate targeting even harder given that poverty is not a static concept but a dynamic one. There are a lot of changes in poverty status year on year. For example, Baulch & Hoddinott (2000) provide an excellent review and show that the group of consistently poor or consistently not poor in panel data is rather small. They show, for three countries (India, Pakistan, and Zimbabwe) where panel data on poverty have been analyzed, that these two groups make up 34%, 45%, and 40%, respectively, of the study population. For income or expenditure quintile data, across seven studies, they show that, of the households that start in the bottom quintile, between 18% and 32% move by one quintile over time, and between 24% and 61% move by two or more quintiles. Such dynamics would worsen the exclusion and inclusion errors of targeting, something UBI would solve by its very nature. Similarly, Jayne et al. (2002) show that food aid allocations in Ethiopia were very spatially stable over time but concentrated in areas that were not the poorest in a survey done in a given year. They show evidence that at least some of this is because of inertia created by the fixed costs of operations and identifying the poor.

Finally, given the discussion of costs, it is worth asking about the administration or implementation costs of a universal system, i.e., the costs of distributing the transfers themselves. To avoid double counting, it will be important for developing countries to have a well-functioning universal identification system, preferably digital, although this will be a one-time cost. This has already happened in India under their Aadhaar biometric system and is slowly happening in other countries, in some countries funded by donor money rather than by government budgets. For example, World Bank pledged $150 million to support Morocco's development of the National Population Register, which will also assign a unique identification number. The UK Department for Information Development contributed $11 million toward Malawi's recent national identity card system. If these identification systems are also connected to digital payment systems such as

mobile money or digital bank accounts, then the marginal costs of implementing or distributing the transfers will be close to zero.

4.3. Targeting with Limited State Capacity

Suppose that we take a more pessimistic view of state capacity, appropriate for most countries in the developing world. In such settings, universality has the added potential benefits of reducing administrative burdens on the bureaucracy (as officials only need to determine who exists, not whether they also meet eligibility criteria) and, in doing so, also reducing the scope for the abuse of state power.

In developing countries, state capacity to implement complex tasks such as targeting is often limited. This can be particularly true in poorer areas, creating unintended regressivity in the allocation of benefits. Labor payments in India's mammoth National Rural Employment Guarantee scheme, for example, are allocated progressively within states due to the self-selection induced by the program's work requirement but regressively across states because the better-run states are better equipped to implement the scheme and tick the boxes required to trigger the release of funds from the central government (Sukhtankar 2016). Similarly, the benefits from a community-driven development scheme in Tanzania were distributed much less progressively than they otherwise would have been because local officials in poorer communities were less likely to apply for funding in the first place (Baird et al. 2013).

When state capacity to discipline front-line bureaucrats is limited, targeting can also create opportunities for corruption and other abuses of power. In Karnataka, for example, the officials tasked with targeting those below poverty line routinely violated the official eligibility criteria. Petty bribery was widespread, and the resulting distribution of benefits was much less progressive than it would have been if the rules had been followed (Niehaus et al. 2013). If the officials had been given less discretion—and, in particular, if they had been told to simply enroll everyone—then they might have had less leverage to extract rents. Complex targeting rules also create the risk of confusion: Eligible households may not know what to expect, and nonrecipients may not know why they are not receiving the transfers. Banerjee et al. (2019) show that informing recipients (and distributors) what they should expect from the program matters and reduces leakages in the program significantly. These are among the reasons that Dreèze & Khera (2010) have argued for simpler approaches—either universal or quasi-universal—in which a minority of the most affluent people are declared ineligible using simple criteria.

In part because of these issues, the de facto targeting of social protection schemes is fairly mixed. Ivaschenko et al. (2018) estimates that, depending on program type, the average share of social program beneficiaries that come from the bottom quintile of the (pretransfer) income distribution is between 33% and 45%. Meaningful proportions of beneficiaries are much better off in relative terms, including 5–10% in the very top quintile. In some cases, the allocation of antipoverty dollars can in fact be quite regressive. In its 2017 Economic Survey, India's Ministry of Finance pointed out that the poorest districts, defined as those which collectively contained 40% of the country's poor, received much less than 40% of the budget allocation for many of the country's largest antipoverty schemes (Minist. Finance 2017). These figures suggest that the typical bureaucracy does not perform all that well when asked to perform a difficult task like targeting and might do better if given the simpler task of enrolling everyone.

4.4. The Bottom Line

Whether and how to target basic incomes will, of course, ultimately depend on specifics of context. That said, our reading of what we currently know—and, crucially, what we do not—about targeting suggests that a few principles should typically hold.

First, the benefits of targeting may be overestimated by analyses that do not take into account the incentives that it generates or the realities of implementation on the ground. Universal or near-universal approaches deserve more consideration than they often receive.

Second, the kinds of targeting that will make most sense will often be relatively simple, such as geographic targeting. Designers should guard against creating too much discretion for the front-line staff who implement targeting, including the implicit discretion that is created when a policy is too complicated for beneficiaries to understand it and hold local officials to account. However, these targets may perform extremely poorly in reaching the poor.

Third, it may be possible to build small ordeals into the design of programs to create some targeting by self-selection and thus make programs more progressive. For example, if a government offered a small basic income to everyone but with some hassle costs involved in collecting it (e.g., a weekly trip to an ATM), then the wealthier households might simply not bother to participate. The danger in this case, of course, will be to not exclude those for whom the ordeals are simply too difficult (e.g., the disabled), and so special provision should be made for them.

Finally, we note that the per-person costs of delivering transfers are falling rapidly in many places due to advances in last-mile digital payment infrastructure. All else equal, this will tend to further increase the appeal of broad or universal targeting.

5. CONCLUSION: BASIC INCOME RESEARCH IN POLITICAL CONTEXT

In reviewing the issues, we identify some that are well understood and others that are not. As researchers, our instinct is, of course, to fill the gaps. However, experiences from earlier basic income pilots also remind us that the ultimate value of any further research will depend on how it interacts with politics.

To our knowledge, the first significant pilot of UBI in a developing country was conducted in the Otjivero-Omitara area of Namibia between January 2008 and December 2009. All residents younger than 60 and registered as living in the area as of July 2007 received monthly, unconditional transfers. A before-and-after analysis by program advocates suggested that rates of poverty and child malnutrition fell, while rates of income-generating activity and children's school attendance rose, among other positive changes, in spite of significant in-migration (Haarmann et al. 2009).

This pilot helped bring the idea of a basic income grant to national prominence. Hage Geingob, the first donor to the pilot project, successfully ran for president in 2015. He created the Ministry for Poverty Eradication and Social Welfare and appointed Bishop Zephania Kameeta, the former Chairperson of Namibia's BIG Coalition, to head it (Basic Income Earth Netw. 2016). However, basic income now seems to be dying a quiet death behind the scenes, superceded by a quasipublic food bank initiative. Conservative voices within the ruling coalition have not altered their stance that giving people money would risk making them lazy. With hindsight, some activists have come to view Kameeta's inclusion in the government as a cynical move to coopt the basic income movement rather than a real commitment to implement its proposals (C. Haarmann, D. Haarmann, personal communication, August 6, 2018).

A second pilot was conducted between 2010 and 2011 by the Self Employed Women's Association (SEWA), an Indian NGO, in the state of Madhya Pradesh. Over 6,000 individuals in nine villages received small monthly transfers over the course of 18 months. Transfers were given to each individual in the selected villages, including smaller transfers for children. Researchers compared outcomes for these individuals to those in control villages, which were chosen at random

from the same pool as the treatment villages. They reported improvements in treated villages on savings and indebtedness; various measures of assets and wealth; child nutrition and food security; spending on health and education; school enrollment, attendance, and performance; labor supply for women; and women's empowerment (Davala et al. 2015, SEWA Bharat 2014).

It remains to be seen what policy impact these results will have. Some of those involved in the project had discussed it with members of the UPA government at the time, but that administration lost power in 2014, shortly after the results were released.

Chief Economic Advisor Arvind Subramaniam then made basic income the topic of a full chapter in his 2016–2017 Economic Survey, citing the experiences in Madhya Pradesh, among others. Interest flagged again until early 2019, when the government of the state of Sikkim announced plans to implement UBI, and the opposition Congress party pledged to guarantee a minimum income for the poor (not universal) if returned to power. Whether either initiative comes to fruition remains to be seen.

Limits to eligibility have proven particularly resilient. In Zambia from 2010 to 2013, for example, the government substantially broadened eligibility for its Social Cash Transfer scheme. It did not make the scheme universal, but it removed means-testing and enrolled all households with children under 5, orphans, or disabled members. A multiyear experimental evaluation found that the transfers not only reduced immediate poverty, but also had substantial impacts on assets and earnings (Handa et al. 2016). These results contributed to an increase of the scheme's budget by a factor of 8. However, they did not convince the government to broaden targeting; in fact, restrictions based on measures of poverty and incapacity to work were reintroduced (van Ufford et al. 2016).

Iran's universal cash transfer scheme has proven relatively durable in comparison but remains under pressure. Transfers were made universally into individual bank accounts beginning in 2011, in which year they accounted for a full 6.5% of GDP and approximately 29% of median household income. The government has subsequently allowed the real value of the transfer to fall by more than half by opting not to adjust it for inflation, however, amid criticism that the transfers reduce the labor supply of the poor (Salehi-Isfahani & Mostafavi-Dehzooei 2017).

We are, of course, in the early days of basic income research in developing countries. The first results from the first large-scale experimental evaluation, the GiveDirectly initiative in Kenya, are due in 2019. This and other future studies can draw many lessons from the earlier pilots. For example, it will be useful to separately estimate impacts on the kinds of people who are not typically enrolled in targeted schemes to understand how impacts vary. However, a broader lesson from these pilots is that the UBI debate is not simply a debate about whether small, regular transfers are an effective way to achieve some policy objective. It is as much a debate about what kind of society people want (and will vote for) as about how to get there.

DISCLOSURE STATEMENT

P.N. is an unpaid director on the board of GiveDirectly, an NGO running the basic income project referenced in the article. The authors are not aware of any other affiliations, memberships, funding, or financial holdings that might be perceived as affecting the objectivity of this review.

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