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## Emerging economists

### International bright young things

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**The next generation of economists do their best work somewhere between the field clinic and the dissection room**

Illustration by Otto Dettmer



TWENTY years ago *The Economist* wrote about eight young economists who were making a big splash in their discipline and beyond. One of them, Paul Krugman, recently won the Nobel prize for his models of international trade and economic geography. Ten years later we tried to repeat the trick, identifying another eight young stars, many of whom were taking their discipline far off-piste. One has since achieved even greater fame than anticipated. Steven Levitt of the University of Chicago became a household name as co-author of "Freakonomics", a bestselling book published in 2005.

"Freakonomics" owed its origins to a profile of Mr Levitt in the *New York Times* magazine in 2003. Its success has won a new readership for economists, beyond the business section and the opinion columns, in the glossier pages of the weekend supplements. The best young economists, as a consequence, have already attracted plenty of attention. That leaves us in a bit of a quandary. We feel like lonely prospectors, who, returning to a favourite stream, find it overtaken by a gold rush.

Undeterred, we have given the prospecting pan another shake. We asked leading authorities in the discipline to name the best young economists in the world. Between them, they proposed over 50 researchers, but several names recurred on many lists. We have sifted the 50 down to eight, all of whom received their PhDs in the past ten years.

## The family tree

Several of the scholars in this year's batch trace their intellectual ancestry back to those we picked ten years ago. For example, **Jesse Shapiro** of the University of Chicago and **Roland Fryer** of Harvard are recognisably the intellectual heirs of Mr Levitt. They share the same knack for finding ingenious ways to answer unlikely questions, often by plundering forgotten troves of data.

At just 29, Mr Shapiro can already boast a collection of eye-catching findings worthy of a sequel to "Freakonomics". He has shown that some judgments are best made without too much information: people are better at predicting the winner of American gubernatorial elections when they watch the candidates with the sound turned off. Harsher jail conditions do nothing to deter prisoners from reoffending. If anything they encourage recidivism. Preschoolers who watch television do better academically than children who don't, especially if their parents have little education or poor English.

Mr Fryer's ambition is to unravel the causes of black underachievement in America, especially in education. His search for explanations extends beyond racism and poverty to contemplate the role of a self-defeating culture. He calculates that a black student who earns straight A grades will have 1.5 fewer friends from his ethnic group than an equally swotty white student.

Michael Kremer, another of those we cited ten years ago, can also claim an intellectual relative in this year's cohort. **Esther Duflo** of the Massachusetts Institute of Technology (MIT) received more recommendations than any other economist. Some who didn't nominate her thought she was too established to count as "new".

With her colleague, Abhijit Banerjee, Ms Duflo and Mr Kremer have remade development economics, nudging it away from its concern with policies, towards a preoccupation with projects. They study economic development as seen from the field, clinic or school, rather than the finance ministry. They might be called the "peace corps" of economists, bringing the blessing of their investigative technique to the neglected villages of India or the denuded farms of western Kenya.

Ms Duflo has made her name carrying out randomised trials of development projects, such as fertiliser subsidies and school recruitment. In these trials, people are randomly assigned to a "treatment" group, which benefits from the project, and a "control" group, which does not. By comparing the average outcome of each group, she can establish whether the project worked and precisely how well.

In one study, Ms Duflo and her colleagues showed that mothers in the Indian state of Rajasthan are three times as likely to have their children vaccinated if they are rewarded with a kilogram of *daal* (lentils) at the immunisation camp. The result is useful to aid workers, but puzzling to economists: why should such a modest incentive (worth less than 50 cents) make such a big difference? Immunisation can save a child's life; a bag of lentils should not sway the mother's decision either way.

Randomised trials "give you the chance to be surprised", Ms Duflo says. Had they arrived at this result using some other method, she and her colleagues would have assumed they had made a mistake. But randomisation removes such doubts, showing that it was indeed the lentils that made the difference. The result cannot be dismissed; it must be explained.

The approach has its critics. A randomised trial can prove that a remedy works, without necessarily showing why. It may not do much to illuminate the mechanism between the lever the experimenters pull and the results they measure. This makes it harder to predict how other people would respond to the remedy or how the same people would respond to an alternative. And even if the trial works on average, that does not mean it will work for any particular individual.

The randomistas, as Ms Duflo and her comrades are called, liken their studies to the clinical trials that prove the efficacy of new drugs. But the ultimate ambition of economics is for something more akin to anatomy. Researchers hope to dissect the underlying physiology of an economic problem, revealing how the leg bone is connected to the thigh bone. With a full anatomy of behaviour—what economists call a structural model—they can determine if a policy or project will work even before it has been attempted.

The early anatomists of the human body suffered from a shortage of fresh cadavers to work on. Medical students would trek long distances to watch a dissection performed. Economists often find themselves in a similar predicament. Short of good empirical meat, they have to rely on elaborate theory and guesswork to fill in what they cannot observe.

Illustration by Otto Dettmer



**Amy Finkelstein**, also of MIT, the fourth of our young stars, has anatomised the market for annuities in Britain. The industry suffers from “asymmetric information”: customers may know more than the provider about their chances of dying. Unfortunately, this private information is as hidden from economists as it is from the annuity company. Ms Finkelstein and a colleague, James Poterba, have shown how to infer the cost of this unseen problem from what can be observed, namely the kind of annuities people choose and the length of their life after retirement.

Like Ms Finkelstein, **Raj Chetty**, recently hired by Harvard from the University of California, Berkeley, is a promising young “public economist”: a student of tax and spend. He has great respect for structural models. But in a recent paper he makes the case for judicious short cuts. Often you don’t need to dissect a whole body; a few choice incisions are enough.

For example, he wanted to know whether policymakers should raise unemployment benefits. To answer this question, a structural model would need to specify how much a dollar is worth to a person on the dole, as compared with someone in work. It would also need to quantify the burden a job hunt imposes. This isn’t easy to find out. But Mr Chetty argues it is unnecessary.

He gleans all the information he needs by looking at the time it takes unemployed people to find a new job. Unsurprisingly, they take longer when their benefits are more generous. This is usually attributed to “moral hazard”—people take less care to escape a danger, such as joblessness, if they are insured against it. But Mr Chetty shows that skewed incentives account for only 40% of the delay.

The rest is due to what he calls a “liquidity effect”. The unemployed typically have few liquid assets to fall back on and little chance of a loan from the bank. This forces them to rush their job search. If they had savings to dip into or credit to tap, they might search with greater deliberation. This kind of dallying is, in a sense, optimal. The unemployed decide that an unhurried job search is worth the extra cost of depleted savings or heavier loan repayments.

Higher benefits ease this liquidity problem. Raising benefits by \$1 a week would do as much social good as raising American GDP by \$290m, Mr Chetty calculates, although government loans to the unemployed might do better still.

Twenty years ago macroeconomists dominated our list of the best young thinkers, but they are under-represented in this year’s batch. We found plenty of agreement about the three or four young macro thinkers most likely to succeed, but surprisingly little confidence that they would. One leader in the field suspected their work represented a moment of beauty, not truth. Another complained that the youngsters lacked the “vision thing” that distinguished the greats of the past.

## Ramsey revisited

If so, perhaps they can blame the times that produced them. They came of age during the Great Moderation, a period of macroeconomic tranquillity and intellectual consensus. They are in thrall not to John Maynard Keynes, sage of the Depression, but to his Cambridge contemporary, Frank Ramsey, a precocious polymath who made his contributions in the prelapsarian 1920s. Ramsey was interested in how much of its income a nation should save so as to maximise its prosperity now and in the future. His work underpins much of modern

macroeconomics, in which agents act today with an eye on tomorrow. But the framework is best suited to analysing steady accumulation, not violent cycles of speculation and liquidation. So it is not the obvious place to start to explain the world economy's present predicament.

The macroeconomist nominated most often for our list was **Iván Werning** of MIT. Mr Werning is an economist's economist; an elegant theorist, whose early contributions provided streamlined proofs that other thinkers could make use of. One of Mr Werning's ambitions is to unite Ramsey's work with that of another elegant theorist, Sir James Mirrlees. Sir James won the Nobel prize in 1996 for exploring how best to set taxes when people can disguise their true worth from the revenue collector. Mr Werning asks the same question, but in the forward-looking, macroeconomic setting provided by Ramsey.

Mr Werning and his co-authors have so far derived at least two theoretical results of note. The first is to show that the unemployed have sufficient incentive to find work, even if they receive unemployment benefits indefinitely. The second is that bequests from one generation to the next should be subsidised by the government, with smaller inheritances receiving higher rates of subsidy. Mr Werning and his co-author, Emmanuel Farhi (a young Harvard macroeconomist), point out that the biggest roll of the dice in life is the family you are born into. Their system of subsidies would take the edge off this uncertainty.

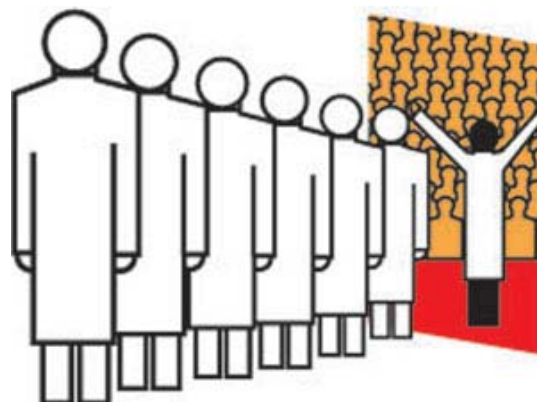
Two of the economists we highlighted ten years ago—David Laibson of Harvard and Matthew Rabin of Berkeley—were exponents of “behavioural economics”, incorporating the insights of psychology into the dismal science. The sub-discipline has continued to flourish in the decade since, seeping so far into the mainstream that its disciples no longer constitute a self-contained school. The randomistas, for example, often invoke behavioural explanations for their experimental results.

**Xavier Gabaix** of New York University, our seventh pick, is another example of someone who is au fait with behavioural economics but not defined by it. He has written papers with Mr Laibson, including one that explains why hotels can get away with overpricing the mini-bar. But his interests extend beyond the behavioural.

He has, for example, shown a fascination with “power laws”: tantalising statistical patterns that seem to crop up wherever you look hard enough. The size of cities, the pay of executives and the performance of the stockmarket all seem to follow such laws. For cities, the law can be crudely expressed as the “rank-size rule”. The second-biggest city will have roughly half the population of the biggest; the population of the third-ranked city will be one-third of the first's, and so on. The relationship between executive pay and company size also obeys a power law: companies twice the size tend to pay their chief executives roughly 25% more.

These curious regularities have more than numerological appeal. They give clues about what can and cannot explain the size and growth of the things they describe. For example, the rank-size rule could not hold if small cities grew systematically faster than big ones, or vice versa. The power law of executive pay also requires a particular kind of economics to explain it. Mr Gabaix thinks the “economics of superstars”, invented by Sherwin Rosen, fits the bill.

Illustration by Otto Dettmer



Top executives may differ only slightly in their talents, just as sports champions differ only slightly from runners-up. But the better managers nonetheless get hired by the bigger firms, just as the best entertainers sing to the largest audience. This means an executive's small edge in managerial skill is amplified, because his

talents go to work on a bigger canvas. Mr Gabaix made a splash in 2006 when he concluded that the “excessive” pay of chief executives was not necessarily excessive. Compensation may have grown sixfold from 1980 to 2003 not because managers were six times greedier, but because the firms they ran were six times bigger.

If the size of firms obeys a power law, economies will comprise some very big firms and a long tail of small ones. The fortunes of the biggest companies might then stir the whole economy, Mr Gabaix conjectures. The \$24 billion dividend paid by Microsoft in December 2004, for example, added 3% to America’s personal income that month. Mr Gabaix calls for a more “granular” approach to macroeconomics, which would weigh the contribution of big firms to national aggregates.

This granular view is already taking hold in studies of international trade. Countries, after all, do not trade with each other; companies do. A few firms usually account for the lion’s share of a country’s exports: in America, the top 10% of exporters account for 96% of the country’s foreign sales, and only 4% of firms export at all.

These observations (drawn from work by Andrew Bernard of Dartmouth College among others) demand a theory to explain them. That gap has been filled by **Marc Melitz**, a trade economist at Princeton University and our final new star.

Mr Melitz is a pioneer of the “new, new trade theory”, which succeeds the “new” trade theory propounded by Mr Krugman almost 30 years ago. The source of its novelty is its recognition that firms differ, and only the best firms export. In America, for example, exporting factories are more than twice as big as plants that do not sell beyond their shores, and they squeeze 14% more out of their workers.

In Mr Melitz’s theory firms first prove themselves at home, discovering their own limits and abilities. Only the best then venture overseas. Entering a foreign market is an expensive endeavour, he points out, even before firms encounter the tariffs or transport costs that preoccupy most trade models. An exporter must find and introduce itself to distant customers, comply with alien regulations and set up distribution channels abroad. One study found that it cost Colombian chemical factories over \$1m to enter a foreign market.

The gains from trade also differ in Mr Melitz’s model. In the new trade theory that preceded it, international commerce raises the productivity of firms by enlarging their market, allowing them to reap economies of scale. In Mr Melitz’s model, trade raises the productivity of industries, not by allowing firms to grow bigger, but by giving the better firms a bigger share of the market. Foreign competition sifts and sorts firms, winnowing out the weakest firms and leaving a greater share of the market to their stronger rivals.

Just as Mr Krugman found a clean way to account for economies of scale, Mr Melitz handles the heterogeneity of firms without spoiling the lines of his model. It now serves as a pliant workhorse for lots of “granular” thinking in the field.

## **Bodice rippers**

Over 60 years ago Paul Samuelson laid down “the foundations of economic analysis” in his seminal work of that name. In the introduction, he describes his dawning realisation of the underlying unity of the subject. As he laboured in each field—consumer behaviour, public finance, international trade, business cycles—he encountered similar problems, which yielded to the same set of mathematical techniques. Mr Samuelson’s book squeezed a shapeless body of economic knowledge into a tight corset.

In the decades since, the laces have been unpicked. It is not just that economists are nosing into new fields of social behaviour. They have been doing that at least since Gary Becker of the University of Chicago wrote about crime and the family in the 1960s and 1970s. But today’s economists show no great attachment to the rational model of behaviour that guided Mr Becker. Economic theory has become so eclectic that ingenious researchers can usually cook up a plausible model to explain whatever empirical results they find interesting. Economics is now defined neither by its subject matter nor by its method.

What, then, unites these eight young stars and the discipline they may come to dominate? Economists still share a taste for the Greek alphabet: they like to provide formal, algebraic accounts of the behaviour they explain. And they pride themselves on the sophistication of their investigative methods. They are usually better at teasing confessions out of data than their rivals in other social sciences. What defines economics?

Economics is what economists do—the best of them, anyway.

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