The tapestry behind Esther Duflo, “Peoples of the World,” was handcrafted by Japanese artist Fumiko Nakayama. It was donated by MIT alumnus Mohammed Abdul Latif Jameel, a major J-PAL funder.

Esther Duflo

The problems of poverty in the developing world are extreme, extensive and seemingly immune to solution. Charitable handouts, massive foreign aid, large construction projects and countless other well-intentioned efforts have failed to alleviate poverty for many in Asia, Africa and Latin America. Market-oriented fixes—improved regulatory efficiency and lower trade barriers—also have had limited effect.

What *does* work? MIT economist Esther Duflo has spent the past 20 years intensely pursuing answers to that question. With randomized control experiments—a technique commonly used to test pharmaceuticals—Duflo and her colleagues investigate potential solutions to a wide variety of health, education and agricultural problems, from sexually transmitted diseases to teacher absenteeism to insufficient fertilizer use.

Her work often reveals weaknesses in popular fixes and conventional wisdom. Microlending, for example, hasn't proven the miracle its advocates espouse, but it can be useful in the right setting. Women's empowerment, though essential, isn't a magic bullet.

At the same time, she's discovered truths that hold great promise. A slight financial nudge dramatically increased fertilizer usage in a western Kenya trial. Monitoring teacher attendance, combined with additional pay for showing up, decreased teacher absenteeism by half in northwest India. Better access to credit for financing water connections in urban Morocco significantly improved family well-being, even without income or health benefits.

Duflo would resist the oxymoronic label, but she is something of a rock star economist: profiled by the *New Yorker* last year, honored as a MacArthur "genius" in 2009, recipient of the 2010 John Bates Clark award as the best economist under 40, and winner—with coauthor Abhijit Banerjee—of the *Financial Times*/ Goldman Sachs "Business Book of the Year" award in November 2011 for *Poor Economics: A Radical Rethinking of the Way to Fight Global Poverty.* She wears celebrity awkwardly. The work is important, she would argue, not the individual. But she knows well that her fame, such as it is, helps promotes her cause, and she's passionately devoted to improving the welfare of the poor.

With *The Region*, she discusses strengths and limits of experimental methods, why reserving leadership posts for women makes sense and a future agenda for development economics. Above all, she emphasizes that poverty and its solutions are multidimensional. "If we think of them each as an isolated data point, their meaning is limited," she said of her research results. "But together, they start painting a picture."

Photographs by Peter Tenzer

A DEEPER CONCEPT OF POVERTY

Region: In *Poor Economics* with Abhijit Banerjee, in your 2007 *Journal of Economic Perspectives* piece together and elsewhere, you describe a richer concept of poverty—more nuanced than the traditional concepts of starving masses or Schultz's phrase: "poor but efficient."[1]

Would you start by explaining that deeper concept and then discuss what it means in terms of how to approach solutions to the problems of poverty?

Duflo: The short answer is that there is not one thing. You can't replace any of the clichés by yet another one. It's a very natural thing to do, to try to reduce a problem to a single dimension, and I think that's what people have done. And in a sense, in each of these clichés that has coexisted or existed in succession, there is a certain amount of truth. It is just that you need some combination of them.

For example, I think "poor but efficient" is actually a very useful step in starting to think about the world of the poor—much better than the ideas that existed before. It was a very foundational step to think, "Well, we are just going to consider the poor in the way that we considered anybody else at the time in economic models," which is people who are making rational decisions.

So there is a lot of use to that, but now we can also include what we've incorporated in the last few years in economics as well, which is that people are not always acting fully rationally. Or they are acting rationally, but they don't have full information. Or they have constraints on what they can do because other people lack information about them.

And so, you add this complexity, and then an added layer is the psychological limitations. You recognize that the poor, like any of us, are social beings who exist within a social context. They have friends and values, and things like that, and all of these things have their weight into the way people make decisions. And so sadly, there's no replacing what is there by something else, but it's keeping in mind the whole picture in thinking about how people make decisions.

"Poor but efficient" is a very useful step in starting to think about the world of the poor. … But now we can also include what we've incorporated in the last few years in economics as well, that people are not always acting fully rationally. Or they are, but they don't have full information. Or they have constraints on what they can do. … And so then you recognize that the poor, like any of us, exist within a social context.

Region: That's a lot of heterogeneity, no?

Duflo: Yes, it is a certain amount of heterogeneity, but that doesn't mean it's unpredictable, because there is some logic to this heterogeneity. You can predict on the basis of other things you have seen before. For example, you can predict what's likely to be a constraint, or under what conditions something is likely to be a constraint.

And therefore, you can foresee where policy action might be effective. You might be wrong, of course. You don't want to replace what was there before, either, by saying, "Oh, we do not know." I think we are very much in favor of an analytical approach where you can predict and understand how people behave and why they behave the way they do. It's just that it requires that we keep a lot of threads together.

THE EXPERIMENTAL APPROACH: ADVANTAGES AND LIMITATIONS

Region: You've been a pioneer of experimental research in developing countries. Your work has inspired many others and helped revitalize development economics generally. But as you're well aware, the experimental approach has also been criticized by some economists, for a variety of reasons, including lack of generalizability, ethical considerations, compliance issues and other issues—points James Heckman made 20 years ago.[2]

What are the central advantages of the experimental approach over other methods—observational studies, for instance, or structural estimation techniques? Or is it complementary to those? And the follow-up question is, Which of the criticisms do you consider well founded, and how do you address them?

Duflo: There are a number of distinct advantages over other methods. One, of course, is the obvious one, which is that running an experiment gives a handle on causality, at least in the particular context in which you run your experiment. When you run an experiment, you modify the conditions in one group and not in another group. Assuming the experiment was well run, you know that whatever different outcomes or behavior you measure are due to the modification of conditions.

One criticism that I don't find useful is, "Oh, but if the experiment is not well run, then that's not true." Well, that's obvious. I don't know how that helps us. That's always the case—true in the lab, true everywhere.

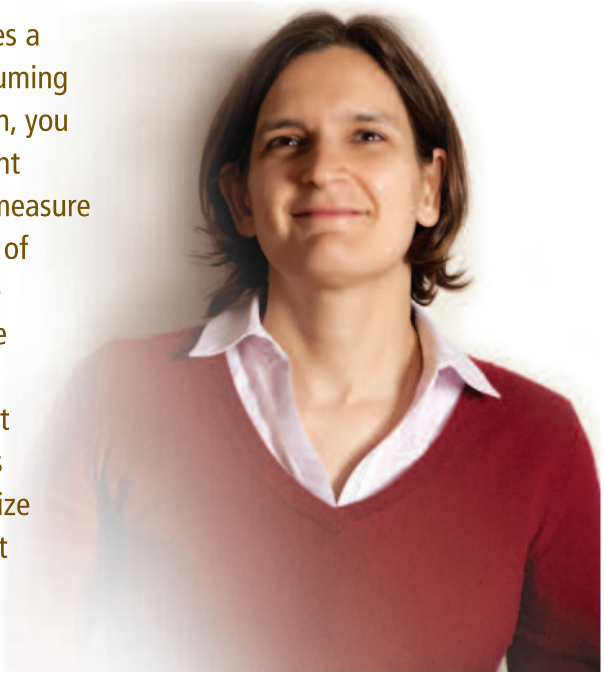
So getting a handle on causality is the first advantage, and it's the one that is easiest to explain to policymakers. If you want to know whether your policy works, that is a very transparent way to do it. And you know it with much more certainty than you would have without an experiment, because usually in the real world when things differ, there are reasons for it, and that possibility prevents you from estimating the causal effects of the policy or intervention.

But there are other advantages, which are more subtle. One is that you can sometimes with experiments estimate things that you just could not estimate in any other way. It's not that you can do it *better* with an experiment; it's that there is *no other way* to get at it.

Region: For example?

Duflo: Suppose you are interested in a range of price elasticity. You might be able to experiment with prices that are just not observed in nature. For example, when people sell things in the market, they don't sell things at the price of zero. [Laughter] You might need an experiment in order to test zero. In fact, that's a point Heckman made a long time ago, saying that that's what experiments should be used for.

Running an experiment gives a handle on causality. … Assuming the experiment was well run, you know that whatever different outcomes or behavior you measure are due to the modification of conditions. … But there are other advantages, which are more subtle. … The only criticism of experiments that I think is really useful is this question of, Does it generalize or not? With the caveat that that question applies pretty much to any method.



Say you are interested in estimating people's response to increasing or decreasing their wage. That response is a combination of an income effect and a substitution effect. In the real world, you can't really distinguish the two, because whenever wages increase, the two things happen.

But in an experimental context, you *can* separate the two. You can give people a bunch of income that doesn't correspond to a wage, which you wouldn't be able to do in a real world setting, so you can estimate the income effects separately from the substitution effect. This was what the negative income tax experiment set out to do, and Heckman was actually quite in favor of this particular use of experiments (in the 1991 article you cite).

A recent example of this is an experiment by Rob Jensen and Nolan Miller, where they look at the effect on consumption of changes in the price of rice.[3] If you decrease the price of rice, will people consume more rice or less rice? In the real world, it's very difficult to know that because whenever the price of rice decreases, that's the result of a combination of supply and demand factors, and isolating variation in the price of rice as purely exogenous is essentially impossible.

So you need an experiment to know, and in fact they found something very interesting when they did this experiment in one place in China where rice is a very important part of the food basket for the poor. And they found that when the price decreased, people ate less rice, not more rice, which means rice is a Giffen good [a product that consumers demand more of as its price rises because the income effect dominates the substitution effect].

Region: I didn't know they existed.

Duflo: Well, exactly. Whether an actual Giffen good exists has been a question since … since [pause]

Region: Giffen himself, I suppose.

Duflo: [Laughter] [Alfred] Marshall brought it up, but he attributed the observation to one Dr. Giffen. And I think this experiment is very fascinating, because you can't investigate it any other way. I think you can't dispute the fact that rice, in this particular place in China, is a Giffen good.

But then it comes to one of the criticisms: "It doesn't generalize." Yes, it doesn't mean that rice is a Giffen good here in the United States. I'm not interested in that question. But the fact that there is one Giffen good somewhere I think makes this interesting. It is incremental knowledge for how we think about the world and is very, very, very important for what we think about the poor and food. And in particular, in the policy domain, it shows that policies that subsidize the price of staples—which is quite common—might be counterproductive from the view of getting people to eat more. It still might be good for the poor, because they consume a lot of staples, and subsidizing a staple improves their income. But if the objective was to make people eat more, that's not necessarily the way.

That does not mean that it would be true in India, but the very fact that there is this possibility means that we want to investigate this question more. And we can try a similar experiment elsewhere to see in what conditions this will reproduce. With a Giffen good, the advantage is that we have a very established theory that helps us think what's likely to be a Giffen good. It has to be something that is a very big part of the budget so that the income effect is large. And it must be an inferior good.

That gives us a sense of, in another place, how would we go about looking for a good that's likely to have the same characteristics? Maybe there are no Giffen goods here because no goods have those characteristics. But maybe if we went to Ethiopia, it would be whatever is the staple food there. We can see what's the share of this staple in people's budgets and get some idea of what we are looking for.

The only criticism of experiments that I think is really useful is this question of, Does it generalize or not? With the caveat that that question applies pretty much to any method. The only way in which experiments are different is, because there are cases—this is a point Heckman made a long time ago—where the experiment modifies the population that you study because not everybody would even agree to be in an experiment.

For example, not every city accepts to be the site of a job market experiment or not every nongovernmental organization accepts to work with you, so any result you find is specific to the context of people who agree to work with you, and the people who agree to work with you might be different. So I think that's a very relevant point, which is specific to experiments.

But that is different from the point generally made, which is that if I have a result somewhere, it may not apply elsewhere. That applies to just about any result from any research approach. Science makes progress thanks to the interplay of theory and experiment that helps us draw generalized knowledge from individual observations.

The only reason we discuss it with experiments is that we have solved the other problems, so there is more time to discuss that. Until now, there has not really been a problem of worrying whether things were generalizing. Sometimes people use quite subtle sources of variation to identify things they are interested in, so they're looking at very few people. And these very few people are very particular, so we cannot generalize from estimates that are identified from variation that is affecting just 5 percent or 2 percent of the population.

But that being said, it's still the case that the question of generalization applies to any study, and thinking about this question is useful and important. I think the answer is that you'll never be able to interpret a single experimental result, except if it's something like the Jensen result, which is kind of a counterexample, it's showing something quite [pause]

Region: Counterintuitive.

Duflo: Yes, counterintuitive. But if you had done 10 experiments where you had found that these 10 goods are Giffen goods, it doesn't tell you that the 11th is, or isn't, a Giffen good. So in most cases, one single result is not sufficient to reach broad conclusions.

But on the other hand, typically these experiments are informed by a broad theory. Either it's implicit or explicit—but very often explicit—and an experiment is set up in part to test that theory. Or even if it's set up to evaluate a policy that someone is running and wants to evaluate, researchers usually get interested because they can put that in a framework. It helps them test some hypothesis.

That's the theoretical framework which helps make sense of the result of the experiments. In a sense, that's why we wrote this book, *Poor Economics*. That's a little bit of, if you take each of these experiments—and not only experiments; we also have lots of nonexperimental research, there are descriptive results, et cetera—fitting together. If we think of them each as an isolated data point, their meaning is limited. But together they start painting a picture.

Region: Like the Fumiko Nakayama tapestry on P-LAB's wall. [See photo description on page 12.]

Duflo: Yes. They all fit into a greater picture.

Sometimes, maybe, an isolated result is very puzzling. So we just have to set it aside and wait to see how other results will fit with it. Maybe it was a fluke. Maybe things actually continue going in that direction, and then it will push theory to develop an answer to this. The theory changes and then that generates a new wave of experiments. Research moves like that.

If you look at the developments of the last 15 years, I would say it's that process you see happening. As I wrote in a simple paper, "Poor but Rational?"[4] in some sense, we had done the first phase at that point; we had a lot of experiments and nonexperiments whose results were a bit odd, and we didn't have a framework to think about them. Since then, people have developed much more of a theoretical framework to think of behavioral economics of poverty. And the new wave of experiments helps fill in that framework.

WOMEN'S EMPOWERMENT AND MARKET FAILURE

Region: At the Fed's Jackson Hole symposium in late August, you suggested that reducing a variety of market failures could better ensure that the well-being of the poor improves as nations grow economically—that there would not be a growth/equity trade-off.[5]

You've also written that while development and women's empowerment are reciprocally intertwined, neither ensures the other.[6] In other words, that growth doesn't guarantee gender equity, and empowerment won't improve all aspects of life.

I read both pieces and wondered whether the market failure argument that you apply to income equality and growth is also relevant to gender equality. That is, can policies to reduce market failures better ensure women's empowerment as nations develop?

Duflo: To a point, yes, there are cases where we see that, I think. For example, very few women are elected as policymakers; it could just be that people don't like to be led by a woman. So then there's no market failure. As women, we may not like it, but that's the market equilibrium.

But it could be that it's because people think that women are not going to be good. Or even not that they think that they won't be good, but maybe they are just worried because they have never seen a woman lead, so their priors—that is, their prior beliefs—are very diffused. It seems to them that it is a very risky proposition to elect a woman, because they don't know whether women are good in general, or not so good in general, so there is much more variance in their estimate of how good a woman will be, compared to a man. So they go for a man always just because they have gone for men always, and it's the safe thing to do.

Region: The known quantity is the default.

Duflo: They know how men are, typically. They don't know about women in that position, and you don't want to take risk in politics. That could be very inefficient because it means they never elect women and never find out that women maybe have the same average quality. In that case, they are depriving themselves of half of the pool of capable leaders. And there it's kind of a market failure.

So if you force people to experiment with women and they discover that women are fine, then they start to elect them themselves.

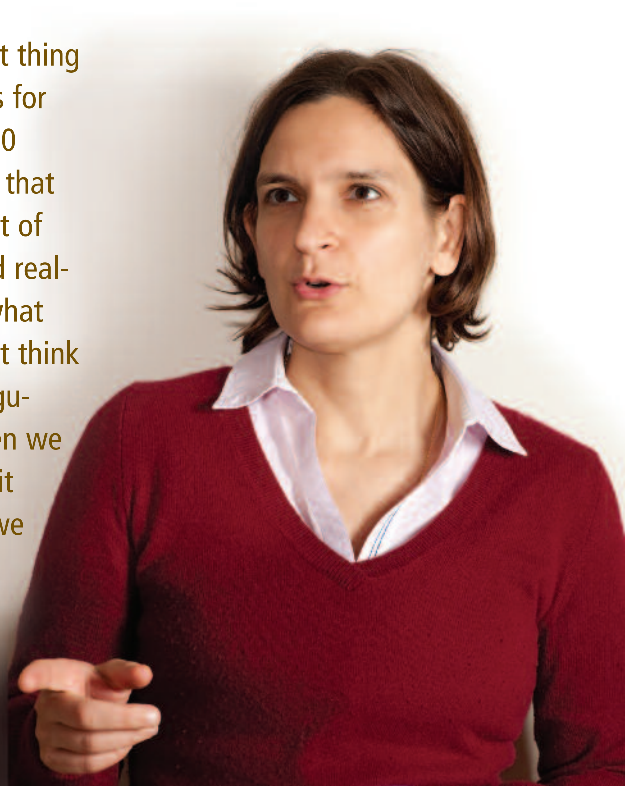
Region: And the forced experiment with women might be through a reservation policy, such as the one you've studied in India.[7]

Duflo: Yes, for example, through reservations. And after reservations go away, they may continue to elect women.

Region: If the results are good—if women are seen to be effective.

Duflo: Yes. If they are not, then they shouldn't be in office. So that's an example where it's a market failure and one that can be addressed by forcing people to experiment. I think that's a much better rationale for reservations than one that is typically made, in terms of outcomes. I think changing perception is a better argument. There is no downside for people to acquire information that they didn't have. Maybe taste discrimi-

It was actually an efficient thing to reserve policy positions for women for five years or 10 years [in India] … just so that people experience the fact of having a woman lead and realize that women are not what they thought. … But I just think that the business case argument should be used when we have evidence for it, and it shouldn't be used when we don't.



nation against women per se had gone away a long time ago, but people still won't elect them just because of this statistical discrimination. I don't think that's true—in our survey in India, people were not shy to admit that they don't like female leaders, but that's possible.

Region: You said, I think, in the women's empowerment paper that to bring about equality might require that policies favoring women should be in place for a long time to come. It's a provocative statement. How have colleagues and policymakers reacted to it?

Duflo: Colleagues are fine with it. Policymakers usually are a bit sad, particularly those who are advocates of women, because the way the case has traditionally been made for empowering women is a business case. To say that you should do it because it is good for everybody.

It's really a whole bunch of arguments, like women will be less likely to be corrupt, they will invest more in girls—you name it. They have better investment opportunities because no one has given them money before. There is a range of claims that people make to say that discriminating in favor of women is the policy efficient thing to do.

I think that's a slightly dangerous case to make, because if you find out eventually that that's not true, it's going to be apparent, and then once the business case disappears—that is, you have problems—people will say, "You fooled us on the business case," and you'll get this backlash.

So I think it's better to call a spade a spade and to say, "Well, if you look at the rich countries, there is still plenty of discrimination against women." So if you care about equality for its own sake, then you might have to continue to help out for a while. We don't know; eventually it might disappear, but it might take some time. We don't know how long because we see we still have discrimination here in the United States, in some domains.

> When I started working in this field, there was a range of questions that were all fitting together in a theoretical framework, but empirically, the questions were quite open. … The idea of carefully looking at data while being inspired by a model is actually quite a tradition in development economics.

That's *not* to say there is *never* a business case. For example, I just made one for the political reservations, that it was actually an efficient thing to do to reserve policy positions for women for five years or 10 years. Have them in place just so that people experience the fact of having a woman lead and realize that women are not what they thought.

But I just think that the business case argument should be used when we have evidence for it, and it shouldn't be used when we don't. Policymakers always want to go back to the business case because it goes well, you know? It's win-win, and win-win arguments are very popular.

Region: Especially in politics, so the politician needn't worry about alienating those who might lose.

Duflo: In politics, exactly, win-win arguments really have this attraction. People want something for nothing, and they can't always have it.

Region: Spoken as a true economist: no free lunches.

Duflo: Actually, as an applied economist, I'm one to think that often you *can* get something for *next* to nothing. There are many cases where there is a lot of inefficiency, and they could be improved. That would increase the size of the cake as well as redistributing it. So in general I'm more on the side of thinking that sometimes it's possible. I just think that we should make these claims only when we are able to make them.

THE COMMON THREAD

Region: You've done such a broad range of empirical work, from microfinance to fertilizer use, teacher absenteeism, school construction, water supplies. And you've conducted these studies from Indonesia to Cote d'Ivoire.

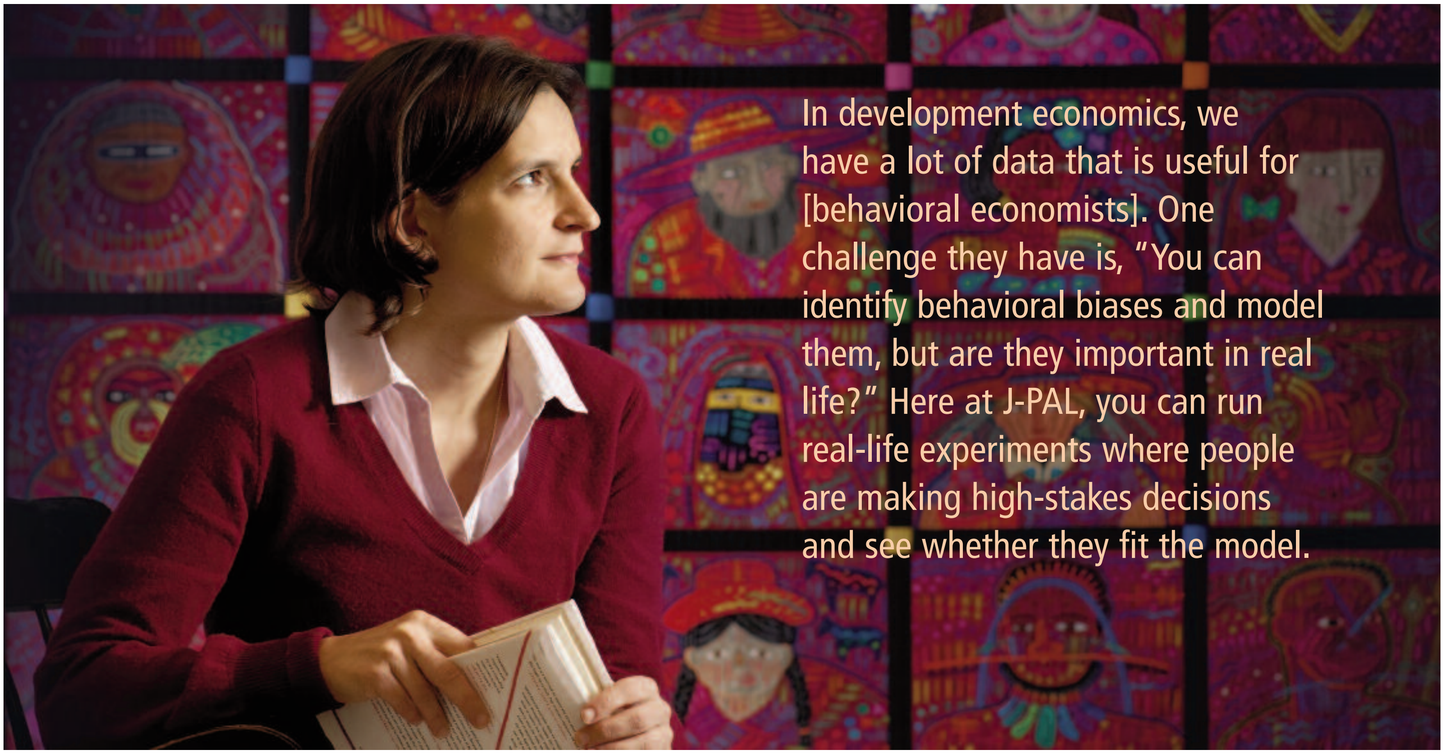
What is the common thread? What are the fundamentals that motivate and give coherence to this wide range of work?

Duflo: So [long pause]. Early on in my career, I guess, that's a question which I wasn't asking myself, and I wasn't, in the sense, particularly interested in that. There are so many questions that are important in development that we know little about. So if I can get an opportunity to answer these questions, I should go for it. I guess that's why there's such a range of things I've studied.

In that range, there was one common thread, which is methodological: If I've made causal statements, they are accurate; these are *true* natural experiments or *true* randomized experiments. So that's always been there.

But in terms of the topics, I guess I've looked at the common core of the type of questions that we as a development community think are important: education, infrastructure, et cetera. And on those questions, to just do what I could do.

I didn't feel that more focus than that was particularly needed because, you know, it was such an open field. It was an excellent field when I entered it. I started at MIT in 1995. It was an ideal time to start working on economic development because the early 1990s had seen a lot of really fundamental theoretical work, particularly by Abhijit Banerjee, Andy Newman, Debraj Ray, Tim Besley and other applied theorists and building on Joseph Stiglitz. So, it's a field that had first been reborn through



applied theory. When I started working in this field, there was a range of questions that were all fitting together in a theoretical framework, but empirically, the questions were quite open.

Not very many people were working in empirical research then. I mean to say, of course, that the field has a long tradition of both empirical and theoretical work, but it was just not a very thick field. There's a tradition of very, very good people. But with all of the theoretical work laid out in the early 1990s, it left many empirical questions wide open.

And then more or less at the same time, there was all of this work in labor economics in the U.S., and later in public finance, that was improving methodology in terms of demonstrating causality. So we had the whole field of development economics in which to make the two play together. The idea of carefully looking at data while being inspired by a model is actually quite a tradition in development economics. For example, Schultz's concept of "poor but rational" was very grounded in the theory of the time, but it also looked at the data that then existed.

That continued constantly over time, and we're doing the same thing—leaning on the theory and using the tools that had been developed more recently. It was a great time, given that these two things were available.

And now you can judge yourself whether or not *Poor Economics* has some common thread. There is no grand unifying theory of everything, which comes back to this idea that there is not one vision of the poor that can explain everything, but there are a number of insights that run through it all. And my work is maybe always, or often, trying to push one of these insights or show evidence—either very traditional things, like how people respond to financial incentives, or slightly more recent things like the fertilizer work, which links more to behavioral economics.

BEHAVIORAL BIASES

Region: Let's jump to that. You mentioned it earlier as well, the behavioral biases and blocks that limit what might be considered rational behavior. You've done research on this with Kremer and Robinson on fertilizer and with Banerjee and others on immunization.[8] Utility-maximizing agents are a bedrock assumption in traditional economic models, so how do you change theory to account for such biases, and does doing so limit the explanatory power and generalizability of theory?

Duflo: Well, the theoretical advances come from people who have done the behavioral work. I tend to take models and apply them to the circumstances that I have. But when you take the workhorse model and add hyperbolic discounting, people are still maximizing utility; they're just maximizing a utility function that's different from the one we used to work with, and probably more realistic.

It's like when information economics came in the 1980s, or the late '70s. Before that, the models assumed that people were perfectly informed when of course they are not. And people like Stiglitz showed that we can incorporate imperfect, asymmetric information and still work with that. And again, you don't have one universal model of everything.

Region: But eventually do they become analytically intractable?

Duflo: There are more things to deal with, but no. Maybe they are harder to put in a big machine to explain the entire economy, like a "Minnesota school" economist would like to do, but they can still be worked with. And if it hasn't already happened, it won't take very long for it to be incorporated in macro models as well.

Region: Who are the behavioral economists that you take your lead from?

Duflo: David Laibson, Matthew Rabin, Sendhil Mullainathan, Dick Thaler.

In development economics, we have a lot of data that is useful for them. One challenge they have is, "You can identify behavioral biases and model them, but are they important in real life?" Here at J-PAL, you can run real-life experiments where people are making high-stakes decisions and see whether they fit the model.

MACRO & MICRO

Region: Most of your work—perhaps all—is at the microeconomic level. How do you integrate your findings with macroeconomic considerations that affect development—aggregate growth, international trade, and fiscal and monetary policy?

Macro models should be micro-founded with the right micro assumptions. "Right" both in terms of incorporating the important dimensions that really need to be there, like credit constraints or some other reason why resources don't flow to their most efficient use, and right quantitatively in terms of micro parameters. … The agenda is very young, but it's being moved.

More About Esther Duflo

Current Positions

Abdul Latif Jameel Professor of Poverty Alleviation and Development Economics, Massachusetts Institute of Technology, since 2005; founder and co-director, Abdul Latif Jameel Poverty Action Lab (J-PAL), since 2005; on MIT faculty since 1999

Previous Positions

Director, Development Program, Center for Economic Policy Research

Research Associate, National Bureau of Economic Research

Board Member, Bureau for Research and Economic Analysis of Development

Fellow, MacArthur Foundation

Fellow, American Academy of Arts and Sciences

Member, Board of Editors, *Annual Review of Economics*

Founding Editor, *American Economic Journal: Applied Economics*

Honors and Awards

David N. Kershaw Award, Association for Public Policy Analysis and Management, 2011

Médaille de l'Innovation, Centre National de la Recherche Scientifique, 2011

Thomas C. Schelling Award, Harvard Kennedy School, 2011

John Bates Clark Medal, American Economic Association, 2010

Calvó-Armengol International Prize, Barcelona Graduate School of Economics, 2009

Best Young French Economist Prize, Le Monde/Cercle des Économistes, 2005

Elaine Bennett Prize for Research, American Economic Association, 2003

Publications

Author of numerous journal articles, particularly on microeconomic issues in developing countries, including household behavior, education, access to finance, health and policy evaluation through use of randomized control trials. Co-author (with Abhijit Banerjee) of *Poor Economics: A Radical Rethinking of the Way to Fight Global Poverty*, selected as the *Financial Times*/Goldman Sachs "Business Book of the Year" in 2011

Education

Massachusetts Institute of Technology, Ph.D. in economics, 1999

DELTA, Paris, master's in economics, 1995

L'Ecole Normale Supérieure, Paris, 1994

Developing theory is not really my role. I'm an empirical person. That's what I'm good at. I'm not going to start writing theory. It's certainly not J-PAL's role. We run experiments; that's what we do.

Duflo: Banerjee and I have a chapter in the *Handbook of Economic Growth* called "Growth Theory from the Lens of Development Economics" that tries to get at that.[9] And the point we are making is that these macro models should be micro-founded with the right micro assumptions. "Right" in terms of incorporating the important dimensions that really need to be there, like credit constraints or some other reason why resources don't flow to their most efficient use, and right quantitatively in terms of micro parameters.

We do it in a primary school sort of way at the end of the paper. I think since then there has been much, much more involved work to do this well. That's something that Rob Townsend here at MIT has been doing for a while. A lot of people trained more in the [Universities of] Minnesota- and Chicago-type of traditions are good at it and have been doing it since then. So, Townsend, Paco (Francisco) Buera, Pete Klenow. Other younger economists are also going in this direction.

I'm not saying we couldn't do it here, and to some extent it is being done here under Rob Townsend. But in any case, I think that is the way to integrate micro and macro development economics. That is, use the micro insights to estimate parameters and also all the important things that it tells you about the way life is, I guess. What needs to be taken into account, like the shape of the production function, whether people need collateral to borrow—incorporate those constraints in models and then do the same calibration exercises.

I think the agenda is very young, but it's being moved. That's one thing that I'm not going to do. It's not my comparative advantage. But I think someone should be doing it, and in fact, they *are*, in a very fruitful way.

RESEARCH AGENDA

Region: In September 2010, you released a paper, "A Research Agenda for Development Economics."[10] It suggests (a) revitalizing applied theory to address limits exposed in earlier theory by recent empirical work, (b) expanding empirical research and (c) expanding both theory and empirical research on aggregate consequences of micro distortions.

Duflo: Yes, the third is what I've just talked about. And the first point we've discussed a bit. So, point two, obviously I have to preach a bit for my own parish along the way.

Region: Yes, please, let's focus on the first two. How can theory be revitalized, and how will empirical work expand? And how do you and J-PAL intend to allocate your time, your resources?

Duflo: This is already happening in a sense. You know, we know more than we did five years ago; there are more applied theory papers that have come out. But it's true that, as I was saying, the really big growth spurt of developments in the late '80s, early '90s was applied theory, and it gave the framework that all of the empirical work built on.

But now sometimes some of the limits of those models have been shown, and it would be nice to have other things as well. Now people should go back to doing that, and I think they will, naturally.

And if they don't, it's not because of development economics per se; it's because economics as a field generally is not very sympathetic to applied theory at the moment. I think theoretical work needs to be considered "hard core" to be interesting. That's not specific to *development* economics; it's a general issue in economics as a field.

But you know, this type of thing comes and goes, so applied work will come back. Developing theory is not really my role. I'm an empirical person.

That's what I'm good at. I'm not going to start writing theory. It's certainly not J-PAL's role. We run experiments; that's what we do.

But to the extent that we train students, we make sure that training in development economics has a good balance of theory and empirical work. We are lucky to have Abhijit Banerjee and Rob Townsend at MIT, who can make sure that this happens!

Region: Thank you very much for your time today. R

—Douglas Clement
Oct. 25, 2011

Endnotes

1 See Schultz (1964).

2 See Heckman (1991).

3 See Jensen and Miller (2008).

4 See Duflo (2006).

5 See Duflo (2011b).

6 See Duflo (2005a).

7 See Duflo et al. (2009) and Duflo (2005b).

8 See Duflo, Kremer and Robinson (2009) and Banerjee et al. (2010).

9 See Banerjee and Duflo (2005).

10 See Duflo (2011a).

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