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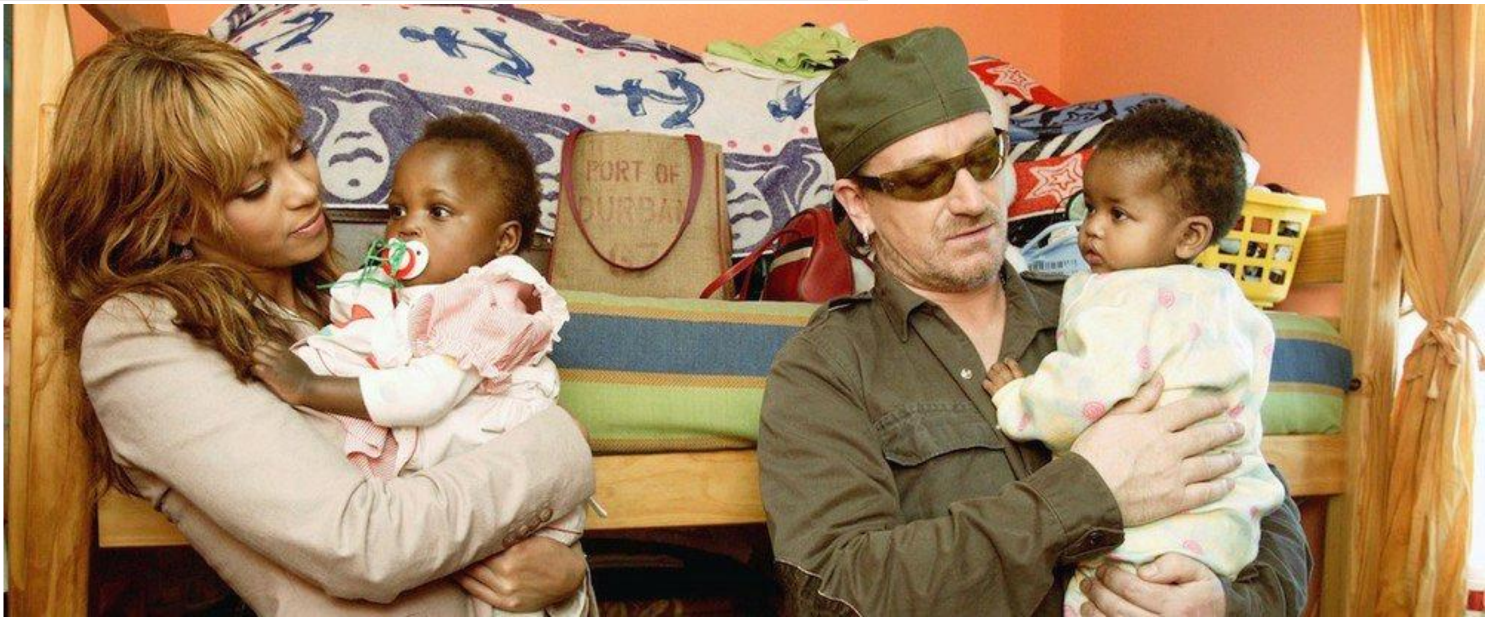

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Stop Trying to Save the World

Big ideas are destroying international development

By **Michael Hobbes**

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It seemed like such a good idea at the time: A merry-go-round hooked up to a water pump. In rural sub-Saharan Africa, where children are plentiful but clean water is scarce, the PlayPump harnessed one to provide the other. Every time the kids spun around on the big colorful wheel, water filled an elevated tank a few yards away, providing fresh, clean water anyone in the village could use all day.

PlayPump International, the NGO that came up with the idea and developed the technology, seemed to have thought of everything. To pay for maintenance, the elevated water tanks sold advertising, becoming billboards for companies seeking access to rural markets. If the ads didn't sell, they would feature HIV/AIDS-prevention campaigns. The whole package cost just $7,000 to install in each village and could provide water for up to 2,500 people.

The donations gushed in. In 2006, the U.S. government and two major foundations pledged $16.4 million in a public ceremony emceed by Bill Clinton and Laura Bush. The technology was touted by the World Bank and made a cameo in America's 2007 Water for the Poor Act. Jay-Z personally pledged $400,000. PlayPump set the goal of installing 4,000 pumps in Africa by 2010. "That would mean clean drinking water for some ten million people," a "Frontline" reporter announced.

By 2007, less than two years after the grants came in, it was already clear these aspirations weren't going to be met. A UNICEF report found pumps abandoned, broken, unmaintained. Of the more than 1,500 pumps that had been installed with the initial burst of grant money in Zambia, one-quarter already needed repair. *The Guardian* said the pumps were "reliant on child labour."

PlayPumps were going to harness the energy of children to provide fresh water to sub-Saharan African villages. They didn't. Gideon Mendel/Corbis

In 2010, “Frontline” returned to the schools where they had filmed children laughing on the merry-go-rounds, splashing each other with water. They discovered pumps rusting, billboards unsold, women stooping to turn the wheel in pairs. Many of the villages hadn’t even been asked if they wanted a PlayPump, they just got one, sometimes replacing the handpumps they already had. In one community, adults were paying children to operate the pump.

Let’s not pretend to be surprised by any of this. The PlayPump story is a sort of *Mad Libs* version of a narrative we’re all familiar with by now: Exciting new development idea, huge impact in one location, influx of donor dollars, quick expansion, failure.

I came across the PlayPump story in Ken Stern’s *With Charity For All,* but I could have plucked one from any of the dozen or so “development doesn’t work” best-sellers to come out in the last ten years. In *The Idealist*—a kind of “where are they now?” for the ideas laid out in Jeffrey Sachs’s *The End of Poverty*—Nina Munk discovers African villages made squalid by the hopes and checkbooks of Western do-gooders. Esther Duflo and Abhijit Banerjee’s *Poor Economics* finds dozens of “common sense” development projects—food aid, crop insurance, microfinance—either don’t help poor people or may even make them poorer.

International development is getting it from all sides. Governments and rich people (“major donors” in NGO-ese) are embracing terms like “philanthrocapitalism,” “social entrepreneurship,” and “impact bonds,” arguing that donations are investments, not gifts. Australia and Canada have done away with their international development agencies altogether, absorbing them into mega-ministries covering foreign affairs and trade.

I am conflicted about this moment. I have worked at international development NGOs almost my entire career (primarily at two mid-sized human rights organizations—one you’ve probably heard of and one you probably haven’t). I’ve been frustrated by the same inefficiencies and assumptions of my sector that are now getting picked apart in public. Like the authors, donors, and governments attacking international development, I’m sometimes disillusioned with what my job requires me to do, what it requires that I demand of others.

Over the last year, I read every book, essay, and roman à clef about my field I could find. I came out convinced that the problems with international development are real, they are fundamental, and I might, in fact, be one of them. But I also found that it's too easy to blame the PlayPumps of the world. Donors, governments, the public, the media, aid recipients themselves—they all contribute to the dysfunction. Maybe the problem isn't that international development doesn't work. It's that it can't.

In the late '90s, Michael Kremer, then an economics professor at MIT, was in Kenya working on an NGO project that distributed textbooks to schools in poor rural districts. Around that time, the ratio of children to textbooks in Kenya was 17 to 1. The intervention seemed obvious: Poor villages need textbooks, rich donors have the money to buy them. All we have to do is link them up.

But in the early stages of the project, Kremer convinced the researchers to do it differently. He wanted to know whether giving kids textbooks actually made them better students. So instead of handing out books and making a simple before-and-after comparison, he designed the project like a pharmaceutical trial. He split the schools into groups, gave some of them the "treatment" (i.e., textbooks) and the others nothing. Then he tested everyone, not just the kids who got the books but also the kids who didn't, to see if his intervention had any effect.

It didn't. The trial took four years, but it was conclusive: Some of the kids improved academically over that time and some got worse, but the treatment group wasn't any better off than the control.

Then Kremer tried something else. Maybe the kids weren't struggling in school because of what was going on in the classroom, but because of what was going on outside of it. So again, Kremer split the schools into groups and spent three years testing and measuring them. This time, the treatment was an actual treatment—medication to eradicate stomach worms. Worm infections affect up to 600 million children around the world, sapping their nutrition and causing, among other things, anemia, stomachaches, and stunting.

Once more, the results were conclusive: The deworming pills made the kids noticeably better off. Absence rates fell by 25 percent, the kids got taller, even their friends and families got healthier. By interrupting the chain of infection, the treatments had reduced worm infections in entire villages. Even more striking, when they tested the same kids nearly a decade later, they had more education and earned higher salaries. The female participants were less likely to be employed in domestic services.

And compared with Kremer's first trial, deworming was a bargain. Textbooks cost $2 to $3 each. Deworming pills were as little as 49 cents. When Kremer calculated the kids' bump in lifetime wages compared with the cost of treatment, it was a 60-to-1 ratio.

This is perfect TED Talk stuff: Conventional wisdom called into question, rigorous science triumphing over dogma. As word of Kremer's study spread, he became part of a growing movement within international development to subject its assumptions to randomized controlled trials.

Dozens of books and articles (and yes, TED Talks) have tracked the rise of the randomistas, as they've come to be called. The most prominent of these, and the most fun to read, is *Poor Economics,* sort of the *Principia Mathematica* of "obvious" development interventions tested and found wanting.

If someone is chronically malnourished, to pick just one example, you should give them some food, right? Duflo and Banerjee describe dozens of projects finding that, when you subsidize or give away food to poor people, they don't actually eat more. Instead, they just replace boring foods with more interesting ones and remain, in the statistics at least, "malnourished."

In Udaipur, India, a survey found that poor people had enough money to increase their food spending by as much as 30 percent, but they chose to spend it on alcohol, tobacco, and festivals instead. Duflo and Banerjee interviewed an out-of-work Indonesian agricultural worker who had been under the food-poverty line for years, but had a TV in his house.

You don't need a Ph.D. to understand the underlying dynamic here: Cheap food is boring. In many developing countries, Duflo and Banerjee found that even the poorest people could afford more than 2,000 calories of staple foods every day. But given the choice between the fourth bowl of rice in one day and the first cigarette, many people opt for the latter.

Even in countries where development projects worked, where poor people went from hungry to nourished, they weren't more likely to get a job or make significantly more money. All the appealing metaphors of NGO websites and academo-best-sellers—"the poverty trap," "the ladder of development"—go limp under the magnifying glass of actually being tested.

Deworming treatment had impressive results on education in Kenya—but programs elsewhere aren't being as rigorously monitored. EvidenceAction/Stephanie Skinner

Armed with his rigorously gathered results, Kremer founded an NGO, Deworm the World. He launched it at the 2007 World Economic Forum and committed to deworming ten million children. He was feted by the Clinton Global Initiative; GlaxoSmithKline, and Johnson & Johnson pledged $600 million worth of deworming treatments a year, enough for every infected primary school student in Africa. The World Health Organization issued a statement of support. Kenya asked him to help create a national program to deworm 3.6 million children. Two states in India initiated similar programs, aiming to treat millions more. The organization now claims to have helped 40 million children in 27 countries.

But wait a minute. Just because something works for 30,000 students in Kenya doesn't mean it will work for millions of them across Africa or India. Deworm the World's website talks a lot about its "evidence-based" approach. (It has now been folded into an NGO called Evidence Action.) Yet the primary evidence that deworming improves education outcomes is from Kremer's single Kenya case and a post-hoc analysis of deworming initiatives in the American South in 1910. In 2012, the organization said that it had treated 17 million children in India, but didn't report whether their attendance, school performance, or graduation rates improved.

I keep thinking I'm missing something really obvious, that I'm looking at the wrong part of their website. So I call up Evidence Action and ask: Are you guys really not testing how deworming affects education anymore?

"We don't measure the effects on school attendance and school performance," says Alix Zwane, Evidence Action's executive director. At the scale they're going for in India, entire states at a time, splitting into control and treatment groups simply wouldn't be feasible.

Kremer tells me that enough trials have been done to warrant the upscaling. "There's more evidence for this than the vast majority of things that governments spend money on." Every time you want to build a new road, you can't stop to ask, Will this one really help people get from place to place?

"Meanwhile," he says, "there's a cohort of children that, if you don't implement the policy now, will go through years of schooling without treatment."

It's an interesting question—when do you have enough evidence to stop testing each new application of a development idea?—and I get that you can't run a four-year trial every time you roll out, say, the measles vaccine to a new country. But like many other aid projects under pressure to scale up too fast and too far, deworming kids to improve their education outcomes isn't the slam-dunk its supporters make it out to be.

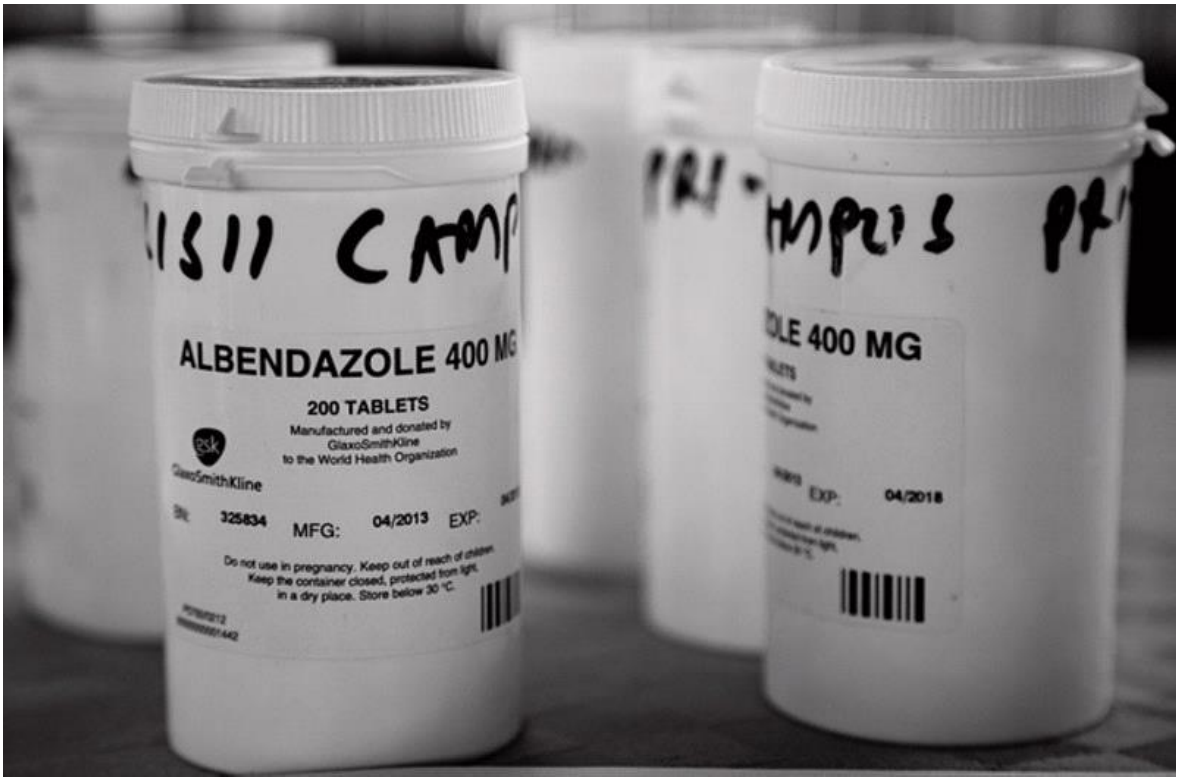
In 2000, the *British Medical Journal* (*BMJ*) published a literature review of 30 randomized control trials of deworming projects in 17 countries. While some of them showed modest gains in weight and height, none of them showed any effect on school attendance or cognitive performance. After criticism of the review by the World Bank and others, the *BMJ* ran it again in 2009 with stricter inclusion criteria. But the results didn't change. Another review, in 2012, found the same thing: "We do not know if these programmes have an effect on weight, height, school attendance, or school performance."

Kremer and Evidence Action dispute the way these reviews were carried out, and sent me an upcoming study from Uganda that found links between deworming and improved test scores. But the evidence they cite on their own website undermines this data. Kremer's 2004 study reporting the results of the original deworming trial notes—in the abstract!—that "we do not find evidence that deworming improves academic test scores," only attendance. Another literature review cited on Deworm the World's website says, "When infected children are given deworming treatment, immediate educational and cognitive benefits are not always apparent."

Then there's the comparison to textbooks. Kenya, it turns out, is a uniquely terrible place to hand out textbooks to kids and expect better academic performance. When Kremer reported that textbooks had no overall effect, he also noted that they did actually improve test scores for the kids who were already at the top of the class. The main problem, it seems, was that the textbooks were in English, the second or third language for most of the kids. Of the third-graders given textbooks, only 15 percent could even read them.

In the 1980s and early '90s, a series of meta-analyses found that textbooks were actually effective at improving school performance in places where the language issues weren't as complex. In his own paper reporting the Kenya results, Kremer noted that, in Nicaragua and the Philippines, giving kids textbooks did improve their test scores.

But the point of all this is not to talk shit on Kremer—who has bettered the world more with his career than I ever have with mine—or to dismantle his deworming charity, or to advocate that we should all go back to giving out free textbooks. What I want to talk shit on is the paradigm of the Big Idea—that once we identify the correct one, we can simply unfurl it on the entire developing world like a picnic blanket.



EvidenceAction/Stephanie Skinner

There are villages where deworming will be the most meaningful education project possible. There are others where free textbooks will. In other places, it will be new school buildings, more teachers, lower fees, better transport, tutors, uniforms. There's probably a village out there where a PlayPump would beat all these approaches combined. The point is, we don't know what works, where, or why. The only way to find out is to test these models—not just before their initial success but afterward, and constantly.

I can see why it's appealing to think that, once you find a successful formula for development, you can just scale it up like a Model T. Host governments want programs that get more effective as they get bigger. Individual donors, you and me, we want to feel like we're backing a plucky little start-up that is going to save the world. No international institution wants to say in their annual report: "There's this great NGO that increased attendance in a Kenyan school district. We're giving them a modest sum to do the same thing in *one other district* in one other country."

The repeated "success, scale, fail" experience of the last 20 years of development practice suggests something super boring: Development projects thrive or tank according to the specific dynamics of the place in which they're applied. It's not that you test something in one place, then scale it up to 50. It's that you test it in one place, then test it in another, then another. No one will ever be invited to explain that in a TED talk.

The last NGO I worked for had 150 employees and a budget of more than $25 million. Employees were divided into "program staff" (the people researching, coordinating, and implementing our mission) and "overhead staff" (the fund-raising, human resources, and accounting departments helping them do it). Like most NGOs, we bragged to our donors that we had low overhead, that their dollars and euros and kroner and francs went to "the cause" and not to our rent or our heating bills. And this was, at least on the Excel sheets, true. Most of our money went to researcher and project manager salaries. The fund-raising, H.R., and accounting departments could have each fit comfortably in a minivan.

The problem is, those overhead tasks don't disappear just because you don't spend money on them. Someone has to monitor the accounts, find new donors, calculate taxes, organize the holiday party. Centralizing these tasks in dedicated departments, hiring specialists, getting good at them, that would have looked like bureaucracy. So instead, we spun them out to the entire staff: We assigned researchers and project managers—anthropology majors mostly, some law school dropouts—to do our H.R., accounting, fund-raising, and project evaluations.

The outcome was as chaotic as it sounds. Want to hire someone? You'll need to write your own job ad, find job boards to post it to, and, in some cases, update the standard employment contract yourself. Want to issue a press release about the results of the study you just performed? Write it yourself and start sending it to journalists. Hopefully you know a few.

The downsides of this approach were most obvious in fund-raising. If there's one thing donors hate, it's paying us to find more donors. So every program staffer was responsible for raising (and accounting, and monitoring, and reporting) funds for their own projects. Staff members spent days doing the same donor research ("which foundations fund work on water scarcity?") that a colleague across the hall did last week. Without a centralized staff to coordinate pitches, we contacted the same donors dozens of times with small-fry requests rather than combining them into one coherent "ask." (One employee, legend had it, asked Google if they could Google Translate our website as an in-kind donation.)

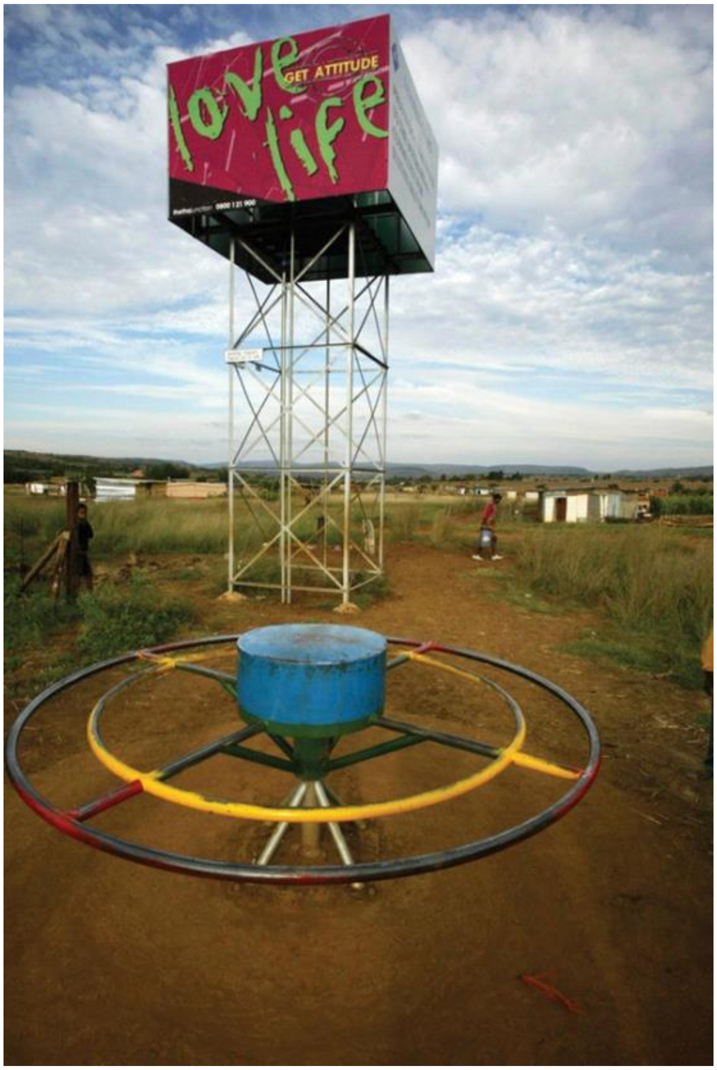
No one had any expertise in writing grant proposals, conducting impact assessments, or managing high-maintenance funders like the European Commission—training courses would have counted as overhead spending. We missed opportunities for new funding, we bungled contracts we already had, and we turned donors against us. Every staff meeting, one or two people announced they were leaving. "I wasn't hired to spend my day fund-raising" were the most common eight words at farewell parties.

My experience wasn't unique. Stern cites the example of the American Red Cross, which sent confused volunteers, clueless employees, and, bafflingly, perishable Danish pastries to the Gulf Coast after Hurricane Katrina because it hadn't invested in training its U.S. staff in actual crisis response. A buddy of mine works at an NGO with 150 staff where the H.R. department is exactly one person, and she's also the receptionist.

It's understandable that donors are paranoid about overhead. The last few years have seen charity after charity busted for blowing donations on corporate junkets, billboard advertising, and outright fraud. Some breast cancer charities pay telemarketing companies 90 cents of each dollar they raise just to raise it. Greg Mortenson, he of the *Three Cups of Tea* school-building empire, had to pay $1 million back to his own charity when a Jon Krakauer exposé revealed that he was spending donations on a never-ending book tour and pocketing the proceeds.

Dan Pallotta, who spent the '90s and 2000s running a $300 million breast cancer and AIDS charity, has produced two books arguing that this obsession with overhead keeps charities from reaching the scale required to take on large problems. Pallotta uses the example of two soup kitchens: One spends 60 cents of every donation dollar on "programs" (i.e., soup), while the other spends 90 cents.

According to the conventional wisdom of donors and charity rating agencies, your donation is better spent on the organization where only 10 percent of spending goes to overhead. But using this one number ignores much more important indicators of the charity's impact. Is the soup nutritious and warm? Is it getting to the right people? Does the kitchen open on time every day and have kind, professional staff? And, hang on, do free warm meals even help people escape poverty? Providing decent service, targeting handouts, testing these assumptions—these things cost money, whether donors like it or not.



Paul Hackett/Corbis

So charities hide overhead, like we did, in overburdened program staff, untrained volunteers, and external consultants. Just as deworming millions of children is different in kind, not degree, from deworming a village of them, running a large, professional charity is completely different from running a new, start-uppy one. Small-scale projects (installing one PlayPump, say) can keep their overhead low through charismatic leaders, passionate staff, and long-standing relationships with the communities they're seeking to assist. Large-scale projects require stuff like budget managers, reporting frameworks, light bulbs, and, yes, a goddamn holiday party.

Pallotta's *Uncharitable* has a nice example of what this looks like. His first cross-country AIDS ride had 39 cyclists and almost zero overhead. The group was small enough to sleep in gymnasiums, to rely on churches and good samaritans to provide food and hot showers. If supplies fell short, they could knock on doors asking for help or, in a pinch, put up their tents in backyards. He raised $80,000.

By the 2000s, the rides were attracting an average of 3,000 riders. A group that size requires a logarithmic increase in organization and support—renting out whole campgrounds, professional catering, dedicated medical and legal staff. Overhead costs ballooned to 42 percent of each donation. But each ride raised $7 million.

As with the actual aid projects themselves, the success of a charity depends on specifics, not a single, one-size-fits-all indicator. Charities do all kinds of stuff—conduct research, train local NGOs, build infrastructure, give away goats. For donors to truly determine how well they're doing it, they'd need to come up with a customized report card for each charity.

For a soup kitchen, it would be the stuff I just mentioned: *Do they open on time? How's their soup?* For an NGO that, say, monitors government infrastructure projects for corruption, it would be things like, *What percentage of projects are they assessing? Are their assessments yielding correct information? Is this information being communicated to the communities affected by corruption?*

Judging charities like this, on the impacts of their work and whether they're addressing the problem they set out to solve, yields qualitative information, sentences, and observations that can't be compared across charities. Given the millions of international development NGOs with their upside-down hats out (the IRS, Stern notes, approves 99.5 percent of charity applications), it's faster and easier to measure them all by the same standard.

This is why donors love overhead. It's one number that allows you to compare the soup kitchen with the anti-corruption think tank. It smells all rigorous and objective, but it doesn't require any actual work. Charities provide their own overhead figures, after all, just like they write their own annual reports and produce their own little Kony 2012 fund-raising videos. International development NGOs aren't always obligated to issue audited accounts. Some of them report no overhead at all, the institutional equivalent of "I didn't inhale."

I'm not going to propose a cute little solution here to make this easier for donors, or suggest some "right" overhead percentage. For most charities, 10 percent overhead probably isn't enough, and 90 percent is just fucking around. But the whole point is that we shouldn't pick just one number to stand in for efficiency. We're always arguing that, if rich countries want to solve the problems of poor ones, they're going to have to spend time getting to know them. It's time we apply the same logic to the agencies we dispatch to do the job.

Dertu isn't a place very many people go on purpose. Located in northeastern Kenya, close to the Somali border, and next door to a sprawling refugee camp, in 2004 it was little more than a rest stop, a place for the local pastoralists to refresh their animals and catch up on local news. Its chief attraction was fresh water from a UNICEF-drilled borehole in the clay. Of the few thousand people living there permanently, more than 80 percent relied on food aid. Ninety percent were illiterate.

This is the "before" picture of Dertu that Jeffrey Sachs found when he initiated his Millennium Villages Project there in 2006. Sachs, a professor at Columbia University, became a Bono-approved development celebrity with his book *The End of Poverty,* a screed against the rich world's complacency in letting easily solvable problems—malaria, literacy, clean water—damn an entire continent to misery.

Sachs's book tour culminated in the establishment of the Millennium Villages Project, an ambitious plan to jump-start development with a huge influx of cash, in-kind support, and infrastructure to some of the poorest settlements in the world. Sachs's premise was that millions of people, dozens of countries, had fallen into the "poverty trap": Living in substandard housing leads to problems concentrating at school. Which leads to not graduating. Which leads to working in low-skilled jobs. Which leads to living in substandard housing. And on and on.

The only solution, Sachs argued, was to dramatically boost people to a level where they could start to develop themselves.

This is, it turns out, an incredibly persuasive idea, and in the two years after the book came out, Sachs raised $120 million (including $50 million from George Soros's personal checkbook) and identified 14 villages throughout sub-Saharan Africa to test his theory.

As described in Nina Munk's *The Idealist: Jeffrey Sachs and the Quest to End Poverty,* things looked promising in Dertu at first. Sachs convinced GE and Ericsson to donate medical equipment and cell phones. He hired local managers who knew the culture and language to ensure his project was responding to Dertu's needs. His teams built housing, schools, roads, health clinics. They set up a livestock market to attract farmers from all over the region.

But soon, the momentum faltered. Without electricity to run it or specialists to maintain it, the advanced medical equipment gathered dust—in Kenya, that means literally. The managers of the project, so knowledgeable about the local culture and mores, eventually succumbed to them, doling out benefits on the basis of tribal favoritism and tit-for-tat back-scratching. The borehole broke down and water had to be shipped in by truck.

The core of the problem, as Munk describes it, was that Dertu became a sort of company town, with the Millennium Villages Project providing the only reliable source of employment, benefits, and public services. Thousands of new residents came from the nearby refugee camp and other parts of Kenya, seeking jobs or handouts. Where Dertu was once a stopover for nomads, the influx of donor money, the improved infrastructure, the free housing and education and health care, had given people a reason to stay. Sachs's funding couldn't keep up. And eventually, it ran out.

In an interview about her book for *EconTalk,* Munk describes what Dertu looked like the last time she saw it, in 2011:

> They were now really living in a kind of squalor that I hadn't seen on my first visit. Their huts were jammed together; they were patched with those horrible polyurethane bags that one sees all over Africa. ... There were streams of slop that were going down between these tightly packed huts. And the latrines had overflowed or were clogged. And no one was able to agree on whose job it was to maintain them. And there were ditches piled high with garbage. And it was just—it made my heart just sink.

This is the paradox: When you improve something, you change it in ways you couldn't have expected. You can find examples of this in every corner of development practice. A project in Kenya that gave kids free uniforms, textbooks, and classroom materials increased enrollment by 50 percent, swamping the teachers and reducing the quality of education for everyone. Communities in India cut off their own water supply so they could be classified as "slums" and be eligible for slum-upgrading funding. I've worked in places where as soon as a company sets up a health clinic or an education program, the local government disappears—why should they spend money on primary schools when a rich company is ready to take on the responsibility?

There's nothing avaricious about this. If anything, it demonstrates the entrepreneurial spirit we're constantly telling the poor they need to demonstrate.

My favorite example of unintended consequences comes, weirdly enough, from the United States. In a speech to a criminology conference, Nancy G. Guerra, the director of the Institute for Global Studies at the University of Delaware, described a project where she held workshops with inner-city Latina teenagers, trying to prevent them from joining gangs. The program worked in that none of the girls committed any violence within six months of the workshops. But by the end of that time, they were all, each and every one, pregnant.

"That behavior was serving a need for them," she says in her speech. "It made them feel powerful, it made them feel important, it gave them a sense of identity. ... When that ended, [they] needed another kind of meaning in their lives."

The fancy academic term for this is "complex adaptive systems." We all understand that every ecosystem, each forest floor or coral reef, is the result of millions of interactions between its constituent parts, a balance of all the aggregated adaptations of plants and animals to their climate and each other. Adding a non-native species, or removing one that has always been there, changes these relationships in ways that are too intertwined and complicated to predict.

Jeffery Sachs, friend of Bono and director of the Millennium Villages Project, addresses more than 4,000 people in Uganda. Guillaume Bonn/Corbis

According to Ben Ramalingam's *Aid on the Edge of Chaos,* international development is just such an invasive species. Why Dertu doesn't have a vaccination clinic, why Kenyan schoolkids can't read, it's a combination of culture, politics, history, laws, infrastructure, individuals—all of a society's component parts, their harmony and their discord, working as one organism. Introducing something foreign into that system—millions in donor cash, dozens of trained personnel and equipment, U.N. Land Rovers—causes it to adapt in ways you can't predict.

A friend of mine works at an NGO that audits factories in India and China, inspecting them for child labor, forced labor, human-trafficking, everything celebrities are always warning us about. I asked him if, after ten years of inspections, conditions have gotten any better. "Yes and no," he said. "Anytime you set a standard, some companies will become sophisticated to meet it, and others will become sophisticated to avoid it."

So international development sucks, right? I've just spent thousands of words telling you all the ways the incentives of donors, recipients, and NGOs contradict each other. Why not just scrap it altogether?

Because I don't think that's the conclusion these examples suggest. I think they suggest something much less dramatic: It's not that development is broken, it's that our expectations of it are.

First, let's de-room this elephant: *Development has happened.* The last 50 years have seen about the biggest explosion of prosperity in human history. China, India, Taiwan, South Korea, Turkey, Mexico—these aren't the only countries where you'd rather be born now than 50 years ago. Even the poorest countries in the world—Burundi, Somalia, Zimbabwe—are doing way better on stuff like vaccinations and literacy than they did earlier in our own lifetimes.

You sometimes hear this Cambrian proliferation of well-being as an argument against development aid, like: "See? China got better *all by itself.*" But the rise of formerly destitute countries into the sweaters-and-smartphones bracket is less a refutation of the impact of development aid than a reality-check of its scale. In 2013, development aid from all the rich countries combined was $134.8 billion, or about $112 per year for each of the world's 1.2 billion people living on less than $1.25 per day. Did we really expect an extra hundred bucks a year to pull anyone, much less a billion of them, out of poverty?

Development, no matter how it happens, is a slow process. It wasn't until about 30 years after Mao's death that China's per capita GDP reached lower-middle-income status. The country's growth is arguably the fastest of any country's since we, as a species, started gathering economic statistics. Even in the most cartoonishly successful scenario imaginable, countries like the Central African Republic (per capita GDP: $700, adjusted for purchasing power), Burundi ($600), and the Democratic Republic of Congo ($400) will take decades just to reach the point where China is now.

The ability of international development projects to speed up this process is limited. Remember how I said the deworming project had a 60-to-1 ratio between the price of the pills and the increase in wages for the kids who got them? The increase was $30. Not $30 per year. The kids earned $30 more *over their lifetimes* as a result of the deworming treatment. You find this a lot in the development literature: Even the most wildly successful projects decrease maternal mortality by a few percent here, add an extra year or two of life expectancy there.

This isn't a criticism of the projects themselves. This is how social policy works, in baby steps and trial-and-error and tweaks, not in game changers. Leave the leaps and bounds to computing power. If a 49-cent deworming treatment really does produce a $30 increase in wages for some of the poorest people on Earth, we are assholes for not spending it.

And this is where I landed after a year of absorbing dozens of books and articles and speeches about international development: The arguments against it are myriad, and mostly logistical and technical. The argument for it is singular, moral, and, to me anyway, utterly convincing: *We have so much, they have so little.*

If we really want to fix development, we need to stop chasing after ideas the way we go on fad diets. Successful programs should be allowed to expand by degrees, not digits (direct cash payments, which have shown impressive results in Kenya and Uganda, are a great candidate for the kind of deliberate expansion I'm talking about). NGOs need to be free to invest in the kinds of systems and processes we're always telling developing countries to put in place. And rich countries need to spend less time debating how to divide up the tiny sliver of our GDP we spend on development and more time figuring out how to leverage our vast economic and political power to let it happen on its own.

As Owen Barder, a senior fellow at the Center for Global Development (from whom I stole many of the ideas in this essay), puts it:

> If we believe that trade is important, we could do more to open our own markets to trade from developing countries. If we believe property rights are important, we could do more to enforce the principle that nations, not illegitimate leaders, own their own natural resources. ... If we believe transparency is important, we could start by requiring our own companies to publish the details of the payments they make to developing countries.

PlayPump International, the charity I started with, doesn't exist anymore. The pumps, however, are still being installed by Roundabout Water Solutions, an NGO that markets them as a "niche solution" that should only be installed at primary schools in poor rural areas. Four years ago, the same evaluations that so harshly criticized the rapid expansion of the project also acknowledged that, in some villages, under the right circumstances, they were fabulously helpful.

In 2010, "Frontline" interviewed the director of PlayPump about its failures, and he said, "It might have been a bit ambitious, but hey, you gotta dream big. Everyone's always said it's such a great idea."

And it was. But maybe when the next great idea comes along, we should all dream a little smaller.

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