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Inside the Machine

Toward a new development economics

Abhijit Vinayak Banerjee

Madame de Pompadour, mistress to King Louis XV of France, had a remarkable personal surgeon. Born François Quesnay in 1694 of laboring parents, and orphaned at age 13, he taught himself to read at age 11, read everything he could get his hands on, and ended up as one of the leading surgeons in France. In his 50s, Quesnay, then a member of France's most exclusive intellectual club, the *philosophes*, started writing about economics.

Quesnay probably did not realize that when he (or, more accurately, his amanuensis, the Marquis de Mirabeau) wrote in 1763 about what “propels the economic machine,” he was launching what would become the dominant metaphor in economics. The idea of an economic machine, self-perpetuating and existing beyond the realm of individual volition, was of course most attractive to those who, like Quesnay, wanted the government to interfere less, but its real force came from the evocation of what, for the layperson, remains the most compelling model ever offered by science—the Newtonian model of the universe.

For critics of capitalism as much as for its cheerleaders, the idea of a world governed by a small set of iron laws was irresistible. Thomas Robert Malthus, who in 1805 became Great Britain's first professor of political economy, at the East India Company's college in southern England, is usually identified with what was probably the first law in economics, the so-called iron law of wages. Malthus did not himself call it that: in his *An Essay on the Principle of Population* (1798) he described it as the “principle of population,” the idea that there is no point in trying to raise the poor's standard of living, because it just encourages them to have more children, which drives their earnings back down.

Karl Marx, writing almost 50 years later, worried more about profits than wages. He thought that “the most important law of modern political economy” was the tendency of capitalists to accumulate an excess of capital, which would drive down the rate of profits and ultimately set off a crisis.

In the nearly 150 years since the first volume of *Das Kapital* was published, in 1867, economics has evolved enormously in terms of both methods and scope, but the framing questions remain similar: Can the machine keep going on its own? Where is it headed? What happens if we try to nudge it in one direction or the other?

Quesnay, originally a farm boy, thought that the machine could in fact keep going, but only thanks to the great bounty of nature. This was what made it possible for the farmer to pay the butcher, the butcher to pay the barber, and so on. He also thought that the machine would run better if the government

stopped trying to tell it where to go.

Economists' framing questions are the same today, but the nature of the answers changed. Since about the 1950s, the norm in economics has been to start from a specific model—a specific set of assumptions about how people make decisions, how technology works, and how markets behave—and to derive, based on mathematical and quasi-mathematical reasoning, predictions about what would happen in a world defined by the model. This has the obvious and immense advantage of making it possible to give some categorical and irrefutable answers to economists' framing questions, if only within the model's circumscribed world. For example, one can actually prove that free trade works, or that monetary policy does not, at least under a particular set of assumptions. Moreover, anyone with a little bit of algebra and patience can play the game of setting up a model—often by tweaking some assumption in someone else's model and deriving new results. One does not need to be a Marx or a Keynes to have something useful to say about the great questions of the time.

Thanks to this approach, the last 50 years have been halcyon days for economists. We have learned a lot about different models, and the process of working them out has revealed many pieces of general economic logic that lurk behind them. Yet it is not clear that this process has taken us much closer to answering the basic questions about the economic machine. If you ask an economist today what the body of economic theory has to tell us about the stability of the capitalist system, or whether the poor countries of today are destined to catch up with the rich countries, or even whether free trade is better than some protection, he would throw up his hands (though in the next instant he would probably offer his own opinions).

Paradoxically, this reflects what is in many ways the great success of economic theory: there are many models, and each model offers its own distinct answer, quite often for sensible reasons. Thus, the question of whether the poor countries will eventually catch up with the rich countries turns on, among other things, whether there are increasing or decreasing returns—i.e., whether the return on investment should be expected to be higher in poor countries or in rich ones. Either seems logically possible: poorer countries have cheaper labor, which should make investment more rewarding, but they lack other forms of capital (such as infrastructure) and skills, which goes the other way. When you refer to the data, some poor countries seem to have very low returns indeed; and while others, such as Pakistan, may have somewhat higher returns than the United States, it appears that the gap is not very large (certainly not large enough to persuade investors in the United States to move large amounts of capital there). Then there are countries, like China, or some of the East Asian Tigers before it, that seem to have had no problem attracting foreign capital, and have clearly caught up, or are in the process of catching up, with the richer countries.

Is it all a matter of luck? Perhaps people are investing in China because everyone else is. Or is it something deeper? After all, despite being run by a party that calls itself communist, China offers a pro-business environment, security of property, and a docile labor force. Is this what investors are looking for? Or is it China's ability to produce a seemingly endless supply of competent entrepreneurs who run China's industry (including most of what foreigners nominally run)? Or should we entertain an altogether more daring possibility: that China's is a healthy and relatively well-educated labor force, tolerant of the inequalities that markets produce because it has known equality

—the accidental gift of 30 years of communism?

The truth is that the Chinese machine has so many potential drivers that it is anybody's guess why it runs. Moreover, no one really knows why all the forces that should have pushed China the other way—a corrupt and opaque system of governance, a decrepit banking system, dwindling natural resources—have not done more damage. But, then, explaining what happens in a country by examining it in isolation is always an unfair challenge. It is easier and also more useful to look for patterns that hold across a large number of countries.

Over the last two decades, a number of economists have spent many tedious hours building what are called cross-country databases. These are collections of historical data from a hundred or more countries on growth rates, savings rates, tax rates, and hundreds of other variables.

The hope was that once we had it all together, the laws of capitalism would reveal themselves to us. And indeed we do see some clear patterns. For example, there is an extremely strong and positive relation between the security of private property in a country and its per capita income. The problem is that like many correlations, it is not clear what this one ultimately tells us. Is private property secure because rich countries can afford to build a court system that protects it, or have rich countries become rich by offering security of private property?

Trying to separate cause and effect is never easy in the social sciences. Some economists have argued that among formerly colonized countries, those colonized by the British ended up with British-style laws that favor private property more than, say, the French-style laws we see in former French colonies. This would justify comparing British ex-colonies with other ex-colonies as a way of comparing alternative property-rights regimes.

But it has also been argued—for example, by James Robinson of Harvard University—that this would be a mistake, since the places the British colonized were unlike the places the French colonized. The differences we observe today may therefore reflect the differences that attracted the British and the French to colonize these areas in the first place.

So we keep digging, looking for the original cause—whatever it was that set the machine rolling. Daron Acemoglu and Simon Johnson of MIT, along with James Robinson, recently led a quest to figure out why some countries have more secure property rights than others. This led them to the early years of colonialism, where they discovered a rather remarkable fact: the countries that have better property rights today were, by all the measures they could come up with, the poorer of the colonized countries. What gave these countries an edge, Acemoglu, Johnson, and Robinson concluded, was the experience of the European settlers when they first got there. In the richer and more populous countries, the settlers died in droves, perhaps because they had more contact with the local population and, thus, the local diseases. In the poorer and more sparsely populated countries, mortality rates were lower. These early experiences determined whether the settlers would come to embrace and inhabit these countries or simply take and run. Where they settled in large numbers, they brought the systems for governing property that were emerging in their home countries, where capitalism as we know it today was in the process of being born. In other places, their primary concern was making sure that the local population did not make too much trouble, which often meant privileging the gun over the protection of individual rights. The

result is that the countries where settler mortality was low are now countries that have better property rights and higher growth.

Whatever one makes of this specific narrative, it is hard not to be a little discouraged by the idea that, where property rights are concerned, things only go well for the countries that started off on the right foot somewhere in the distant past. After all, the United States also had 400 years of history where these rights were debated, fought over, and finally embraced. Children in the United States grow up learning that history, and learning why it was worth fighting for. It is hard to imagine that this is not valuable in itself. It is therefore hard to be sanguine about growth automatically picking up if we were to suddenly institute U.S.-style property rights in Sierra Leone.

In any case, Acemoglu, Johnson, and Robinson's theory of where good property rights come from is hardly the kind of law that Marx or Malthus was looking for, but it very much reflects the kind of ambiguity that pervades growth economics today. Instead of a handful of simple and clear-cut laws that tell us what to do and what to expect, we have a hundred competing tendencies and possibilities, of uncertain strength and, quite often, direction, with little guidance as to how to add them up. We can explain every fact many times over, with the result that there is very little left that we can both believe strongly and act upon. Indeed, the only theories that we hold onto with some confidence are disaster warnings—banning all trade is bad, as is banning all private enterprise and printing money to pay everyone. With anything more nuanced, or less negative, there are too many doubts and differences.

It is perhaps natural that the reaction to this kind of uncertainty is to be pessimistic about the possibility of taking any constructive action. William Easterly, the most articulate of the pessimists, in his 2006 book *The White Man's Burden*, comes very close to suggesting that there are no recipes for growth that can be brought in from the outside, other than the recipe of giving people within the country incentives to find a recipe on their own.

But this is not what the evidence is telling us. All it is saying is that the cross-country data we are using is not up to answering the kinds of questions that are being asked of it. It does not mean that these are the only useful questions to ask, or that there is no other kind of data that can help us.

Consider, as an illustration, one of the perennial favorite projects in the policymaking world: investing in education. There are three things that cross-country data tells about this. First, richer countries invest a higher fraction of their incomes in education. Second, more education in 1960 predicts faster subsequent income growth. Third, and much more surprising, between 1960 and 1985, there seems to be no relation between investment in education (measured by the increase in the number of years the average person spends in school) and growth in incomes. Some of the countries that invested the most in education grew very fast (Taiwan, Singapore, Korea), but others (Angola, Mozambique, Zambia) did disastrously.

In his earlier book *The Elusive Quest for Growth* (2001), Easterly takes it on himself to sort out this evidence. He feels (rightly) that the fact that rich countries invest more in education is uninteresting, because we do not know whether they are rich because they invest more or the other way around. The fact that the countries that were better educated in 1960 grew faster in the subsequent period troubles him more. He argues that, given that almost every country is better educated now than it was in the 1960s, growth should have

accelerated everywhere, but it has actually significantly decelerated between 1960 and 1990. This is a clever argument but one that only works if what matters for future growth is the absolute level of education, whereas all the data tells us is that better educated countries do better, which could reflect the importance of having more education than your competitors.

For good measure, Easterly also invokes the reverse-causation argument—that people were getting educated in anticipation of future growth. But this is an argument against taking this piece of evidence at face value, not evidence that investing in education does not pay.

Having disposed of the two more optimistic pieces of evidence, Easterly focuses on the absence of correlation between increases in education and the growth rate. This, he suggests, is symptomatic of the waste that is created when people get educated because the government wants them to, or because donors are paying for it, and not because the market gives them reasons to. “Education,” he concludes, “is another magic formula that has failed to live up to expectations.”

But this is the wrong answer to the wrong question. It is the machine question: can we find some universal law in the cross-country data that says, invest in education? Good to start a discussion, like the question about whether aid is good for growth, but in the end unanswerable, at least to the point where it can be used for policy. How does one respond, for example, to the challenge that one reason so many African countries invested so much in education was to forestall civil conflicts? As newly independent states, it probably made sense for them to invest in education as a way of building a national consciousness, as well as to offer their people something they had been denied under colonial rule. It seems possible that things would have been even worse without the investment (it is also possible that the education contributed to the civil war, as unemployed educated youths looked for a way to vent their frustrations).

It is the wrong answer in part because this is, to use the colorful American expression, Monday-morning quarterbacking. Some of the countries with the least correlation between investment in education and growth are African countries that invested a lot in education and then fell into civil war. Angola, one of the biggest investors in education, fought Portuguese colonialists from 1961 to 1975, and once they left, fought internally until 2002. Mozambique, another champion of education, fought the Portuguese from 1965 to 1975 and wound up in a civil war between 1981 and 1992. Senegal, also on the list of education winners, has had civil conflicts since 1960, and Sudan, also on the list, has had civil conflicts between 1963 and 1972 and then again since 1983. Did any of these countries expect the conflict to go on for as long as it did? Would they have invested as much if they had known? It is easy, in retrospect, to criticize the investments, but what happened in these countries was probably worse than anyone had reason to expect, especially given that this was the world’s first experience of decolonization (at least in the modern sense). Given what we knew when these countries first started investing in education, investing a lot might well have been the right option.

It is also the wrong answer because it forgets that “education” is just shorthand for an enormously complex set of different strategies, and not a single button on the machine to be pushed or not. As a result, it ignores everything that was historically specific about what happened in the poor countries that scaled up education very fast, starting in the 1960s. To begin

with, in many, the colonialists or domestic equivalents (as in Nepal) had done very little to modernize the education system, with the obvious consequence that they were short on potential teachers when they started to universalize education. In these countries, almost none of the teachers were college graduates, and very few had even been to an upper secondary school.

Compare this with the experience of the countries that had high levels of education in 1960. These were countries whose education systems had evolved, often over hundreds of years, so that the supply of qualified teachers moved in step with demand. Why should we be surprised if their investments paid off more than those of their latter-day emulators?

Moreover, it is not at all clear that anyone in 1970 could have anticipated many of the problems that made scaling up high-quality education particularly hard. I have already mentioned the civil wars. But the economics also changed.

In 1973, George Psacharopoulos of the World Bank published a book called *Returns to Education: An International Comparison*. One of his key points was that investing in primary education pays off much more than investing in any other kind of education. This was good news for the countries that had just started universalizing primary education, especially since, given their resources, it was the one place they could make a difference with relative ease. Unfortunately, it turned out to be false. More recent estimates suggest that the benefits from an additional year of education do not fall as we go from primary education to tertiary education, and, in fact, they go up. This may reflect changes in the way education outcomes are being measured (we suspect that the more recent measures are more accurate), but the world has also changed since the 1970s. Everywhere we have seen the earnings of college-educated people rise relative to those who have less education, which is part of why inequality has been rising. We do not fully understand why this has happened, but it is plausible that the move toward high-tech has something to do with it.

From the point of view of the late investors, this is bad news for two reasons. It means that teachers, who tend to be college-educated, are getting more expensive. And it means that to realize the full returns from the investment in education they need to push forward toward secondary education and beyond, which is obviously harder. It is true that these are also problems for richer countries, but they can handle them better, for two reasons. First, they tend to have much better funded governments and therefore can afford to spend more on teacher salaries while the poorer countries have to let teacher quality slip. Second, the fact that rich countries already have a lot of education means that parents in rich countries can do a lot to shepherd their children through primary school, and often through secondary school as well. Given that most parents in poor countries of that generation never had the advantage of an education, good teachers are particularly valuable.

To sum up, with the benefit of hindsight, it does appear that these countries over-invested in education, at least in part because they (and everybody else) underestimated the challenge. Any country investing in education today would know not to try anything so ambitious, and things might go better.

That said, it also seems clear that education systems in many poor countries are facing disaster. When a group of scholars from Harvard University and the World Bank sent observers unannounced to 3,700 public and private schools

in India on three separate occasions, they found that 25 percent of teachers were absent on any given day. Moreover, only 45 percent of the teachers present were actually teaching when the observers arrived. The rest were drinking tea, or talking to other teachers, or reading the newspaper. And lest this seem like some South Asian aberration, the absence rate they found in Uganda was even higher (27 percent). The study did not try to assess the quality of teaching, but it is hard to hope for much from teachers who do not want to come to school and who ignore the students when they do.

It is therefore hardly a surprise that students in these school systems are not learning much. A 2005 nationally representative survey in India found that only 43 percent of fifth-graders could do simple (one-digit) subtractions and divisions, and only 60 percent could read at a second-grade level. Yet 93 percent of six-to-14-year-olds say that they go to school (though daily attendance is only about 70 percent, reflecting, perhaps, their level of enthusiasm).

What should we do? One answer often heard among economists is that we should stop trying to educate those who do not want to be educated. This comes with the corollary that when the market creates enough demand for educated workers, education will automatically happen. This goes against the idea, popularized by the Nobel Prize-winning University of Chicago economist Robert Lucas, that we cannot leave education entirely to private incentives because people benefit from the education of people around them, though in fairness it must be said that the empirical support for this view, as of now, is not overwhelming. It also flies in the face of the long history in the West of compulsory-schooling laws, and what we know about their impact.

The most compelling study on this subject of which I am aware is by Josh Angrist, of MIT, and Alan Krueger, of Princeton, published in 1991. They looked at what happened to people in the United States who dropped out of school at age 16, which is when it ceases to be mandatory. Among the study's subjects, there were some who ten years before had been just old enough to make it into first grade, and some who missed the cutoff age by a few days. Therefore, looking carefully at the group, you would find some who ended up with almost one whole year more of schooling, just because of the accident of having been born a few days earlier. The result was much like what would have happened if a lottery had determined whether each child would be put into school for nine or ten years—which is why economists call it a natural experiment. The differences in what eventually happened to them could be confidently ascribed to the fact that some got more education than others.

Angrist and Krueger found that being forced to stay longer in school does in fact pay off. Those who had stayed in school longer were paid more—the market rewarded investment in education, even by these young people who were dying to get out of school, and who would drop out as soon as they were given the chance. In other words, the incentives were there, but that was not enough for these children. And when they were forced to get educated, it made them more productive—and happier as well, according to a more recent study by Phil Oreopoulos of the University of Toronto.

Chris Spohr, from the Asian Development Bank, looked at what happened when Taiwan, in 1968, made it compulsory for children to go to school for nine years. He shows in a 2003 paper that for those who were young enough to be covered by these new laws, but not for the rest, this meant that they went to school for about a quarter of a year more on average. A quarter of a year

might seem tiny until you realize that most children in Taiwan would have stayed in school for nine years even without the law. So the quarter of a year increment per child came from averaging a lot of zeros with quite large increases for the relatively small group that was planning to drop out.

Like Angrist and Krueger, Spohr finds evidence that this extra, enforced schooling did pay off. Girls who were compelled to spend longer in school earned substantially more.

In the heart of Mumbai, India's commercial capital and quintessential boomtown, are the slums of L Ward. Almost every child goes to school in L Ward, and for most, school means the free public schools run by the Mumbai Municipal Corporation. When Pratham, an ngo with a long history of working on education, started testing the children in these municipal schools, even they were slightly taken aback. Only about a quarter of third-graders could do what was, at best, first-grade mathematics—recognizing numbers, counting, single-digit addition. Yet all around them was India's fabulously booming service economy, where firms were fighting to get educated workers. Could the government do much more to improve incentives for getting an education?

The real problem, some would say, is the quality of schools. The government should get out of the business of delivering education and distribute vouchers instead.

There is certainly something to this view. Teacher attendance is better in private schools, at least in India, according to the survey mentioned before. The average absence rate is 20 percent in private-aided schools (private schools that get some funding from the government) and 23 percent in fully private schools (which are more likely to be new schools), compared to 25 percent in government schools. The likelihood of a teacher actually teaching when observed in the school is 59 percent in private-aided schools and 48 percent in fully private ones, compared to 45 percent in government schools. Better, but not hugely better.

There is also some difference in test performance. We already considered a survey in India that measured whether fifth-graders could read and do elementary math. In that survey it came out that while just over 40 percent of public-school children could do the math and 60 percent could do the reading, the corresponding numbers for private schools were 52 percent and 70 percent. There is clearly a gap, but given that this was second-grade-level math and reading, the private schools are not doing spectacularly either, especially given that we would expect these children to be from the more motivated families.

The truth is that we do not really understand what is going wrong. Perhaps it is the quality of the teachers. After all, the reason that private schools have managed to grow so fast in India over the last few years is that they are cheap enough to be within reach of the average family, and the most important way they keep costs down is by paying their teachers very little—sometimes less than a quarter of what teachers are paid in public schools. At that price, since they seem to teach at least as well as the public-school teachers, they are a bargain, but they do not promise inspired teaching, nor should we expect it.

This is why another influential body of opinion wants us to revive the government-run school system. Public schools have the most qualified

teachers and a tradition of professionalism. They also have the weight of world history behind them; public schooling has been a part of the development process of all the developed countries of today.

What public schools need, in this view, is a dose of community control. This is a strategy that appears prominently in the World Bank's report *Making Services Work for Poor People*. People in the community, more than anyone else, see what is wrong with local schools and have the interests of their children at heart. Therefore, if they only had the power to reward teachers' performance and punish their negligence, things would work better.

Like incentives and vouchers, this is a sensible idea. The question is, how far can we expect it to take us? There are community-run schools in India, and the same survey that reported on teacher absenteeism in government and private schools tells us that teachers in community schools actually come less often than in either.

This becomes less of a puzzle when one talks to the community. In a study financed by the World Bank and initiated in 2005, a group of us (Rukmini Banerji from Pratham; Stuti Khemani from the World Bank; and Esther Duflo, Rachel Glennerster, and I from mit) carried out a survey of households to gauge the role of the community in overseeing education in rural areas of the North Indian state of Uttar Pradesh. One of the questions we asked the surveyed households was whether there was any committee in the village that was meant to deal with education issues. By law every village in Uttar Pradesh has to have a village education committee, and these were no exception. Yet a startling 92 percent of parents of children in the government school responded that they did not know of any such committee. Of those who claimed to know that such a committee existed, only two percent could name any of its members.

This was less of a surprise to us after we went to talk to the village education committees. In Uttar Pradesh, these committees consist of an average of five members: the school headmaster, the *pradhan* (head of the village government), and three parents. We found that among the parent members, about one in four does not know that he is on the committee. And of those who do know that they are a part of the committee, roughly two thirds are unaware of the Sarva Shiksha Aviyan, the big new program that is supposed to bring new resources to village schools.

In part, this reflects the fact that people in rural Uttar Pradesh seem not to be particularly engaged with any of the institutions of local governance. Only 14.2 percent of respondents knew of a household member ever having been to a Gram Sabha (village meeting), which are required to be held in every village from time to time. Over 90 percent said that they did not know when or where the Gram Sabha was held.

But even among the households who do go to the meeting, education does not seem to be a priority. Of those who have attended a Gram Sabha, only 5.8 percent mention education when asked about which issues were covered in the previous meeting. Parents are no more interested in education than anyone else. When they were asked what they considered to be the most pressing issues in the village, education ranked fifth, with only 13.9 percent of respondents even mentioning it. This is despite the fact that the villagers say that parents need to take responsibility for making the school run better.

It is not clear what is going on here. One possibility, of course, is that these parents do not value education. But it is also possible that they feel that the task of monitoring the teacher is beyond them. After all, the teacher is typically much better connected than they are, both socially and politically, and hence more powerful, and in any case they find it hard to judge how well he is teaching (though they can surely tell when he is not there). They also do not really know how bad things are: based on our interviews of parents and our actual tests of the children, most parents, but especially parents of children who are doing badly, have an inflated view of their children's abilities.

None of this means that we can do without more parental and community involvement. But if new investment in education is to be the transformative force that it is intended to be, a lot of other things will have to change.

It is the same with all of these: incentives, vouchers, community control. We come to them not as useful insights, which they surely are, but as a one-stop solution to the problems of education. To those who believe in it, the word "incentives" is an abstraction, a metonymy for faith in the power of the market. They do not claim to know how exactly the market will achieve the promised miracle, but it will do it (indeed, for them this unpredictability is part of its appeal). It will do it despite the fact that for the children in L Ward, jobs in the white-collar service sector are but a distant promise, largely disconnected from the lives of most people around them. Despite the fact that most of these children cannot get any help with their homework from their parents. Despite the fact that they have to compete in public exams with children who have three tutors with master's degrees helping them along. The market will figure it all out.

It is the same with the community. Benjamin A. Olken, a junior fellow of the Harvard Society of Fellows, conducted a randomized experiment in Indonesia with the help of the World Bank and the Indonesian government, in which the community was encouraged to report on corruption in road construction. The reports had no effect on the total amount of corruption in construction, though we have some evidence that they encouraged corrupt village officials to hide things better. In contrast, when the government sent outside auditors to evaluate road construction in a randomly chosen set of villages, there was a significant reduction in corruption. When I reported this result to a prominent champion of the community, his reaction was that the intervention was wrong. When I asked what he would have done differently, he shrugged: he was not sure. But it will work if they do it right, he assured me.

And in fact, it turns out that he may have been right. When Olken went back to the data and looked again, he found that things worked better when the comment forms were sent home with schoolchildren, than when the village elites could control who got them. In retrospect, this is no great surprise; what is curious is that this suggestion did not come from the champion.

The problem, in the end, is that we economists and development experts are still thinking in machine mode—we are looking for the right button to push. Education is one such button. Within education, there are more buttons: Economists talk of decentralization, incentives, vouchers, competition. Education experts talk about pedagogy. Government officials seem to swear by teacher training. If only we could do it right, whatever the favored "it" might be, we would be home free.

The reason we like these buttons so much, it seems to me, is that they save us the trouble of stepping into the machine. By assuming that the machine either runs on its own or does not run at all, we avoid having to go looking for where the wheels are getting caught and figuring out what small adjustments it would take to get the machine to run properly. To say that we need to move to a voucher system does not oblige us to figure out how to make it work—how to make sure that parents do not trade in the vouchers for cash (because they do not attach enough value to their children's education) and that schools do not take parents for a ride (because parents may not know what a good education looks like). And how to get the private schools to be more effective—after all, at least in India, even children who go to private schools are nowhere near grade level. And many other messy details that every real program has to contend with.

The great virtue of the recent emphasis on randomized evaluations of social programs, it seems to me, is that they force us to venture inside the machine. To implement a proper evaluation, one has to know the exact details that define a program. And as economists think about them, they begin to build stories about them and get ideas about how to change them for the better.

A wonderful example of delving into the bowels of the machine can be found in a recent paper by Esther Duflo and Stephen Ryan of mit and Rema Hanna of nyu. Seva Mandir, an ngo in Western India, had long been concerned about the fact that in many of the primary schools they run there were reports that teachers do not come to school. The problem was that these were one-teacher schools, so if the teacher was not there, no one other than the children and their parents would know. And they tended to be in relatively remote areas, so arranging for someone to routinely check on them was out of the question. What could they do?

When Seva Mandir explained this challenge to Duflo, who had worked with them before, she had a brain wave. Cameras were getting cheaper all the time. Why not tell the teacher to get a child to take a picture of him and the class at the beginning of each day and at the end, with a time-and-date stamp on each picture. That way you will know at least that he was there at two points in a given day. Seva Mandir agreed to give it a try; and to make the teachers take it seriously, they announced that salaries would be tied to the pictures: teachers would be paid 50 rupees for every day for which they had two pictures. The 50-rupee number was chosen to give a teacher who showed up for 20 days a month what he used to get under the old system (1,000 rupees). There was some concern that teachers would resist the new system, but on the whole it was surprisingly well received: the teachers liked it because it put their destiny in their own hands.

Duflo, Hanna, and Ryan carried out a randomized evaluation of this program. The results showed that teacher absences (measured by unannounced visits by monitors to both experimental and control schools) were 42 percent in the control schools and 22 percent in the schools where the cameras were being used—and at the end of the year, children in the camera schools performed much better on their exams. Moreover, given how responsive teachers seemed to be to the incentives, Duflo, Hanna, and Ryan concluded that it would be worth raising the daily payment by 5 rupees, to 55 rupees per day.

Seva Mandir considered the experiment a success, and the program continues. But now that they have seen the benefits of giving the teachers incentives, they have begun to wonder whether there are cheaper options, and

ones that are more unobtrusive. The plan is to think of new ways to appeal to the teachers' motivations. The last time I was at Seva Mandir, I watched Duflo, her colleague Sendhil Mullainathan from Harvard, and Neelima Khetan from Seva Mandir debating how teachers would react to being confronted by empty pages in a child's notebook, left empty to show that the teacher was not there. I thought I saw a new economics being born. <

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