

45

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Laya Maheshwari – The Next Big Thing Is
a Series of Small Things

THE NEXT BIG THING IS A SERIES OF SMALL THINGS

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Laya Maheshwari is an Indian student currently enrolled in the MSc in Social Policy and Planning programme at the London School of Economics (LSE) and Political Science. For his dissertation, he is analysing state textbooks in India for possible discrimination and propagandistic ideologies against Islam. While at LSE, he has received the StockWell Communications Polis Research Prize to undertake research on the uses and benefits of negative communication, for which he shall use a behavioural psychology framework. For pursuing his graduate studies, he was named one of four KC Mahindra Fellows from all over India. His undergraduate alma mater is St. Xavier's College, Mumbai, where he majored in journalism. When time allows, he writes about film and culture. His work has appeared in publications like *The Guardian*, *The LA Review of Books*, *The Times of Israel*, and *Playboy*. In September, he will become part of the world's first batch of Yenching Scholars, Peking University's new fellowship for 100 individuals interested in studying China.

Thinking of *Big Ideas* is tempting. It is an unbeatable addiction to imagine a magic pill or silver bullet: finding that one *thing* we are doing wrong but can correct with a single change and consequently improve circumstances drastically. And everyone loves big ideas: they are flashy; they grab attention. A big idea gets its creator headlines, funding grants, TED Talks, and — who knows — maybe even a Nobel Prize. When was the last time a book about a small, boring, potentially insignificant idea turned into a bestseller?

The task of changing the world, the present would have us believe, is one of high stakes: requiring massive ambition and offering large rewards. And there is no room for those who cannot dream big.

However, this essay will articulate a different argument. It operates on the premise that if something sounds too good to be true, it probably is. Something that fits the format of a TED Talk, i.e. one

pathbreaking approach that challenges conventional dogma and exposes miraculous truths about society, may be simplifying the problem itself. It wonders what are the ramifications of big ideas: Are they worth the hype? And it suggests that perhaps the next big idea should be an acknowledgment of the fallibility of big ideas.

A BIG IDEA GONE WRONG

If one were to think of the two biggest problems facing Africa today, many would pipe up to decry the lack of clean water and the difficult conditions for African children. A few years ago, one big idea came along that sought to hit these two birds with one stone — or merry-go-round, if you will. The vision was simple: place a merry-go-round in an African village. Connect it to an overhead water tank a few metres away. As children play on the merry-go-round, the kinetic energy is used to pump underground

water, filling the tank. And since the tank is connected to a tap valve, villagers can access clean water easily. Children have space to play; they and their parents have water to drink.

The people responsible, a company named PlayPumps International, had seemingly thought of everything. The tank, seven metres above ground, would have billboards for advertising on two sides and HIV/AIDS prevention campaigns on the other two.

They won the right attention. PlayPump received the World Bank Development Marketplace Award in 2000. Six years later, the US Government announced \$10 million in support. Celebrities were enamoured too; American rapper Jay Z pledged \$250,000.

However, things did not go according to plan. A 2009 investigative report in *The Guardian* found that many PlayPumps were lying disused; children were not playing on them and the tanks did not

contain any water. What happened?

The original aim, a PlayPump press release stated, was to install pumps in 4,000 African villages, with the eventual goal of fulfilling 10 million people's water needs. This led to such aggressive installation of units that, in some sites, the residents were not even consulted. Some villagers in Mozambique complained that a PlayPump replaced the community's earlier water-drawing apparatus without their approval.

In hindsight, the *goal* itself seems chimerical. According to the Sphere Project, an average human requires 15 litres of water daily. For satisfying 10 million people's requirements, a typical PlayPump would have had to be played 27 hours a day, according to Andrew Chambers' calculations in *The Guardian*. Moreover, water is most acutely required in the morning's early hours; getting children to play at this time was an unforeseen obstacle. In many places, the adults seeking water had to move the merry-go-round themselves. This created additional difficulties for elderly women.

Not only was the PlayPump model based on flawed water demand assessment, too complex for local maintenance, and reliant on child labour, but it was also too expensive. For the cost of one PlayPump unit, four wells with conventional hand-pumps could have been dug up. After numerous complaints by Mozambican villagers about dysfunctional units, PlayPumps International had to appease them with their traditional hand-pumps, bringing things back to square one — only with massive fund wastage en route.

KNOWING THE CAUSES OF THINGS

The PlayPump fiasco would be worthy of outrage even if it were an outlier, but it is merely an addition to a long list of novel-sounding plans that pan out terribly in reality. The chain of events is

similar: Someone envisions an idea that has never been thought of, designs a flowchart depicting its power, and attains enough resources to implement it on the ground. Some time later, everyone is taken aback when the idea does not revolutionise the world. And they go back to the drawing board.

It would do investors, governments, and concerned citizens good to look at the causes of these repeated failures. Hopefully, the lessons learnt would prevent future disasters.

In *Aid on the Edge of Chaos*, Ben Ramalingam writes about "complex adaptive systems." Any society in the world does not exist in a vacuum. A multifaceted, intertwined web of factors influences its characteristics. For example, a community in rural India may be poor not just because of a plague, but a combination of economic, religious, cultural, geographical, historical, and political reasons. This community, along with all communities in every part of the world, is an ecosystem. It is diverse, unpredictable.

Many big ideas in international development and aid today come from a mindset of underlying condescension. They assume that the recipient of the idea is a blank canvas. The glorious solution can be *injected* into the intended site; it will heal the rupture it is targeted at, and everything will be perfect after. They see progress as a one-way street, when it is anything but.

Jeffrey Sachs, a professor at Columbia University, is a big proponent of the power of foreign aid. He claimed in his book *The End of Poverty* that several African nations are stuck in a *poverty trap*, which they could be lifted from if they just had more resources at hand.

He founded the Millennium Villages Project, aimed at eradicating poverty from sub-Saharan countries through targeted interventions in agriculture,

medicine, education etc. The initiative commenced in Dertu, a town in north-east Kenya, where Sachs used part of his \$120 million donations to build housing, schools, roads, a livestock farmers' market, and health clinics.

However, the Project did not account for the people reacting to this external intervention and changing the status quo. Kenyans from nearby villages flocked to Dertu, overloading its public infrastructure and limited resources. The newly built facilities broke down, and the town ended up worse than before. After witnessing life in Dertu, Nina Munk, who wrote the book *The Idealist: Jeffrey Sachs and the Quest to End Poverty*, said in an interview that "the situation made my heart sink."

FOLLOWING THE RIGHT STEPS

The preponderance of such projects reveals another dangerous inclination of policy: to rely on process above all else. Several big ideas are about following certain steps and expecting certain results, a notion that can be summarised as: "If I do this, I will get that." This is hazardous because of its reductionism; it assumes linearity and causality on the recipient's side even though common sense and empirical research tell us that is untrue.

The PlayPump and Dertu scenarios both reflect their creators' ex ante optimism bias, but a more important take-away is that we should not confuse *outputs* with *outcomes*. An installed water tank may not lead to water storage; laying roads may not ensure smoother connectivity.

This problem is especially pernicious in education. The Millennium Development Goals are rife with big ideas, one of them achieving universal primary education. Governments worldwide have responded by making enrolment in school till a certain age compulsory. However, such

targets assume that enrolment is equivalent to education, which is equivalent to learning. In *The Rebirth of Education*, Lant Pritchett argues “schooling is not the same as learning.” He cites a survey in Andhra Pradesh, a state in South India, which found that only one in 20 children in grade five could do basic arithmetic. Thankfully, this misconception is now being remedied in official visions. UNESCO’s *Global Monitoring Report 2012* admits that “education is not only about making sure all children can attend school.”

Why, then, does this misguided focus still persist? In one of his most publicised experiments, Michael Kremer, a professor at Harvard University, distributed free textbooks to Kenyan schoolchildren — a succinct big idea. The logic being: if you give children textbooks, surely learning shall follow. The intervention was a twofold failure. The distribution of textbooks did not improve learning, because they were in English — a language many children could not read. They actually exacerbated inequalities; upper-class children could read English and had yet another advantage over their peers.

Kremer later performed a follow-up study, buffeted by another Big Idea. He conducted a randomised control trial (RCT) in Kenyan villages to test the impact of deworming pills on children’s learning outcomes. This experiment was apparently a massive success, and led to Kremer *scaling up* the project by founding an NGO, Deworm the World. The World Health Organisation issued a statement of support; GlaxoSmithKline and Johnson & Johnson promised \$600 million worth of treatment per year. Just making sure children are dewormed would enhance their classroom performance and life outcomes, or so it was hoped.

However, the abstract of the original 2004 study admits that deworming did

not lead to an improvement in learning. A review of evidence published in the *British Medical Journal* also concluded that there was no concrete linkage between deworming and educational attainment. This is yet another example of a Big Idea — one so blindingly obvious it seems a winner upon inception — not actually achieving its intended effect.

THE WAY OUT?

The allure of focusing on processes is understandable. Adhering to a *checklist* is easy, for one. It is also hard to debate the logic of using rich people’s money to offer poor children textbooks and medicines they cannot afford and then expect an improvement in the latter’s lives. It is beneficial in numerous ways to prefer tangible manifestations of policy and aid. If you build a health clinic, there is an instant and obvious payoff to your investment. Everyone can see you have been doing something. This is particularly important for elected representatives and donor-dependent agencies: their hard work is on public display.

But, what these policymakers need right now is a daring turn. They must display a willingness to forgo the visible forms of *work* for invisible ones. They should resist the temptation of the sexy, headline-making big idea and instead concentrate on the mundane small ideas that have been relegated to the background. The next revolution will not come from an amazing, new approach; it will come by welcoming back into the fold the obvious truths we are ignoring at our own peril.

At the time of the PlayPump rollout, there were around 375,000 hand-pumps in Africa. More than 150,000 had malfunctioned. Repairing those hand-pumps would have achieved much more than the PlayPumps did. *The Economist* has carried a feature on the Myanmar government’s plans to build highways in

Yangon — a perennial big idea. Yet, there are fears that this will lead to more car-ownership, corruption, and pollution. A concerned citizen told *The Economist* that the government would do better by just fixing traffic lights.

The problem is *repairing* existing things does not sound sexy. It does not get you votes. It will certainly not win you a Nobel Prize. Yet, that is what the world needs today. The development technology of the future will arise from boring topics. Building new schools and ensuring children sit inside them for 15 years makes for good photos and headlines, but the problem is actually in our education system, and how easy it is for students to slip through the cracks.

But tackling that problem would require moving beyond the comforting rigidity of a *checklist* or process. It cannot be subjected to easy quantification and eye-popping numbers or percentages. It demands probing, qualitative analysis that may yield uncomfortable truths. Checking whether we have performed steps A to Z shall no longer suffice. The impact of each step and the ramifications of its change will need exploring. It is easy to count how many days a doctor turned up at the clinic, much harder to check whether he prescribed the *right* medicine. But is there any doubt which is more important?

The problems we face — starvation, illiteracy, pollution etc. — are complex, overarching, messy, and difficult. Is it a surprise, then, that our approach to solving them may be complex, overarching, messy, and difficult?

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