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Economic Development

Cash as Capital

To fulfill the promise of cash transfers, we need to double down on investment in research.

By Chris Blattman, Michael Faye, Dean Karlan, Paul Niehaus & Chris Udry | Summer 2017

We are now roughly 15 years into the new era of experimental evaluation of programs to fight extreme poverty. It has been a wild ride, as experimentalists have applied randomized controlled trials through a long backlog of programs—representing billions of dollars of annual spending—that had previously been evaluated using flawed methods or simply assumed to work. Among all the insights and controversies from this new approach, one of the more positive surprises has been cash transfers: simply giving money to poor people.

(Illustration by Mike Austin)

Proponents of cash transfers have long argued that they give the poor the flexibility to choose the best opportunities—rather than having such opportunities chosen for them—and that they can also streamline the delivery of aid. Critics countered that the poor may simply waste much of the money or even harm themselves by purchasing drugs and alcohol.

In July 2016, the Overseas Development Institute (https://www.odi.org/) published a review (https://www.odi.org/sites/odi.org.uk/files/resource-documents/10749.pdf) of 165 studies from 30 countries that highlighted "how powerful a policy instrument cash transfers can be, and … the range of potential benefits for beneficiaries." Former United Nations Secretary-General Ban Ki-moon has argued that "where markets and operational contexts permit, cash-based programming should be the preferred and default method of support." Separate reviews by economists at the World Bank and MIT, respectively, find that recipients of cash transfers have not blown them on temptations such as booze (http://documents.worldbank.org/curated/en/617631468001808739/pdf/WPS6886.pdf) or become dependent (http://economics.mit.edu/files/10861) on them and stopped working to improve their lives. Giving money to people who do not have much of it is no longer taboo.

Swayed by the evidence, aid agencies and reformers within emerging-market governments have begun to push cash-based solutions to replace the most inefficient and inflexible legacy interventions. Take humanitarian support, for example: Cash is an effective way to meet the basic needs of people displaced by conflict or disaster. And yet, at most 5 percent of humanitarian aid is given this way. At the first World Humanitarian Summit (https://www.worldhumanitariansummit.org/) in May 2016, participants agreed that cash-based responses to humanitarian crises should increase.

Emerging-market governments have also begun to shift away from expensive, regressive, and distortionary subsidies of basic commodities such as food or fuels and instead are giving cash to the poor. Yet while the case for the near-term impact of cash transfers is compelling, there has been less evidence on whether they—or any other intervention—drive longer-term change. This is simply because it is rare to see long-term evaluations in development. But it would be foolish to infer a *lack of impact* from a *lack of evidence of impact*.

The latest research, however, provides some reason for cautious optimism. In a wave of new, longer-term evaluations, cash continues to perform well, with several studies finding large and sustained impacts from onetime grants. But these studies also reveal that cash does not always have sustained impacts. The challenge now is to figure out why cash makes such a big long-term difference for some and not for others.

Evidence of Long-Term Impact

What do we know so far? Several studies have found large long-term increases in earnings as a result of onetime capital infusions. Young people in northern Uganda who received such grants started trades (https://academic.oup.com/qje/article/129/2/697/1866610/Generating-Skilled-Self-Employment-in-Developing) and achieved a 40 percent annual rate of return on the grants after four years. A similar program in Sri Lanka doubled these rates (https://3ieifpriseminarseries.files.wordpress.com/2012/05/gender_business_training.pdf) of return. These programs have several common threads. They involve "business development" grants that are typically large (nearly $400 in Uganda and $100-$200 in Sri Lanka) and given either to poor people with very small existing businesses or to those who want to develop a business or trade.

There is also some evidence that programs not designed to finance productive investment, such as PROGRESA in Mexico or Zambia's Child Grant Programme, have similarly raised incomes (http://onlinelibrary.wiley.com/doi/10.1002/pam.21892/full) . More subtly, there is evidence that some recipients invest in things that eliminate expensive, annual cost burdens. For example, GiveDirectly (https://www.givedirectly.org/) recipients in Kenya invest in metal roofs (https://www.princeton.edu/~joha/publications/Haushofer_Shapiro_UCT_2016.04.25.pdf) to avoid rethatching every year.

Collectively, this evidence of long-term impact is not only compelling but actually stronger than the evidence we have for many of the things on which aid dollars are currently spent. Given everything we know about developing economies, it makes sense that onetime transfers can have long-term effects. In developing countries, capital and insurance markets often function poorly, so poor people with good investment opportunities cannot seize them. Existing loan products remain fairly short-term and expensive, and programs devoted to such lending are not yet having the impacts (https://www.aeaweb.org/articles?id=10.1257/app.20140287) many hoped for. As a result, people react to transfers by finding ways to raise future standards of living, such as installing a better roof, putting a child through school, or starting a business.

Yet not all studies find long-term impacts from cash transfers. For example, grants of $120 to micro-enterprises in Ghana increased some measures (https://web.stanford.edu/~fafchamp/CapitalDrop.pdf) of profit for men but none for women, at least within the first year or so of follow-up. Grants of $200 to street youth (including petty criminals) in Liberia increased their legitimate businesses and earnings, but only temporarily (https://www.aeaweb.org/articles?id=10.1257/aer.20150503) , likely because their investments were later stolen.

To be fair, there is no reason to expect all cash-transfer recipients to invest, or to invest all the money they get, or to get good returns. Some recipients may not have access to good investment opportunities or information on such opportunities, or may be uncomfortable taking the associated risks. For these groups—or in the absence of other well-functioning markets—additional interventions or complexity may be needed to supplement the cash programming. For example, graduation programs, which provide productive assets, training, life coaching, health services, and bank accounts in addition to cash, have demonstrated positive impacts (http://www.econ.yale.edu/~cru2/pdf/Science-2015-TUP.pdf) on long-term average income (http://sticerd.lse.ac.uk/dps/eopp/eopp58.pdf) up to seven years. (Albeit, as with cash, tremendous variation between those helped the most and those helped the least.)

Other recipients may face different circumstances. They may have different means of financing their investments, or may simply have other immediate needs, such as acute hunger or health issues; yet others may have shorter time horizons—the elderly, for example, might prioritize health care and time with loved ones over asset accumulation. And other people may want to invest but lack the self-discipline to forgo immediate needs or the social status to fend off conflicting claims on their resources. And of course, some people may invest and simply see their investments fail. Even ostensibly great investors have the occasional bad decade or two (http://www.moneytalksnews.com/why-youre-probably-better-investing-than-donald-trump/) .

Future Research Agenda

To understand and optimize the impact of such cash-transfer programs and other development interventions, we need to design and run more ambitious, longer-term evaluations at a larger scale. Experimental evaluations are usually sized and budgeted to give reasonably precise estimates of the average impact on the *overall* population. To learn for whom something works, we have to delve into the details of subgroup analysis. Are effects biggest for the poorest or the less poor? The young, middle-aged, or elderly? Men or women? Or will some completely different predictors turn out to be crucial, such as mental health or the role of the women in the household? To answer questions about these subgroups, we need to study large samples of each.

We also need to coordinate evaluation across sites, to improve comparability. Context is likely to matter at least as much as individual characteristics. Impacts, for example, could be weaker in northern Uganda, which has experienced decades of conflict, than in nearby Rwanda, which has enjoyed decades of (relative) stability. Testing for such differences requires a study that is carefully coordinated across multiple sites. Studies like this have historically been rare but recently become more common—for example, in recent evaluations of graduation programs and of microcredit.

Testing at a large scale would also help us learn how to transfer capital and employ other interventions more effectively for longer-term impact. "Giving people money" seems simple enough, but the phrase covers myriad design choices. Should money be paid in weekly, monthly, quarterly, or annual installments? Over what period? Should we decide how often people get paid or let the recipients decide? Should we ask people to plan how they want to use the money? Should we provide any accountability mechanisms to help them stick to these plans? Can we use technology to do these things in cheap, scalable ways? A large-scale evaluation would enable us to test different design variants and begin to learn which ones enable recipients to get the most out of their money.

There are yet further challenges for researchers. For example, for whom does cash promise longer-term impact, and how does the answer affect how we define success? Should antipoverty programs start selecting recipients based on how they can be expected to use the money instead of how poor they are to start with? Or is it ethically preferable to treat everyone who lacks money equally and let them define success for themselves?

This research agenda is ambitious, but individual organizations are already making significant progress. GiveDirectly, for example, is running a study in Western Kenya that they estimate is large enough to detect effects for subgroups as small as 15 percent of the population. But completing the agenda will take determined leadership and a mandate to invest in global public goods such as evidence creation. There is some precedent for this: The Transfer Project (https://transfer.cpc.unc.edu/) , for example, has coordinated and synthesized a number of studies on cash transfers for social protection in Africa, with financial backing from UNICEF (https://www.unicef.org/) and the UK Department for International Development (https://www.gov.uk/government/organisations/department-for-international-development) , among others. With hundreds of billions of dollars set to flow to the poor in upcoming years, it would be foolish not to make a corresponding investment in understanding, lest we diminish the good that such cash transfers can accomplish.

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