Eighty percent of the world’s population lives in “developing or emerging” countries. Living conditions are often dire: almost 2.5 billion people live on less than $2 per day, over one billion remain without access to electricity, and over one billion people still defecate in the open. Some challenges for which seemingly simple solutions are readily available continue to cause great harm: indoor air pollution generated from traditional cook-stoves accounts for 22% of communicable child deaths globally (WHO 2005). Poor sanitation is estimated to cause 280,000 deaths annually (Pruss-Ustun et al 2014). A central puzzle in development is that effective, inexpensive technologies with the potential to address many of these problems exist, but are often not adopted or used. Prominent examples include health products (insecticide treated bednets, toilets, improved cookstoves), along with financial services (insurance, savings), agricultural technologies (fertilizer), and other rational response behaviors (e.g. rural-urban migration) that improve economic productivity.

A substantial portion of my research explores different aspects of these low-adoption problems and their development consequences. I test the predictions of microeconomic models of technology adoption and behavior change by implementing large-scale field experiments that involve marketing new technologies. I describe below four distinct (but related) aspects of this research program, and the table below notes one key contribution under each of these themes:

<table>
<thead>
<tr>
<th>Theme</th>
<th>Key Paper</th>
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<tr>
<td>(a) The decision-making environment for the extreme poor, focusing on how risk and price sensitivity keeps demand low,</td>
<td>“Under-investment in a Profitable Technology…”, <em>Econometrica</em> 2014</td>
</tr>
<tr>
<td>(b) The role of social networks in technology adoption</td>
<td>“Social Learning and Communication,” R&amp;R, <em>Review of Economic Studies</em></td>
</tr>
<tr>
<td>(c) Gender-specific determinants of adoption</td>
<td>Intra-household Externalities and the Low Demand for Improved Stoves, R&amp;R, <em>Economic Journal</em></td>
</tr>
<tr>
<td>(d) General equilibrium consequences of marketing and deploying new technologies</td>
<td>“Risk, Insurance and Wages in General Equilibrium”</td>
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This research agenda not only contributes to our understanding of development, but it also sheds light on the challenges associated with creating markets for new products in emerging countries. The developing world includes some of the world’s largest and most rapidly growing economies. India, Brazil, Bangladesh, Indonesia—a few of the countries where I conduct field research—have a combined population of almost two billion, and have all experienced average annual growth rates exceeding 6% in recent years. These economies are an increasingly influential part of the global business environment, and represent new markets and sources of inputs for multinationals. Devising more effective marketing strategies for these populations on the basis of rigorous behavioral research is integral to the mission of any business, economics, or public policy program.
Technology Adoption Decisions of the Poor

Analysis of technology adoption in developing countries must recognize that the poor may face fundamentally different constraints, and this contributes to the problem of low adoption. People may simply not have the money (or willingness) to spend on a new product like an improved stove, when their outside option (cooking with a traditional stove) is free. They may lack information about the health consequences of current practice. They may be too risk averse to spend money on a technology with uncertain benefits, especially when they are extremely poor and face competing demands for those funds.

I document the problem of low demand for improved cookstove technologies in [Mobarak et al., “The Low Demand for…”, PNAS 2013], using a stove marketing experiment. The study shows that the demand for improved stoves is extremely price elastic. The paper also documents that these consumers are aware of the adverse health consequences of traditional cooking. Accordingly, a simple information campaign about the health benefits of switching to improved stoves generates very low uptake in the field experiment without the addition of price subsidies.

The importance of price relative to information failures is also evident in another “technology” I marketed in rural Bangladesh: seasonal migration to cities to help agrarian households cope with a seasonal famine. Such pre-harvest lean seasons are common across South and South-East Asia and sub-Saharan Africa. Farmers receive most of their income at harvest, and wages fall and employment opportunities become scarce during the pre-harvest period. Noting that urban labor markets are less susceptible to such seasonal fluctuations, I randomly assigned either cash, or credit incentives, or information about job opportunities, to induce seasonal migration to nearby cities. We first document, using a multi-year program evaluation in [“Under-investment in a Profitable Technology…”, Econometrica 2014] that the intervention was very effective: a $8.50 grant or loan (conditional on migration) induced 22% more households to send a migrant, those migrants earned $110 at the destination, and their families consumed 600 calories more per person per day. About 75-80% of the migrants had a “successful” labor market experience in the city, and about half the induced migrants chose to re-migrate on their own (without any further subsidies) in subsequent years.

These positive results point to the same puzzle that forms the core of my research agenda: Why were these poor households facing an annual threat of seasonal famine not migrating to begin with? A model with two key elements can rationalize this behavior: (a) experimenting with a new activity is risky, given uncertain returns to migration, and (b) migration failure can be very costly for individuals close to subsistence. With a downside risk to migration, poor households may fear an unlikely but disastrous outcome where they pay the cost of moving, but return home hungry at a time when their family is already under the threat of famine. This model is related to a view of poverty as vulnerability (Banerjee 2004), where the poor cannot take advantage of opportunities because they are afraid of losses.

We return to our data and find support for this structure: households that are close to subsistence—on whom experimenting imposes the biggest risk—start with lower migration rates, but are the most responsive to our intervention. To test the theory, we design an index insurance contract for migration, where beneficiaries receive a $10 credit for the bus ticket, but where the repayment amount varies depending on weather conditions that affect employment opportunities in the city. This treatment proved to be even more effective in inducing migration.

Results of these tests notwithstanding, it is still puzzling that the households we induced to move were not experimenting with migration in years in which their income realization was high, or that they did not save up to experiment. To explore, the fourth part of this paper calibrates the model allowing for buffer stock savings and shows that some other element, such as savings
constraints (Dupas and Robinson 2013) has to be added to our risk aversion model to offer a fully quantitatively satisfying explanation for the phenomenon we observe.

The paper therefore offers a path forward for this field of research by identifying conditions under which this type of under-investment is most likely to be observed, and elements that could be added to our model to further explore technology adoption decisions of the poor. This paper appears on the Ph.D. reading lists of most major development programs, I have presented it many top universities (MIT, Yale, Harvard, UC-Berkeley, LSE, Stanford…), and it is accumulating citations at a rapid clip. I am optimistic that the path we have charted out will lead to exciting research on migration and on technology adoption in the coming years.

I have investigated other interrelationships between risk and technology adoption in a series of papers with Mark Rosenzweig, based on a field experiment where we designed and marketed rainfall index insurance (against delays in the arrival of monsoon) to 5,000 farmers across three states in India. The first paper [“Selling Formal Insurance to the Informally Insured”] examines whether pre-existing informal risk sharing networks (that the poor traditionally must rely on to cope with adverse shocks) crowd out demand for formal index-based insurance. The paper uncovers a rich and nuanced set of interactions between the formal and informal markets. Theory suggests that informal networks lower the demand for formal index insurance only if the network indemnifies against aggregate risk, but not if its primary role is to insure against farmer-specific losses. When formal insurance carries basis risk (mismatches between payouts and actual losses due to the remote location of the rainfall gauge), informal risk sharing that covers idiosyncratic losses enhance the benefits of index insurance, because the network can help precisely where the imperfect index contract fails. We find empirical support for all these nuanced predictions of the model by randomizing index insurance offers across castes (which are the primary networks for risk sharing), and the locations of rainfall gauges across villages in India. An unusual feature of this research design that permits an investigation of the interactions between pre-existing institutions and insurance marketing is that we were allowed to randomly assign insurance in a set of villages where another organization had collected long-run panel data for forty years. The historical data allowed us to construct a rich empirical characterization of the attributes of informal risk sharing networks, which was essential for a study like this.

Impact and Future Directions

My research agenda on risk has already had important policy impact. The World Bank’s annual flagship publication for 2014 focused on risk and opportunity, and both the migration and insurance research are featured prominently in the chapter on household risk management. I was invited to give an overview lecture to the 2014 World Development Report (WDR) writing team, subsequently wrote a background chapter, and I also recorded a lecture for the World Bank’s Massive Online Open Course (MOOC) designed to disseminate the findings of the report to policymakers around the world.

The program evaluation component of the *Econometrica* paper suggests that the migration subsidy program we designed was effective in mitigating the effects of the seasonal famine in Bangladesh. This program is now being replicated and scaled up in various countries. *Evidence Action*, an organization that “translates research into tangible results, scaling up what works” has raised money for a seasonal migration support program inspired by our paper. They hired two of my Yale students to explore (a) the feasibility of replicating the model in Malawi and Zambia, and (b) the general equilibrium (and any other unintended) consequences of scaling up the program in Bangladesh. The Australian development agency *AusAID* is funding a replication to encourage seasonal migration from the poor eastern islands of Indonesia, and the NGO *Innovations for Poverty Action* is exploring seasonal migration from northern Ghana to Accra.
The model of risk aversion has the potential to shed light on other long-standing technology adoption puzzles, such as the slow diffusion of Green Revolution technologies, or the value of risk-free trial periods for selling durables like stoves. I am exploring these themes in ongoing experiments.

**Leveraging the Power of Social Networks to Promote New Technologies**

The paper on rainfall insurance suggests that networks mediate the demand for new technologies. An influential literature documents the existence of social learning in a variety of settings. My own body of research explores the policy implications of these results: if learning from others is an important conduit for diffusion, can we use social networks as a policy lever to promote welfare-enhancing products in developing countries?

My first attempt, with cook-stoves [Miller and Mobarak, *Marketing Science*] shows that marketing through “opinion leaders” in rural Bangladesh is more effective for a stove type where the benefits are less self-evident, and that the influence of leaders’ adoption decisions dissipates once households have the opportunity to experiment with the stoves themselves. Furthermore, this multi-year experiment shows that consumers rely more on social networks to learn about negative product attributes in institutional environments characterized by low consumer trust about new products.

My second attempt, which disseminated new agricultural technologies through village networks in partnership with the Malawi Ministry of Agriculture [“Social Learning and Communication,” R&R, *Review of Economic Studies*], was much more successful as a policy intervention. We compare dissemination through extension workers against two new methods we devised: (a) teaching “lead farmers” most willing to experiment, and giving them the responsibility to disseminate to others in the village; and (b) asking average-looking “peer farmers” to disseminate in their networks. Lead farmers are typically rich, high-status individuals, but with a downside that their experience is less relevant for the average farmer with less resources at her disposal. Conversely, peer farmers represent a more relevant and proximate information source, but they may lack social stature or credibility. We introduce a cross-cutting treatment where half of all communicators are offered performance-based incentives, with payments conditional on measured gains in knowledge and adoption in their village. We use these experiments to test a model of technology diffusion with communication incentives.

The data show that peer farmers are highly responsive to incentives, and paying them to communicate is a cost-effective way to increase technology adoption and agricultural productivity. The fact that incentives affect the flow of information suggests that diffusion between agents in a network cannot be treated as automatic, in contrast to a central assumption made in the social learning literature. Using social networks to improve technology diffusion policy therefore requires more in-depth analysis of communication.

Given the success of the peer farmer strategy, a follow-up experiment [“Can Network Theory based Targeting Increase Technology Adoption?”] takes a more sophisticated theory-driven approach to identify “optimal communicators” who would maximize diffusion in the network. We first collect census data to map all social network relationships in 200 villages in Malawi, and conduct simulations on these data to identify the theoretically optimal communicators under two specific models of diffusion: simple contagion or complex contagion theory. Our field experiment compares these theory-driven network targeting approaches to simpler, scalable strategies that rely on either extension agents or geographic location to select network partners. We trace adoption patterns in all villages for the next three seasons, and find that theory-driven targeting leads to greater diffusion than the simpler approaches. The individual-level adoption data reveals that
diffusion patterns are most consistent with the theory of complex contagion, where a farmer needs to know multiple people using the technology before she is convinced to adopt herself.

Networks are also likely important for sanitation behavior, because toilet use generates large positive externalities by reducing the risk of disease transmission. Sanitation investments may be strategic complements: returns to investment may be very low if neighbors do not invest simultaneously, as household members would fall sick anyway if the surrounding environment is unclean.

We document the existence of a strategic complementarity using a large-scale experiment in Bangladesh [Strategies to Induce Investment in Sanitation…], where we randomly varied the proportion of households in each community who are subsidized to invest in toilets. Holding own price constant, we see that each household is much more likely to invest in a toilet if a larger number of neighbors simultaneously receive discounts. Our follow-up work explores the precise mechanisms underlying the social multiplier effect we document: learning, or changes in social norms, or disease transmission pathways.

I have also invoked social influence in order to induce firms in Bangladesh to pay taxes, in a large-scale experiment on 20,000+ VAT paying firms. When enforcement is not feasible, providing social recognition to firms (which may be cheap for the tax authority, but valuable to the recipients) may be a cost-effective way to induce firms to register and pay taxes. The experiment shows that in markets where there is a pre-existing norm of paying (i.e. at least some members have paid in the past), non-compliant firms become more likely to pay if the tax authorities threaten to reveal the identities of non-payers to neighbors. The results are consistent with model where social pressure is most effective when a firm is deviating from the local norm.

Impact and Future Directions

The Department of Agriculture in Nepal has replicated the peer farmer program upon learning about the Malawi experiments through the Poverty Action Lab (JPAL). I added a treatment arm to this replication where we provide flat payments to the communicators (as opposed to performance-based incentives) to see if the flat rewards, which do not require costly monitoring of performance, can realize the adoption gains that we observed in Malawi with incentive payments. The simpler payment structure would be a much more scalable and feasible diffusion strategy for developing country governments.

The sanitation intervention which combined joint commitment with discounts was successful in reducing the rate of open defecation from 40% to 21%. The Gates Foundation convened an event where I presented these results to their largest institutional grantees in the sanitation sector, including the World Bank, UNICEF, and large NGOs such as Wateraid, BRAC and Plan International. Many in this sector hold strong beliefs that subsidies cause dependency and undermine markets, but our experimental results have moved the sector’s priors about the value of subsidies targeted to the poor in a coordinated way. Our approach is now being replicated and scaled up in India, and the philanthropic-minded CEOs of two of the largest hedge funds in London have committed to invest in these efforts. These developments were serendipitous, in that the newly elected Prime Minister of India has made eliminating open defecation his signature issue both in his pre-election pledge and in his inaugural speech, exactly around the time when we began sharing the results from Bangladesh.

The State Minister for Finance in Bangladesh recently discussed the taxation project at a public event, and pledged to employ innovative and non-standard ideas like social recognition to address the low rates of tax compliance. The Chairman of the National Board of Revenue declared at the same event that he has formed a committee to explore the implications of our experimental results, and to consider how social recognition can be institutionalized across all forms of revenue collection.
Gender Aspects of Technology Adoption Decisions

Technology adoption decisions are sometimes made by individuals rather than unitary households, and this may explain low adoption puzzles for technologies whose benefits are gender specific. “Intra-household Externalities and..., R&R, *Economic Journal*” shows that women are willing to adopt a healthier technology, but they lack the authority to make financial decisions. “*Gender Differences in the Effects of Vocational Training*” shows that young women face higher opportunity costs of participating in a training program designed to build labor market skills. They are less regular and they drop out more frequently. Consequently the trainers invest less in female trainees, which ultimately results in worse training experience and outcomes for women. Finally, “The Limits of Engineering Solutions...” shows that households in rural Uganda are generally unwilling to pay even 5-10% of the market price of a rainwater storage device (RSD) that reduces the burden of water collection for women and children. When households use freely provided RSDs, the take-up only displaces the water collection trips that adult men and women were making, and not the trips made by children. The time women save gets appropriated by non-water related household chores. A technically effective solution will not have its intended effects without a deeper analysis of intra-household optimization. The household’s low perceived value of time for women and children may drive both their initial location choices and their subsequent (ineffective) reactions to interventions.

General Equilibrium Impacts of New Technologies

I now design and conduct field experiments at a large enough scale such that the programs sometimes induce general equilibrium changes, altering wages and prices, or inducing reactions from local politicians. Tracking these changes is important, because comprehensive evaluation of any development program requires an accounting of general-equilibrium effects, especially if we are interested in assessing potential effects when the program is scaled up.

“Does Development Aid Undermine Political Accountability?” tracks both leader and constituent responses to a large-scale sanitation program we implemented in rural Bangladesh in a sample of 76,000 individuals. Leaders responded to the program by spending more time in the (randomly chosen) villages that received subsidies. However, program beneficiaries are not easily fooled by their leaders, and do not incorrectly attribute credit to their leaders for a program for which they were not responsible. These results suggest that concerns about foreign aid undermining political accountability, as expressed by Bill Easterly and Dambisa Moyo in popular books, may be overstated.

In “Risk, Insurance and Wages in General Equilibrium”, we examine general-equilibrium labor market effects of marketing rainfall insurance to both landless and cultivating households in rural India. We first show that insured farmers take more risk in their cultivation decisions. This increases the risk exposure of landless agricultural wage workers hired by those cultivators. Theory suggests, and our data confirm, that labor demand becomes more rainfall sensitive, and wage volatility increases because the high-risk high-return technologies pay off more during good rainfall states, but are more likely to fail during droughts. However, selling insurance to landless laborers allows them to mitigate this risk. Insured laborers who receive payouts during droughts are less desperate to supply labor when wages are low, and their labor supply choices therefore indirectly insure other uninsured laborers through wage effects.
An unfortunate feature of the regulatory environment in India makes this research particularly policy relevant. Index insurance is regulated like conventional indemnity products, which precludes landless laborers (who do not have a tangible insurable interest, such as land) from purchasing rainfall insurance. We were able to circumvent this regulatory barrier for research purposes, and show that the demand for rainfall insurance among the landless is just as strong as it is amongst cultivators. Simulations based on our estimates show that insuring cultivators without offering the product to the landless makes the landless (the poorest segment of the population who face the most difficulty managing risk) worse off compared to a situation where insurance does not exist at all. The paper thus provides a straightforward policy lesson for regulators in India and in other agrarian economies: extend agricultural insurance to landless wage workers.

Other Research with Policy Impact

A non-trivial (but decreasing) share of my research output to date is unrelated to technology adoption questions. Some other notable papers that have had impact on research and policy are:

(a) “Decentralization and Pollution Spillovers,” R&R Review of Economic Studies, uses a series of natural experiments created by changing county boundaries in Brazil to study cross-border pollution externalities.

(b) “Manufacturing Growth and the Lives of Bangladeshi Women,” R&R at JDE, shows that access to garment sector jobs led rural Bangladeshi parents to keep young daughters in school longer (since factory jobs reward literacy and numeracy), and these girls subsequently delayed marriage and childbirth. Given the recent well-publicized factory disasters and attendant pressures to restrict exports from Bangladesh, it was important for this paper to carefully document the full range of welfare effects of factory jobs. The paper shows that export restrictions have the potential to hurt the same workers that they are designed to protect. The results have been covered by the New York Times, BBC, CBC, NPR, and many other outlets in Asia and Europe. The study was directly cited in a New York Times editorial.

(c) “Skilled Immigration and Innovation,” Economic Journal 2012, documents the contribution of foreign Ph.D. students in science and engineering on innovation produced by U.S. academic departments. The study was heavily cited in the media during the last round of immigration policy debates. I was invited to write an op-ed in the New York Times about high-skilled immigration.

Teaching

My research on technology adoption has direct implications for marketing strategies in emerging markets. I have designed a graduate elective course at Yale on marketing new products in developing countries based on this frontier literature. I also teach an elective course on the unique challenges and business opportunities offered by emerging markets. This course draws heavily on my earlier macro-development style research\(^1\) on institutions, political economy, growth and corruption, as well as on challenges that I have faced in implementing and scaling up programs in the field. The course now operates at maximum capacity for our classrooms, with about 75 students enrolled last year. Finally, I take 25-30 SOM students on a 2-week international experience trip every year to a country where I have ongoing field research projects. While the broad theme of every trip

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\(^1\) I have not talked about this research in this statement because I shifted towards field experiment based research on micro-development when I moved to Yale in 2007, but this earlier work features heavily in the MBA and Executive Education courses I designed at Yale SOM.
is to understand development challenges and prospects for the country we visit, we usually delve deeper into the sector that is the subject of my field project. My research therefore feeds directly into the teaching mission of Yale’s graduate programs in business and international affairs. I have also been very active in advising Ph.D. students, with one or two students on the market every year. I received a campus-wide “Most Outstanding Faculty Advisor Award” at the University of Colorado at Boulder. In the MBA program, I regularly advise students in their Global Social Entrepreneurship (GSE) projects, and guest-lecture in the GSE program every year.

Service to the Profession

I have expended some energy and effort over the past five years to ensure that my research, as well as related research by colleagues, has its intended policy impact. One example is that I (along with S. Galiani and E. Duflo) raised over $5 million for the Urban Services Initiative (USI) at the Jameel Poverty Action Lab at MIT, to develop a research program focused on the needs of the urban poor. USI events have induced about 25-30 top-level researchers from the U.S. and Europe to travel to South Asia and Africa to collaborate with implementing organizations and local researchers based in those regions. To date, we have provided over a million dollars in funding for new research projects. I have mentored an Ethiopian and a Bangladeshi national as post-docs to build in-region capacity to conduct field-experiment based modern development economics research.

Another example is that I helped to design and build the Bangladesh research program for the DFID-funded International Growth Centre based at LSE. The program has funded 15-20 research projects by both international and local researchers, created joint research opportunities for both groups, and brought in 20-30 top economists to Dhaka and London to meet with policymakers and start new projects. These interactions with policymakers have laid the platform for our research to have immediate policy impact in Bangladesh. Another metric of success is that the program induced non-development economists like Raj Chetty to begin working in Bangladesh, and innovate in partnership with the government.

Finally, in order to facilitate the field research funded by these programs, I set up the Bangladesh field office for Innovations for Poverty Action (IPA), a research and evaluation -focused NGO based in New Haven. About a dozen international researchers have thus far used IPA facilities to conduct large-scale field experiments in rural and urban Bangladesh.

In addition, I have engaged in the usual service activities for Yale and for the profession, reviewing internal and external grant applications, refereeing for over 30 journals, sitting on national or international committees related to my research expertise (e.g. the Global Alliance for Clean Cookstoves), editing one of the leading field journals in development economics (Economic Development and Cultural Change), and writing background chapters for the United Nations Human Development Report and the World Bank World Development Report.

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