

Esther Duflo: 2010 John Bates Clark Medalist

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Esther Duflo, winner of the 2010 John Bates Clark Medal, has made extraordinary contributions to development economics. Esther has focused on topics that many economists think are central to understanding the differences in welfare between the high-income and low-income countries of the world: not only education, credit, food, and health, but also more nebulous concepts like social institutions and leadership. She has erected and inspired a research apparatus all over the developing world that seeks to address these questions by integrating large-scale field experiments with economic theory to yield important insights for development policy and our understanding of behavior and institutions in developing countries. She exemplifies and has played a vital role in the exciting renaissance of development economics over the past decade.

Esther was born in France, and received her B.A. in history and economics from the Ecole Normale Supérieure in 1994 and her Masters in Economics from DELTA in 1995. She did her Ph.D. in Economics at MIT, completing her doctorate in 1999. She roiled the economics establishment from the start of her career, with MIT breaking standard practice to offer an assistant professorship to one of its own students. In conversations with her at that time, I had to recognize that my efforts to recruit her to Yale instead were doomed to failure and agree with her reasoning that the environment that she had available to her at MIT was perhaps uniquely well-suited to her vision of the work she planned to do as an economist. She decided to stay at MIT as an Assistant Professor, and has remained there ever since. This is just one of the times that her choice to ignore conventional wisdom worked out pretty well.

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Esther has an inspiring description of the development of her research agenda in the Winter 2011 newsletter of the Committee on the Status of Women in the Economics Profession [31]. She concludes by listing the basic questions that keep her moving: “what makes poor people tick, what keeps them stuck, and how economic policy can help them?” Esther is passionate about her mission to find levers to make life better for the poor. To many who know her, this is the source of her extraordinary drive and intensity. There are few precedents for Esther in our profession; right from the start of her career as a new assistant professor, she has taken on a rare combination of professional roles as a cutting-edge researcher, a catalyst of research for a new generation of scholars, a policy activist, and a public intellectual. Instead of diffusing her impact, this coupling of her intellectual agenda with her passionate social activism has begun to reshape scholarship, policy, public debate, and the everyday lives of many of the world’s poor.

I’ll divide my discussion of Esther’s work into four admittedly arbitrary categories: educational production, the economic lives of the poor, women as decisionmakers, and a broad category of market and policy failures. I will then offer some thoughts on Esther’s role as a scholar-activist. All references to Esther’s work in this paper will refer to the numbers on her papers as listed in Table 1.

Educational Production

The causal effect of schooling on wages is a classic empirical question and a central topic in labor economics: literally hundreds of published papers attempt to measure the effect of an increase in educational attainment on wages. The main challenge is that educational attainment is not exogenously allocated across people: at the individual level, people and their families choose how long to invest in schooling, and at the community level, there is a political process that determines the allocation of resources to education. In [1], Esther offers an exceptionally clean examination of this question. Esther exploits the fact that in 1973 the Indonesian government adopted a new policy to construct a large number of new primary schools focusing on areas of the country where enrollment rates at the start of the program were particularly low. Esther uses a difference-in-differences estimator, comparing the cross-cohort changes in school attainment and wages of children who were born in areas of intensive school construction to similar changes across cohorts of children in areas with less-intensive construction.

Esther uses estimates of cohort-specific changes in educational attainment as a function of the intensity of school construction, along with a series of robustness checks and placebo experiments to provide convincing evidence that it is the school construction program itself that is driving changes in enrollment. She then generates two-stage least squares estimates of the wage return to education, with an appropriately exogenous source of variation in education. She estimates a rate of return to education of 7–10 percent.



Esther Duflo

Throughout [1], Esther acknowledges that there may be general equilibrium effects of the massive increase in educational attainment generated by Indonesia's school construction program—that is, relative wages of educated and uneducated workers may be changing. These general economywide effects are explored in [2]. She shows that wages of cohorts too old to have benefitted from the school construction program rose less rapidly in districts where intensive school-building occurred than in districts less affected by the program. She explores with her typical care (and largely dismisses) the possibilities that these results were driven by selective attrition, migration, or changes in labor force participation. The dramatic increase in the local supply of educated workers associated with the school-building program led to a decline in the wages of older workers, whose education did not change. She interprets this result in the context of a dual economy model with a formal and an informal sector. She shows that her pattern of results is consistent with a closed economy model in which labor (both skilled and unskilled) is mobile across sectors (but not regions) and in which the accumulation of physical capital was uninfluenced by the dramatic changes in education. “Even 25 years after the program was initiated, physical capital does not seem to have been accumulated to employ the new efficiency units of labor created by the program” (p. 194). Therefore, wages of older workers fell in response to the increase in human capital generated by the new schools. One may speculate that this finding has something to do with Esther's subsequent work on capital market imperfections, credit constraints, and various forms of myopic behavior by investors.

Esther has used a variant of the difference-in-differences estimation strategy in a number of other papers as well, and has thought seriously about the econometrics of this procedure. She and her coauthors noticed that the combination of strong autocorrelation in the dependent variable, many time periods, and infrequent changes in the “treatment” indicator could, if ignored, conspire to lead to serious understatements

Table 1

Selected Papers by Esther Duflo

1. "Schooling and Labor Market Consequences of School Construction in Indonesia: Evidence from an Unusual Policy Experiment." 2001. *American Economic Review*, 91 (4): 795–813.
2. "The Medium Run Consequences of Educational Expansion: Evidence from a Large School Construction Program in Indonesia." 2004. *Journal of Development Economics*, 74(1): 163–197. (See also BREAD Working Paper No. 002, 2001).
3. "How Much Should We Trust Difference in Differences Estimates?" (with Sendhil Mullainathan and Marianne Bertrand). 2004. *Quarterly Journal of Economics*, 119(1): 249–75.
4. "Dams," (with Rohini Pande). 2007. *Quarterly Journal of Economics*, 122(2): 601–646. (See also NBER Working Paper No. 11711, 2005; BREAD Working Paper No. 100, 2005).
5. "The Miracle of Microfinance? Evidence from a Randomized Evaluation," (with Abhijit Banerjee, Rachel Glennerster, and Cynthia Kinnan). 2010. J-PAL Working Paper. <http://www.povertyactionlab.org/sites/default/files/publications/44-%20June%202010.pdf>.
6. "The Economic Lives of the Poor," (with Abhijit Banerjee). 2007. *Journal of Economic Perspectives*, 21(1): 141–67.
7. "Aging and Death under a Dollar a Day," (with Abhijit Banerjee). Forthcoming. In *Research Findings in the Economics of Aging*, ed. D. A. Wise. Chicago: University of Chicago Press.
8. "What is Middle Class about the Middle Classes around the World?" (with Abhijit Banerjee). 2008. *Journal of Economic Perspectives*, 22(2): 3–28. (See also CEPR Discussion Paper No. 6613, 2007; BREAD Working Paper No. 163, 2007).
9. "Giving Credit Where It Is Due," (with Abhijit Banerjee). 2010. *Journal of Economic Perspectives*, 24(3): 61–79.
10. "Growth Theory through the Lens of Development Economics," (with Abhijit Banerjee). 2005. In *Handbook of Economic Growth*, Vol. 1A, ed. Steve Durlauf and Philippe Aghion, 473–552. North-Holland, Elsevier.
11. "Do Firms Want to Borrow More: Testing Credit Constraints Using a Targeted Lending Program," (with Abhijit Banerjee). 2004. BREAD Working Paper No. 005.
12. "Inequality and Growth: What Can the Data Say?" (with Abhijit Banerjee). 2003. *Journal of Economic Growth*, 8(3): 267–99.
13. "Grandmothers and Granddaughters: Old Age Pension and Intra-household Allocation in South Africa." 2003. *World Bank Economic Review*, 17(1): 1–25.
14. "Poor but Rational?" 2006. In *Understanding Poverty*, ed. Abhijit Banerjee, Dilip Mookherjee, and Roland Benabou, 367–78. New York: Oxford University Press.
15. "Nudging Farmers to Use Fertilizer: Evidence from Kenya," (with Michael Kremer and Jonathan Robinson). Forthcoming. *American Economic Review*. (See also NBER Working Paper No. 15131, 2009).
16. "How High Are Rates of Return to Fertilizer? Evidence from Field Experiments in Kenya," (with Michael Kremer and Jonathan Robinson). 2008. *American Economic Review*, 98(2): 482–88.
17. "Improving Immunization Coverage in Rural India: A Clustered Randomized Controlled Evaluation of Immunization Campaigns with and without Incentives," (with Abhijit Banerjee, Rachel Glennerster, and Dhruva Kothari). 2010. *BMJ*, May 17. <http://www.bmj.com/content/340/bmj.c2220.full?sid=73fc574c-7f8f-4207-b920-74464525413a>.
18. "Saving Incentives for Low- and Middle-Income Families: Evidence from a Field Experiment with H&R Block," (with William Gale, Jeffrey Liebman, Peter Orszag, and Emmanuel Saez). 2006. *Quarterly Journal of Economics*, 121(4): 1311–46. (Awarded the 2007 Certificate of Excellence from TIAA-CREF Institute; see also NBER Working Paper No. 11680, 2005).
19. "Women as Policy Makers: Evidence from a Randomized Policy Experiment in India," (with Raghavendra Chattopadhyay). 2004. *Econometrica*, 72(5): 1409–43 (See also NBER Working Paper No. 8615, 2001; BREAD Working Paper No. 001, 2001).
20. "Powerful Women: Does Exposure Reduce Bias?" (with Lori Beaman, Raghavendra Chattopadhyay, Rohini Pande, and Petia Topalova). 2009. *Quarterly Journal of Economics*, 124(4): 1497–1540. (See also CEPR Discussion Paper No. 6922, 2008; NBER Working Paper No. 14198, 2008).

Table 1 (continued)

21.	“Health Care Delivery in Rural Rajasthan,” (with Abhijit Banerjee and Angus Deaton). 2004. <i>Economic and Political Weekly</i> , 39(9): 944–49.
22.	“Pitfalls of Participatory Programs: Evidence from a Randomized Evaluation in Education in India” (with Abhijit Banerjee, Rukmini Banerji, Rachel Glennerster, Stuti Khemani). 2010. <i>American Economic Journal: Economic Policy</i> , 2(1): 1–30. (See also CEPR Discussion Paper No. 6781, 2008).
23.	“Addressing Absence,” (with Abhijit Banerjee). 2006. <i>Journal of Economic Perspectives</i> , 20(1): 117–32. (See also BREAD Policy Paper No. 008, 2005).
24.	“Incentives Work: Getting Teachers to Come to School,” (with Rema Hanna and Stephen Ryan). Forthcoming. <i>American Economic Review</i> . (See also NBER Working Paper No. 11880, 2005; BREAD Working Paper No. 103, November 2007).
25.	“Putting Band Aid on a Corpse: Incentives for Nurses in the Indian Public Health Care System,” (with Abhijit Banerjee and Rachel Glennerster). 2007. <i>Journal of the European Economic Association</i> , 6(2–3): 487–500.
26.	“Remedying Education: Evidence from Two Randomized Experiments in India,” (with Abhijit Banerjee, Shawn Cole, and Leigh Linden). 2007. <i>Quarterly Journal of Economics</i> , 122(3): 1235–64. (See also NBER Working Paper No. 11904, 2005; BREAD Working Paper No. 109, 2005).
27.	“Peer Effects and the Impacts of Tracking: Evidence from a Randomized Evaluation in Kenya,” (with Pascaline Dupas and Michael Kremer). Forthcoming. <i>American Economic Review</i> . (See also CEPR Discussion Paper No. 7043, 2009; NBER Working Paper No. 14475, 2008).
28.	“The Experimental Approach to Development Economics,” (with Abhijit Banerjee). 2009. <i>Annual Review of Economics</i> , vol. 1, pp. 151–178. (See also CEPR Discussion Paper No. 7037, 2008; NBER Working Paper No. 14467, 2008).
29.	“Using Randomization in Development Economics Research: A Toolkit,” (with Rachel Glennerster, and Michael Kremer). 2007. In <i>Handbook of Development Economics</i> , Vol.4, ed. T. Paul Schultz, and John Strauss, 3895–62. Elsevier Science Ltd.: North Holland, Elsevier. (See also NBER Technical Working Paper No. 333, 2006).
30.	“Field Experiments in Development Economics,” 2006. In <i>Advances in Economics and Econometrics: Theory and Applications, Ninth World Congress</i> , Vol 2. ed. Richard Blundell, Whitney Newey, and Torsten Persson, 322–48. Cambridge University Press. (See also BREAD Policy Paper No. 002, 2005).
31.	“Finding the Right Questions,” 2001. <i>Newsletter of the Committee on the Status of Women in the Economics Profession</i> , Winter, pp. 4–5, 10.
32.	“Reputation Effects and the Limits of Contracting: A Study of the Indian Software Industry,” (with Abhijit Banerjee). 2000. <i>Quarterly Journal of Economics</i> , 115(3): 989–1018.

of the standard error of the estimated impact of the treatment. In [3], they present an eminently practical discussion of this problem, and propose simple solutions. This turns out to be the most cited of all of Esther’s papers.

Esther has characteristically reached beyond estimating the returns to education based on already-enacted government programs and studied the potential gains from programs that seek to boost education in other ways: through tutoring, monitoring teachers, paying attention to peer effects, and more. For example, an intervention designed by a nongovernment organization with Esther and colleagues generated remarkably strong gains in educational attainment by providing a “demonstration class” in randomly-selected villages to show how local volunteers could help children learn to read [22]. After these demonstration classes, many local volunteers set up reading classes in their villages. A year after the demonstration classes, Esther and colleagues find strong improvements in reading ability,

particularly for children at initially low levels, in the villages in where this intervention was implemented. Clearly, these villages have both a demand for and a supply of adults ready to make a contribution to schooling. However, this randomized evaluation provides no evidence that local community participation in governance of schools is effective in improving education. In [23], Esther and Abhijit Banerjee provide a useful review of the disappointing results of a variety of interventions that were designed to improve service delivery via community involvement.

Simple and direct incentives to improve education seem more effective. Esther along with Rema Hanna and Stephen P. Ryan [24] show that routine monitoring of teacher attendance (via taking a picture with a camera) combined with high-powered incentives (additional pay for attendance) has a strong effect on teacher presence in the classroom. This system was introduced in a random set of about 60 schools in Rajasthan, a state in northwest India. Date/time stamps on the photos recorded the teachers' presence. Teachers received a base pay and an additional bonus per day they attend beyond a minimum up to a maximum defined by the number of school days in the month. Teacher absenteeism dropped by half in the treatment schools relative to the control. There is no evidence that the attendance incentive changed teacher effort in other dimensions, and the number of days children were in school increased along with teacher attendance. Student learning improved: test scores in the treatment schools were about 0.2 standard deviations higher than in the control, and more students graduated into the next level of education.

In this paper, Esther and her coauthors go well beyond a standard evaluation of this specific intervention to estimate a pair of structural models of teacher labor supply to permit an analysis of the prospective effects of alternative compensation packages. They can identify the parameters of the model because the incentive scheme has strong nonlinearities that change the shadow value of working over time. Incentives change in a discontinuous way with each new month, and then evolve depending on the number of days worked so far and the number of days remaining in the month. The models permit teacher-level unobserved heterogeneity, and the two models vary in their assumptions regarding the dynamics of the opportunity cost of working. The models are estimated on the treatment group (since this is the group with nonlinear incentives) and then tested out-of-sample on the control group, where the incentive is set to zero. Esther and her coauthors are successful in matching the out-of-sample decisions of un-incentivized teachers, and in addition do well in accounting for changes in labor supply after the close of the experiment, when a new incentive scheme was introduced.

With estimates from the structural model in hand, it is possible to calculate a cost-minimizing incentive scheme by hypothetically varying the size of the bonus and the threshold base number of days of attendance. Indeed, the new incentive scheme alluded to above was introduced after the model was estimated and the first version of the paper written, and moves in the direction of the cost-minimizing scheme calculated by Esther and her coauthors. This paper provides a welcome and important contribution to the small literature that combines the techniques of structural modeling with the strong levers for econometric identification that

can be provided through a randomized intervention. At one level, the substantive conclusion is not that surprising: incentives improve teacher attendance. However, the paper also shows that this feeds through all the way to student accomplishment, which is not obvious. Moreover, the paper provides convincing evidence on important behavioral parameters—for example, the wage elasticity of the supply of teacher labor is between 0.2 and 0.3—which permits discussion of the effects of a variety of alternative policies for improving public service delivery.

In urban areas in India, teacher absence is not the dramatic problem we find in rural areas, yet student achievement remains remarkably low. In Vadodara, a major city included in one of Esther's studies, fewer than one in five grade 3 students can answer correctly grade 1 math test questions. Esther with several coauthors examine a pair of added-resource interventions designed to improve learning in schools that serve poor urban families [26]. One involves remedial "pull-out" services in which a paraprofessional from the community works with poorly performing students in grade 3 or 4 outside of their regular classroom. The second is a computer-assisted learning program in which grade 4 children play games that involve math puzzles on a shared computer for two hours a week. Esther and her coauthors find substantial positive short-run effects of both interventions. The relevant test scores in the treatment schools were 0.15 to 0.35 standard deviations higher in treatment than in control schools while the program operated. These large gains attenuated over the following year after students left the program, but remained significantly positive. Both of these programs are relatively inexpensive and were designed in a fashion that permits a move to scale.

The design of this project also permitted Esther and her colleagues to look inside the classroom to examine the mechanisms through which the remedial "pull-out" program improved average test scores. There are two potential pathways: 1) the program may have had a direct effect on the children who left the classroom and received additional instruction, and 2) the children staying behind had a smaller, more homogeneous, and higher-performing group of classmates. Using initial test scores as an instrument for direct participation in the pull-out program, it is possible to estimate the gain in test scores for those who were tutored, and the gain for those left behind in the classroom, relative to comparable control group children. The entire effect of the program appears to have been on the direct participants; there is no evidence of any indirect class-size or peer-group effect.

In contrast, Esther, Pascaline Dupas, and Michael Kremer [27] show strong effects of tracking via two distinct mechanisms in an experimental study in Kenya. A set of schools were provided with an additional teacher to divide their first grade into two classrooms. A random half of the schools assigned the students to sections randomly; in another half of the schools, students were assigned to either a "high" or a "low" track depending on initial achievement. Students scoring in the high and low halves of the pre-assignment assessment gained similarly (0.15 to 0.2 standard deviations in test scores), and these effects were persistent for at least one year after tracking ended.

Esther and her coauthors construct a model that embeds two main effects of tracking. One is a direct peer effect, in which students benefit from having higher-achieving peers. The second is that tracking may permit teachers to better

match their instructional choices to their students' needs. Either or both of these mechanisms could affect teacher effort as well, depending upon the way teacher payoffs respond to the distribution of student achievement. The model generates rich predictions regarding the effect of the tracking program on different parts of the distribution of initial achievement. For example, because tracking raised performance for all students, teacher behavior must be adjusting to the composition of their classes; this is the only mechanism that can generate benefits at the bottom of the initial distribution.

The tracking schools provide a superb setting for a regression discontinuity analysis because each initial first grade class is split into two at a different point in the distribution of initial achievement, providing a separate discontinuity point for each school. Esther and her colleagues use this feature of the data to show that the bottom-ranked (by the initial achievement test) students in the upper class and the top-ranked students in the lower class gain equally from the tracking. And these students gain as much from the tracking as do students elsewhere in the distribution. These results also imply that teachers adjust their behavior to class composition, and more strikingly, that the teacher's reward function is convex in the distribution of final scores. To see this, note that linear rewards would imply that teachers teach to the median of the distribution of children in their class; this in turn would imply that 1) students starting in the middle of the prior distribution of achievement would do less well under tracking, because they now move to the extreme of the post-tracking distribution, and 2) students just above the median would do better than students just below, because they would gain from higher-achieving peers. A convex reward function would imply that teachers focus on the upper tail of their students, consistent with these empirical results. Their model also implies that if the teacher payoff function is convex, the teacher (randomly) assigned to the lower track in tracking schools will devote less effort than a colleague teaching the upper track, and this is indeed the case.

The random assignment of students to classes in the nontracking schools is also informative about the learning process. An exogenous shift up in the distribution of prior achievement (scores on the pre-assignment assessment) in a class will strongly benefit students in the upper tail: they benefit from both the peer effect and from a closer match to material taught. For students further down in the distribution, the gains from this shift become ambiguous as the positive peer effect is balanced by the negative effect of the upward shift in the instructional target. Esther and her coauthors find that the top students benefit strongly from an improvement in the prior achievement of their classmates, while there is no effect in the middle of the distribution. A positive effect reemerges at the bottom of the distribution, presumably because these children were already so far below the level of the instructional target that the effect of a further mismatch is outweighed by the positive peer effect. This paper both provides a rigorous evaluation of a specific development intervention, and uses the variation generated by the randomized control trial to investigate the mechanisms that underlie the intervention's effect. Here, we are able to peer inside the classroom and see peer

effects, teacher effort, and the targeting of instruction move in response to the environmental changes induced by the intervention.

The Economic Lives of the Poor

In [6], [7], and [8]—with two of these papers appearing in this journal—Esther and her coauthor Abhijit Banerjee provide rich descriptions of the economic lives of the poor and middle class in a wide range of developing countries. These papers use survey data from 13 countries to provide a broad overview of the consumption patterns, asset holdings, earnings profiles, health status, and market and economic environment of, in turn, the poor (consuming less than \$2/day) and the middle class. These masquerade as purely descriptive papers, unconcerned with formal modeling or statistical identification. But, of course, there is a message.

Some of what we learn is unsurprising. Perhaps the key distinction between the poor and the middle class is that the latter tend to have jobs with a regular paycheck. The poor live in large families, in small homes, devoting a high fraction of their expenditure to food and very little to entertainment. Apart from land, the poor own few assets. The credit and insurance markets available to the poor are almost entirely informal, if present at all. It is difficult for the poor to find safe and reasonably remunerative ways of saving. Their businesses are tiny.

In [6], Esther and Abhijit Banerjee argue that there is some slack in the consumption budget even of the poor. Respondents name alcohol, tobacco, sugar, tea, snacks, entertainment, and festivals as items they would like to cut. (I'm tempted to look for some hidden Puritans in Esther and Abhijit's family histories!) Combine this finding with the fact that many of the poor have high-interest debt while many others have businesses with apparently high returns to investment, and a puzzle arises. Almost everyone spends on festivals, which shows that the poor do save when they have a goal that is salient. A simple inability to commit is not the problem. Esther and Abhijit conclude this paper with the troubling comment that "one senses a reluctance of poor people to commit themselves psychologically to a project of making more money. Perhaps at some level this avoidance is emotionally wise: thinking about the economic problems of life must make it harder to avoid confronting the sheer inadequacy of the standard of living faced by the extremely poor" (p. 165).

Esther's work has focused on three types of constraints that hem in the lives of the poor and block growth: financial market failures; the organization of households; and behavioral constraints.

Much of Esther's thinking about financial markets in developing countries appears in a set of conceptual pieces written with Abhijit Banerjee [9], [10]. They focus on empirical findings from a variety of sources that indicate a willingness to borrow at extremely high interest rates with no correspondingly high default rates; widely varying interest rates over small geographical areas; very high rates of return to investment for many entrepreneurs; and tremendous variation in the returns to

investment across businesses within countries (and within sectors in countries). All of this points to a set of important financial market failures.

Esther again goes beyond characterizing the evidence on the possible existence of credit constraints to an examination of the economics of specific programs intended to alleviate their consequences. With a number of coauthors, Esther provides the first large-scale evidence from a randomized evaluation of the expansion of a microfinance institution [5]. The microfinance institution randomized the neighborhoods in the slums of Hyderabad into which it expanded. Esther and her coauthors show that this expansion is associated with increased borrowing from the specific institution, but that after 12–18 months, this increased borrowing has no effect on per capita expenditure on overall consumption and no effect on health, women’s empowerment, or education. Instead, existing business owners use the credit to expand their enterprises (durable goods expenditure increases by almost 50 percent). Even more interesting, individuals with a high predicted likelihood of opening a new business increase their expenditure on durables and reduce their consumption of nondurables. This pattern is consistent with the model that they must save to meet a fixed entry cost to entrepreneurship and that microfinance borrowing provides a commitment mechanism that permits them to overcome time-inconsistent preferences that previously hindered such saving. Thus, while this paper is mostly concerned with evaluating the effects of the expansion of microfinance into poor urban neighborhoods, Esther and her coauthors also are able to examine some of the underlying mechanisms through which these effects are realized. Understanding the constraints that “keep them stuck” has been a central focus of much of her work.

In [11], Esther and Abhijit argue that even relatively large firms in India are credit constrained. They show that firms that became eligible for inexpensive directed credit and then ineligible again (as a consequence of a sequence of policy changes) used those additional resources not to substitute away from existing credit, but rather to finance an expansion of production. The rate of return from this expansion was remarkably high, about 75 percent.

Nonconvexities in production technologies are central to Esther and Abhijit’s understanding of the implications of the microeconomic evidence on imperfections in the allocation of resources across firms for aggregate growth patterns in [10]. Nonconvexities arise, for example, when there are fixed startup costs to begin production, which in turn may be a consequence of a minimal scale of the machinery involved, of startup marketing costs, or of the need to develop a reputation [32]. In general, they envision a class of growth models in which factor prices (and occupational choice) depend upon the distribution of wealth via the interaction of imperfect financial markets and production nonconvexities.¹ A robust

¹ Banerjee and Newman (1993) is obviously a source for their thinking on this, but they draw on and relate to a rich literature including, for example, Galor and Zeira (1993), Mookherjee and Ray (2003), Lloyd-Ellis and Bernhardt (2000), Townsend and Ueda (2006), and Buera (2008). There is also an important set of political economy models that generate interactions between inequality and growth,

implication of these kinds of models is that the relationships between growth, the level of income, and inequality are typically nonlinear. For example, in a very poor country with fixed costs of entry to a productive sector (and capital market failures), a small increase in inequality may quicken growth (as more people can invest), while a similar increase in inequality in a richer country could reduce growth via diminishing returns. In [12], they explore the consequences of this observation for the large literature based on cross-country regressions of the relationship between these variables. Their conclusions are sobering: “[T]here is no reason to expect that we can learn about the relationship between inequality and growth by running linear cross-country regressions. There are no strong grounds for thinking that the right specification would be monotonic, let alone linear.” They go on to show that the cross-country data provides evidence for strong nonlinearities in the relationship between inequality and growth. Moreover, these nonlinearities provide a consistent interpretation of the widely varying estimates of the relationship between inequality and growth that emerge from different specifications.

In [14], Esther notes that development economists have spent much of the past few decades illuminating the market imperfections that push the allocation of resources (very) far from the first-best Pareto efficient equilibrium. However, virtually all of this work (including my own) maintains the assumption of individual rationality. A typical empirical paper in development assumes (either explicitly in a motivating model, or implicitly in the authors’ formulation of the empirical strategy) that agents choose actions to maximize a utility function generated by a stable set of well-behaved preferences. Esther is a pioneer in an emerging literature that documents a set of behavioral biases that make matters rather less clear-cut.

In [15], a superb example of work that examines this set of issues, Esther, Michael Kremer, and Jonathan Robinson set up a long-term sequence of experiments with a set of farmers in western Kenya to try to understand why they don’t use fertilizer despite its apparent profitability. They began with a series of randomized control trials on farmers’ fields to measure the profitability of fertilizer use in [15] and [16]. The results are striking. Farmers are correct to ignore the recommendations of the Ministry of Agriculture; its recommendations are unprofitable. However, there is an intermediate level of fertilizer use that generates a high annualized rate of return, somewhere between 50 and 85 percent (the uncertainty is generated mostly because of the lack of data on labor inputs, so it has to be imputed from comparable Kenyan data from Suri (2011)). This translates into potential gains of between \$10 and \$15 per household per year; not monumental, but significant. So why aren’t farmers using fertilizer?

If you ask farmers, they almost invariably reply that they would love to use fertilizer, but that they don’t have the money. This is a bit difficult to understand given that fertilizer is not a binary choice: many farmers would seem able to afford at least some if they wished to do so. Esther and her colleagues are able to rule out many

which Esther and Abhijit Banerjee drew on in [12] including Alesina and Rodrik (1994), Persson and Tabellini (1994), and Benhabib and Rustichini (1998).

of the hypotheses that development economists would immediately offer for why farmers are not using an apparently profitable technology, such as lack of information or heterogeneous returns. They provide a model in which some farmers are stochastically present-biased and not fully sophisticated, with the result that farmers systematically underestimate the probability that they will be present-biased in the future. Purchasing fertilizer has some small fixed cost in utility. With discounting, the existence of this fixed cost implies that farmers who plan to use fertilizer will defer that purchase until the last possible moment. But then some farmers will surprise themselves by being impatient in the crucial moment before the application of fertilizer and hence decide not to use fertilizer at all.

A fertilizer subsidy could induce the last-moment present-biased farmer to use fertilizer, but involves a heavy cost and induces overuse of fertilizer by non-present-biased farmers. Instead, the model points to a specific, modest nudge that would overcome this set of behavioral biases. A small, time-limited discount offered for fertilizer purchase right after harvest could induce significant changes in behavior. This subsidy would just need to be large enough to make up for the farmer incurring the small fixed cost from choosing to use fertilizer now rather than later, plus the opportunity cost of the capital committed to the fertilizer during the period from just after harvest to just before planting. Esther and her collaborators designed an intervention to do precisely that, and randomized its availability in a sample of farmers (along with a set of other interventions designed to rule out alternative hypotheses). Farmers who were offered a small subsidy (actually, just free delivery) right after harvest increased fertilizer use dramatically—10 to 20 percentage points from a base of about 25 percent. Farmers offered a 50 percent subsidy along with free delivery later in the season increased their use by about the same amount, consistent with the theory and with a calibrated example.

This paper combines a beautiful, simple theory that has strong implications for behavior with a long-term engagement with farmers that permitted Esther and her colleagues to design a sequence of randomized trials that provide very strong evidence that farmer investment decisions are influenced by a particular constellation of behavioral biases. Moreover, they were able to design a sequence of auxiliary experiments to explore alternative hypotheses. For example, one interpretation of the results so far is that farmers are time-consistent, the returns to fertilizer are low, and the return to saving is lower than the discount rate. In this case, a small time-limited subsidy just after harvest would be more effective in inducing take-up than would be a similar subsidy announced just before planting. The research team is able to reject this model by examining the responsiveness of farmers to alternative interventions in which subsidies available just before planting were offered just after harvest (but delivery and payment were scheduled for just before planting). Many farmers agreed to take up these future subsidies, as indeed all fully time-consistent farmers would. However, none in fact were able or willing to pay for the subsidized fertilizer when the time for planting finally arrived. Similar auxiliary experiments are used to examine an array of plausible alternative hypotheses; in the end, even a relative skeptic like me finds himself utterly convinced. Agricultural investment

in western Kenya is shaped by the partially-naive, stochastic present-bias of small-scale farmers. As a consequence, profitable use of fertilizer is foregone, and farmer households sacrifice a small but noticeable amount of income.

Esther has a set of other papers that broadly address issues in behavioral economics. With several coauthors, she uses a randomized control trial to show a remarkably high price elasticity of demand for child immunization in [17]; Kremer and Holla (2009) interpret this and similar high elasticities for health investment goods in terms of time-inconsistent preferences. The strong effects of presentation of information about retirement savings on U.S. workers that she documents with several coauthors in [18] addresses a parallel theme in the behavioral literature.

Women as Economic Decisionmakers

Esther's paper [13] on the intrahousehold allocation of resources in South Africa uses the receipt of the large cash benefits associated with the South African old-age pension system to provide a very convincing case that the identity within a household of the recipient of a transfer matters for expenditure decisions. This is a striking rejection of the so-called "unitary household model," in which the household is modeled as a single decisionmaker, and has important policy implications for the design of cash transfer or welfare systems. The old-age pension provides a very large (for Africans) transfer to households with sufficiently old members. The most convincing evidence in this paper comes from Esther's imaginative construction of an effective panel dataset from her single cross-section, using the fact that child height reflects a child's nutritional history.

She compares the height of children in families eligible for the pension to those in families not eligible among children born before the old-age pension was extended, and to the same difference among children who were exposed to the pension for their entire lives. If the pension transfers are associated with better nutrition, then this difference should be greater among children who were exposed for longer. She finds that having a woman (typically a grandmother) in the household eligible for the pension strongly improves the nutrition of girls. There is no effect for boys, and the receipt of a pension by a man in the household has no effect on the nutritional status of any children. The opportunities for children, then, are shaped by whether the resources are made available to men or to women in the household.

A similar pattern emerges in politics. Esther's program of research in political economy has yielded insights about the nature of political competition, the implications of quota systems for holding political office, the politics of allocating local public goods, and gender stereotyping and bias in elections. She takes advantage of a massive, randomized program of reserving positions of leadership in local governments in India for women and members of historically disadvantaged castes and tribes. These reservation policies typically bind: very rarely are women elected to leadership positions that are not reserved for women. Esther's political economy work has focused on the experience of this system of political reservation in local

councils in West Bengal and Rajasthan states, where the specific rules implementing the policy ensure that a random one-third of “Pradhans” (local council heads who are the only full-time member of their council)—are reserved for women. In addition, a random one-third of the seats on the council are reserved for women. Because reservation status is randomly allocated across councils, differences across councils in political outcomes and political processes can be attributed to the reservation status.

In [19], Esther and Raghavendra Chattopadhyay show that the identity of the local council leader matters for policy decisions: where the Pradhan position is reserved for a woman, the local council invested more heavily in public goods that were more closely linked to women’s concerns. The result is strong and perhaps surprising. The workhorse median voter model implies that the identity of the Pradhan would have no influence on realized policies on public good provision; similar consequences emerge in a Coasian world, or in a situation in which the women served as “shadow Pradhans” who are really covers for their husbands or the local elite.

The paper provides a basic citizen candidate model (Osborne and Slivinski, 1996; Besley and Coate, 1997). In this model, politicians cannot commit to specific policy choices. After the election, his or her own preferences influence his or her actions. Hence, the identity of the leader can influence policy realizations. Esther and her coauthor hypothesize that men and women have systematically different preferences, so reservation of a position for a woman changes the eventual policy realization. Under most conditions, the policy outcomes will be closer to what women want in reserved councils than in unreserved councils. To measure gender-specific preferences, the paper relies on data on formal comments that men and women bring to the local council. Submitting a complaint is costly, so these data may provide information on differential preferences across goods by gender. Esther and her coauthor provide a model that illuminates the conditions under which this is true and provides paths for testing these conditions.

Local councils reserved for women leaders invest more in the public goods more closely linked to women’s concerns: drinking water and roads in West Bengal and drinking water in Rajasthan. And they invest less in public goods relatively associated with men: education in West Bengal and roads in Rajasthan.² The randomization makes us confident that these differences are a consequence of the reservation status of the local council, but it is not yet clear that they can be attributed fully to the gender of the Pradhan. Women elected as Pradhan in reserved councils differ from men in many dimensions: in particular, they are much more likely to be new politicians and they are much less likely to be re-elected. In an elegant section of this paper, Esther and her coauthor use specific features of the rotation system of reservation to disentangle the effect of gender from the effects of

² These spatial variations in gender-based policy preferences are related to the economic environment. Women provide the bulk of the labor for roads in West Bengal, but not Rajasthan. Foster and Rosenzweig (2004) examine similar questions and show how differences in preferences over policy can be derived from a model that links identified populations with particular economic interests.

experience, lame duck status, and some dimensions of social status. Their strategy works off the interplay of the two levels of reservation: the Pradhan office, and the regular council seats.

For example, a random subset of unreserved Pradhans will be newly-elected as a consequence of the reservation system. If the previous Pradhan had been a man, and his seat on the council is reserved for a woman, then he cannot run for re-election on the council. Hence the Pradhan for that council will be freshly-elected (and almost surely male, since the council leadership—the Pradhan position itself—is not reserved). This suggests comparing policy outcomes in local councils reserved for a female Pradhan to those in local councils in which the former Pradhan is excluded by reservation (of his own seat); in both instances the Pradhan is newly elected. The results are virtually identical to those for the whole sample: the reservation-induced changes in policy are not attributable to the newness of female Pradhans. Similar exercises make us confident that the policy outcomes are not driven by the fact that women are not likely to run again (by comparing outcomes in local councils that will be reserved for women in the next elections) or due to the lower social status of the women who are elected under reservation (by comparing outcomes in local councils reserved for Scheduled Castes or Scheduled Tribes). The policy changes associated with reserving the local council leadership position for women indeed seem to be a consequence of the effect of the policy on the gender of the Pradhan. This finding is important both for its direct policy implications—gender quotas are being implemented at various levels of government in a wide array of countries—and because it shows that the identity of policymakers affects policy choices.

In [20], Esther and several coauthors examine the effects of political reservation on voter attitudes towards women leaders. Suppose that voters have some taste-based preference for male leaders. If voters are risk-averse, and there is some uncertainty about the quality of candidates for office, then this initial preference for male leaders can be reinforced by statistical discrimination as voters become more familiar with (and better able to judge) male candidates. A temporary policy of affirmative action could have a long-run effect by providing voters with the opportunity to learn more about how to judge female candidates, thus reducing statistical discrimination. The authors of this paper find dramatic increases in female participation and victory in elections that were not reserved for women in those local councils that had previously been reserved for women in two successive election cycles (over 10 years). The paper also examines changes in voter attitudes towards female politicians, which is one mechanism through which the reservation policy could influence success of future female political leaders.

Esther and her coauthors devise an innovative program of field research to collect both explicit and implicit measures of voters' tastes regarding male and female leaders and perceptions of effectiveness. They collect explicit "feeling thermometer" data on how villagers feel about the general idea of male and female village leaders. To measure subtler bias, they adopt a set of "implicit association tests." I highly recommend trying one of these if you have never done so; they

are interesting and thought-provoking (there is a set available online at <https://implicit.harvard.edu/implicit/demo/>). For this paper, they use three measures, two of which are designed to measure villager tastes for male or female leaders, the final one which examines the strength of stereotyping men and women into leadership and domestic tasks. In addition, they use a series of vignettes and recorded speeches in which the gender of the leader is varied to examine villager perceptions of male and female leader effectiveness. None of these measures captures perfectly the ideas in their model of the “taste” for male/female leaders or the voter perceptions of the effectiveness of male or female leaders, but it is plausible that the “feelings” measures are closer to the former, while the “effectiveness” questions reflect perceptions of actual performance.

They find no evidence that deeper tastes for male versus female leaders become less biased against women as a consequence of the reservation of Pradhan positions for women. There is (unsurprisingly, perhaps) a strong, deep preference or social norm in West Bengal villages against women in leadership positions. Both genders express an explicit distaste for female leaders, and the taste version of the implicit association tests show strong same-gender preference that does not change after reservation. There is no evidence that experience with female local leaders over a five- to ten-year period influences these preferences or norms.

However, beliefs about the effectiveness of women as leaders are much more flexible and respond strongly over that same time frame. Reservation of the Pradhan position for a woman strongly improved men’s judgments of the effectiveness of female leaders. Notable electoral gains for women in unreserved positions were associated with these changing beliefs. Esther and her colleagues have provided strong evidence that gender-based political reservation may have durable effects on attitudes towards female leadership and provide a route towards increasing the participation of women in governance.

What Works

One dimension of Esther’s research is in the tradition of program evaluation. What is the impact of a specific intervention on outcomes of interest? There are a multitude of programs and policies that have been proposed as levers to improve the lives of the poor. Which of these work? How much of an effect do they have?

Esther and Rohini Pande (4) examine the effects of large-scale dam construction in India. They combine a panel of district-level data on agricultural production and poverty with information on the construction of about 2,500 dams over a period of almost 30 years. A simple comparison of changes in agricultural output or poverty in areas with and without dams is potentially quite misleading, because obviously areas in which dams were constructed are likely different on a variety of dimensions, most importantly agricultural productivity. They use the fact that dams for irrigation are built on rivers that flow gently as the key to their identification strategy. Rivers that are flat will be unlikely candidates for dams; dams may appear

on rivers that are somewhat steeper, and then as the steepness increases irrigation dam construction again falls off (and hydroelectric dam construction picks up as the gradient becomes very steep).

Thus, they use the gradient as a source of exogenous variation for looking at the effects of dams on agricultural productivity.³ They show that dam construction is associated with increases in agricultural production in downstream districts but not with production in the district of construction, and that poverty is moderately reduced in downstream districts while increasing dramatically in the districts of dam construction. On balance, overall agricultural output increases with dam construction, but so does overall poverty. In essence, there is a failure to redistribute the gains associated with dam construction to those who suffer from their construction. Of course, this result does not mean (as Deaton, 2010, reminds us) that any particular dam project can be supposed to have these effects on poverty or agriculture, but nonetheless, it suggests a higher standard of evidence when a dam is proposed.

Esther has been engaged for a number of years in an extensive research program examining the delivery of public services to the poor. The state of publicly provided services like basic education and health care is deplorable in many countries (World Bank, 2003). Health and education services are often plagued by very high levels of provider absenteeism and very low quality of services. For example, Esther, with Abhijit Banerjee and Angus Deaton [21] show that small rural government clinics in Rajasthan, India, are closed more than half the time during their regular opening hours; people substitute by using unregulated and often unqualified traditional healers. Esther has been working to understand why and to examine potential remedies.

Many development practitioners argue that beneficiary participation in the management of service delivery can make schools and clinics operate more effectively. Esther and several colleagues cast cold water on local community participation as a panacea for poor service delivery in [22]. They work with the flagship educational nongovernment organization in India to randomize the introduction of interventions designed to support the work of local “village education committees” across almost 300 villages in Uttar Pradesh. These village education committees are designed to be the key intermediary between parents of school children and the district educational authorities. They have some direct control over school operations, and are tasked with providing a channel for the village to lobby higher authorities for resources or changes in school activities. The interventions designed by the nongovernment organization with Esther and her colleagues included two “best practice” programs to improve the effectiveness of these village education committees, involving training of the committee members themselves, outreach

³ Actually, they use the interaction of river gradient with the number of dams built in India in a given year times the number of dams in that state in a baseline period. The key identifying assumption is that this interaction of river gradient and number of dams does not influence economic activity independently, conditional on the interaction of dam construction with overall district topographical measures.

and education for the entire village, and community participation in measuring and evaluating children's educational outcomes.

While the interventions improved the knowledge of committee members of their own potential role in the educational system, there is no evidence that the interventions brought any change in the level of engagement of parents with the educational system, nor any change in the resources available to schools, nor any change in attendance by children. Most importantly, there is no change in learning after these community-building interventions.

Esther, Abhijit Banerjee, and Rachel Glennerster [25] worked with a nongovernment organization and the state and local health administrations in Rajasthan to examine the effects of routine monitoring (via a type of time-clock) and incentive pay (deductions for missing work too often) on the attendance patterns of Assistant Nurse-Midwives. This program was initially successful—after six months attendance was dramatically improved in treatment clinics relative to the control. However, over the next ten months, the system fell apart, and at the close of the experiment, there was disastrously high absenteeism (over 60 percent) at both treatment and control clinics. Over time, the local and state administration cooperated with the nurses in dismantling the system: the nurses broke the machines while the administration issued excused absences so that pay was not deducted for absence. Esther and her colleagues provide a number of reasons for this outcome: bureaucratic struggles within the administration, diffuse responsibility over the healthcare system, and the difficulty people face in evaluating the quality of the health care they receive.

Scholar-Activist and Public Intellectual

While constructing this extraordinary record of research, Esther has devoted enormous energy and enthusiasm to institution building. She was a founder and a primary driving force behind the creation of the Bureau for Research and Economic Analysis of Development, a leading professional organization of development economists. She is a co-director of the London-based Centre for Economic Policy Research program in development economics. But she is best known globally as one of the founders of the Jameel Poverty Action Lab at MIT. Her vision of J-PAL as a network of professors united by their use of randomized control trials to evaluate interventions designed to alleviate poverty has turned out to be exceptionally powerful.⁴

Over J-PAL's almost decade of existence, and I believe in large measure due to the example of its members and its institutional support, the tools of randomized evaluations have become much more common and prominent in development economics. These tools draw on a rich tradition of social experiments like the four "negative income tax" experiments carried out from the late 1960s into the early 1980s, and

⁴ I should note that I recently joined the J-PAL Board.

also on the active and growing use of lab experiments (sometimes “in the field”) in economics. Esther’s own research has played an important role in the improvement and popularization of these tools, and she has, with a variety of coauthors, written a set of extremely useful guides, reviews, and discussions of the use of social experiments in development economics in [28], [29], and [30]. Her extraordinary teaching and mentorship of students and research assistants has also contributed to the spread of these tools. She began the worldwide J-PAL training courses, which have taught these tools to a broad range of practitioners and researchers. Esther has devoted enormous energy to encouraging donors, policymakers, and development nongovernment organizations to evaluate their work rigorously and to base funding decisions and the design of future projects on the outcomes of these evaluations. She has been persuasive enough that her efforts have contributed to the significant recent changes we have seen in the practices of several major institutions, including the World Bank.

Inevitably, this change in the field of development economics has occasioned some critical comment (Barrett and Carter, 2011; Basu, 2005; Deaton, 2010; Ravallion, 2009; see Heckman, 1992, for a precursor of much of the discussion). The critics have raised important and interrelated issues about randomized control trials, including the role of theory, dealing with essential heterogeneity, compliance issues, ethical dilemmas, the narrow focus on mean treatment effects, external validity, and equilibrium effects. There’s been a lot of heat, but also a good deal of value in the methodological discussions. To me, the most important commentary on the randomized control trial movement in development economics is that some researchers may be seduced by the notion that this method relieves one of the responsibility to think carefully and systematically about the mechanisms that underlie the changes associated with any particular development intervention. It is possible to see how this could happen; indeed, it’s not too difficult to find examples. But Esther has worked hard to show that experimental tools are most valuable precisely in those situations in which they provide the key to revealing otherwise hidden mechanisms. We need to understand the way constraints and opportunities interact in the context of a policy or program. In her work this theme is typically very explicit (for examples, see the discussions in [15], [20], [24], [27]). With Abhijit Banerjee ([28], p. 174) she writes “to be interesting, experiments need to be ambitious, and need to be informed by theory. This is also, conveniently, when they are likely to be the most useful for policymakers. . . . [Economists] are often in a position to midwife the process of policy discovery, based on the interplay of theory and experimental research.”

We can’t randomize Esther Duflo to determine her causal effect on the field of economics. Even so, it’s safe to say that she is one of those rare intellectuals who can inspire an entire cohort of scholars to follow and build on her example. She has had a major positive effect on the renaissance in development economics over the past decade. The excitement that her research has generated in the field is palpable; her papers are taught in all the major Ph.D. training programs, and her ideas about evaluating development effectiveness have spread broadly through

the community of development practitioners and the general public—through her column in *Libération* and increasingly frequent media appearances—as well as through the profession. I’m shocked to realize that she’s done this all in just a bit more than a decade. We have a lot to look forward to.

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