

'New Development Economics' and the Challenge to Theory

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New Development Economics?

If we take the list of papers cited by BBM (Bardhan, Basu and Mookherjee, all in this issue) as representative of what they call "New Development Economics", what is striking is how old-fashioned it all sounds. The topics, for the most part, are the familiar ones: education, health, credit, technology, land, migration. Even institutions, which sound a bit more novel, were being discussed and debated by North and his colleagues more than 20 years ago. It is true that agriculture is a bit less in fashion now than it was 25 years ago, and there is, to my taste, a bit too much about human capital, but relative to many other fields (think of macroeconomics) the picture is one of remarkable stability.

In terms of empirical methodology, while there is heightened emphasis on running well-identified regressions, the basic concern with distinguishing causation from mere correlation, that it reflects, obviously goes back a long way. The seemingly unending debate among development economists of the 1960s and 1970s about whether bigger farms were less productive (the so-called size-productivity debate), was ultimately a debate about causation – about whether it was really size that was doing the damage or something associated with size. What has changed since then is that development economists have now embraced all the strategies that have been developed in the last two decades, mostly by labour economists, for dealing with identification issues – difference-in-difference, regression discontinuity designs, randomised experiments, etc – and have access to the kind of data that is needed to make use of these techniques.

In other words, these papers reflect, more than anything else, the mainstreaming of empirical work in development economics. The field, as Mookherjee explicitly recognises, has matured.

In Defence of Empirical Method

BBM share a feeling that in the process of maturing things have gone a bit awry. Their main complaint, though none of them quite puts it this way, is that empirical standards are now unrealistically high, with the consequence that the top journals are now filled with well-identified but uninteresting papers.

To some extent this has to be right. To accept something that obviously does not meet the standards of the profession because the subject is interesting, editors have to exercise judgment, and exercising judgment is never easy, especially once you recognise that this will expose you to the charge of favouring certain people and/or ideas. There are certainly interesting papers that shed important light onto questions we know little about that do not get published because they do not quite make it on some methodological imperative.

Would I therefore rather that we had not come to a point where development economists are at the cutting edge of empirical techniques? Absolutely not! I think the fact that there is no special pleading for empirical work using developing country data is the reason why so many of the brightest and the best among economics graduate students are coming into the field now. When I was a graduate student, everyone always said that development economics was very important but no one actually did it: The problem was that if you were a top student you wanted to be where people appreciate your mastery of everything that was clever and new (I, for example, did theory, which was very much in fashion then). Now it is development economics that is in fashion.

As far as I am concerned this alone is enough compensation for the sacrifices that we have to make to the harsh god of identification. But I am also convinced that the danger of unwarranted rejections is nowhere as serious as the danger of publishing spurious results. As I have argued elsewhere, there is no dearth of

ideas in the world of development practitioners today. What is missing is discrimination: There is no rigorous process by which bad ideas get dropped and good ideas are identified and made better.

A World Bank publication from few years ago¹ provides an excellent example of the attitude towards evidence in the development community outside academia. The *Sourcebook* is meant to be a catalogue of what, according to the bank, are the right strategies for poverty reduction. These are also, we presume, strategies into which the bank is prepared to put its money. It provides a very long list of recommended projects, which include: computer kiosks in villages; cell phones for rent in rural areas; scholarships targeted toward girls who go to secondary school; schooling voucher programmes for poor children; joint forest management programmes; water users' groups; citizen report cards for public services; participatory poverty assessments; internet access for tiny firms; land titling; legal reform; micro-credit based on group lending; and many, many, others.

While many of these are surely good ideas, the book does not tell us how we know that they work. Indeed, one memorable example makes clear that this is not a primary concern of the authors. The book describes a programme called the *Gyandoot* programme which is based in Madhya Pradesh in India and provides computer kiosks in rural areas. The *Sourcebook* acknowledges that this project was hit hard by lack of electricity and poor connectivity and that "currently only a few of the kiosks have proved to be commercially viable". It then goes on to say, entirely without irony, "following the success of the initiative..." (p 80).

The most useful thing a development economist can do in this environment is to stand up for hard evidence and to be demanding about the kind of empirical work that gets into the better journals. Regressions published in the top places have a way of filtering into policy conversations – the power of statistics combined with the imprimatur of a top journal can be irresistible. A recent example is the work by Burnside and Dollar on the impact of aid on growth: In a paper published in the *American Economic Review* (2000),² they showed results suggesting that aid does help growth but only when it is given to countries that pursue the "right" policies. This is, of course, also the idea behind president Bush's Millennium Challenge Account (MCA), and while I do not have any direct evidence of a causal connection

between the two, the Burnside-Dollar work is frequently cited as a part of case for the MCA (including by the United States Congress Research Services). Yet there are multiple reasons why we (and especially the editors of the *AER*) should have been more skeptical of the Burnside and Dollar results. First, because policies are not exogenous: They reflect other things that are right about the country. Second, aid is not exogenous either – countries with bad policies that received a lot of aid may have other problems (for example, dictatorships that are aligned with the US tended to get a lot of aid in the old days), which make it hard to compare them with other countries. In any case, a later paper by Easterly, Levine and Roodman (2003) showed that the result in the Burnside and Dollar paper disappears when they update the data.³ But the MCA is here to stay.⁴

But how do we know that well-identified regressions are any better? For one, as Basu points out, there is the very real risk of reporting bias: To get published it helps to have the kind of results that the editors or referees want. Basu is worried that this favours papers with unexpected results, but it could also favour results of a particular political bent.

Ill-identified Regressions

Note, however, that this is potentially just as true of ill-identified regressions as it is of well-identified ones. Indeed, the fact that papers these days are getting published because they have a clever identification strategy, something that Mookherjee and Bardhan deplore, has the advantage of reducing the emphasis on results. The main contribution of a number of the papers that BBM cite as examples of new development economics (Acemoglu, Johnson and Robinson (2001);⁵ Munshi (2003);⁶ etc) was to provide credible evidence for what many others before them have claimed (“institutions matter”, “social networks are important”, etc).

That being said, I think there is a widely shared feeling that we need to do more about this problem. In particular, I support an initiative to set up a website where every randomised evaluation has to be registered before it gets launched. At the time of registration, the principals of the study will be required to list the outcomes of interest and their predictions regarding each of them (just the direction, not the magnitudes), as well as a reasonable study completion date. They will then be required

to report whatever results they have, on or before the announced date. This would guarantee that all studies (including the ones that fail and the ones that have boring results) get recorded, that the authors cannot decide *ex post* to emphasise interesting outcomes that they never thought of when they started the work, and that the experiment does not continue until it yields the desired result. Doing something like this for non-experimental work would be harder: While experiments tend to be very visible, there is no way to know all the failed regressions that get run – unless the authors want to report them. One thing that might help is to set up a web-space where people can report their failed regressions (just a paragraph . . . we ran this regression in this data set and found the wrong answer or no answer), and then editors can require all future published authors on the same subject to cite these failed studies. In the end, however, this remains another point in favour of randomised experiments over more traditional empirical exercises.

On the other hand, it is true, as Bardhan points out, that randomised evaluations typically give us only the short-term impact of a particular intervention. However, the main reason for this is that the authors of the study want to be able to report some results without having to wait forever, though in many there is nothing, in principle, to stop them from continuing to follow the treatment and control groups after the first round results have been reported. For example, consider Miguel-Kremer’s work on deworming cited by Basu.⁷ In that study, they randomly assigned a programme of giving free deworming medicine to children across a set of schools in Busia District in Western Kenya and found that the children in the schools where the medicine was given came to school much more regularly. While that paper is published, the study continues: The goal is to discover whether the fact that they came to school more often helps these children in their future life. To do so they will need to follow the children in both the treatment and control groups as they grow older. This is possible because even though the control schools may now be treated, the children who graduated from those schools before the medicine was introduced there continue to be a valid control group for the corresponding cohorts in the treatment schools.

The point is that there is no necessary conflict between the goals of extending the

treatment to the control group (assuming it works) and studying the long-term impact of the treatment. There are cases where this will not work – these are programmes where literally every group will end up treated once the programme is expanded – but these are more the exception than the rule. The bigger constraint, it seems to me, is the sheer effort that goes into following people into the future, but if the stakes are high enough, it will be done.

Among the various concerns raised by BBM, the two most important are what I would call the problem of scope and the problem of size. The problem of scope comes from the fact, emphasised by Bardhan and Mookherjee, that most well-identified empirical exercises tend to be relatively localised: This is what makes it possible to rule out other confounding factors (the extreme case of this is a randomised experiment where we deliberately restrict the domain in order to have full control over the programme placement). This, they suggest, compromises the external validity of these results – how can we draw general lessons from something so limited?

Basu takes this argument a step further. The external validity of a piece of empirical research, however carefully done, he argues, cannot be entirely derived from something internal to that work. To apply these results, we need to have a theory that helps us decide whether a particular location (in space, or Basu emphasises, in time) is sufficiently similar to the location from where the results came. And in most cases this theory would not come close to meeting the standards of evidence that development economics today aspires to, and even if it did, the problem of external validity would remain, just shifted back a layer.

This is obviously correct. Indeed it seems to me to be a version of David Hume’s justly famous demonstration of the lack of a rational basis for induction. But at this point Basu makes a curious leap into what reads like radical scepticism. As he puts it, “Two correctly done empirical results may have the property that one resonates with our intuition – we simply feel that if it was true in the past it has a reasonable chance of being true in the future – and the other does not. My inclination would be to go along with the intuition (while admitting that intuitions often go haywire).” This seems to imply (by continuity) that if one empirical conclusion accorded less well with his intuition than another, but was somewhat more “correctly done”,

would he still go with the latter? In other words, he denies the primacy of empirical methods over his intuition.

I must confess that I do not understand this position. To take an example, one of the early randomised experiments in Busia district of Kenya looked at the effect of having flipcharts in the classroom on the performance of children.⁸ It found that the children in the schools that were randomly chosen to get a flipchart do not do any better. The study then looks at what it would have found if it had tried to answer the same question based on running a retrospective cross section regression using data from other schools in the same part of Kenya. In other words, they ran a simple regression of the test scores on whether the school had flipcharts in this other sample of schools. They found that having flipchart raises student performance about 0.2 standard deviations.

My inclination, faced with this evidence, is to assume that the experiment gave more or less the right answer for what would happen in an average school. I start from the fact that the experiment was correctly done: The experimental result is therefore the right conclusion for those particular schools. The question then is whether this also the correct result for other schools of this type in the area, including the ones in the retrospective study. To believe this, I would need to believe: (a) that the cross section study is biased; and, (b) that the treatment effect does not vary across the two populations.

Say that Basu, on the other hand, believes that the retrospective result is the correct generalisable prediction because it fits better with his prior intuition (this is entirely hypothetical and almost surely unfair to him, but it helps to set out the structure of our disagreement). He therefore believes that: (a) the retrospective result is unbiased; and, (b) that the treatment effect is not the same in these two populations.

These two sets of beliefs (his and mine) are, however, not symmetrical. The fact that the treatment effect varies in this way is not something that he has any independent reason to believe: The study which (I assume) summarises everything either of us knows about the area, suggests that the populations are in fact quite similar. Or to put it differently, the only reason to suppose that the treatment effect is higher in one of these populations than the other is that this is what squares with his prior – this was not something that he learnt

from the data generated by the retrospective study or any other study.

Standard Observation

My suspicion of the results from the retrospective study, on the other hand, stems from the standard observation, confirmed by any number of studies, that schools in developing countries that have more school inputs tend to be schools where parents are richer and/or care more about education and hence are schools where the children would have done better even without the extra inputs. I worry that the fact that children in schools with flipcharts do better than children in schools without flipcharts could be partly a result of this. In other words, I favour the experimental result mainly because I have an independent reason to suspect that the retrospective result is biased upwards.

Moreover, my assumption that the treatment effect is the same in the two populations, while not justified by any direct evidence, would seem to be the natural default assumption in the absence of any information to the contrary. It is clearly the way we are designed to function in the world – when we meet a new child our first instinct is to treat him like all the other children we know – though I would not know whether it is evolution that has made us so (as Basu would have it), or our own experience with what is sometimes called the “uniformity of nature”.

None of this, however, tells us exactly what to do when we expect there to be systematic differences between the two populations. If our only really reliable evidence was from India but we were interested in what might happen in Kenya, it probably does make sense to look at the available (low quality) evidence from East Africa. Moreover, if the two types of evidence disagree, we might even decide to put a substantial amount of weight on the less reliable evidence, if it turns out that it fits better with our prior beliefs. Nevertheless, there remains an essential asymmetry between the two: The well-identified regression does give us the “correct” estimate for at least one population, while the other may not be right for anyone. For this reason, even if we have many low quality regressions that say the same thing, there is no sense in which the high quality evidence becomes irrelevant – after all, the same source of bias could be afflicting all the low quality results. The evidence

remains anchored by that one high quality result.

That being said, the only way to build trust in experimental and quasi-experimental results is to replicate them in several different locations. One of the strengths of the Miguel-Kremer work on deworming cited by Basu,⁹ is that it has now been reproduced in India (the original study was in Kenya) with very similar results.¹⁰ Moreover, Bleakley (2004),¹¹ using quasi-experimental methods to estimate the effect of deworming in the US. South also finds a structural estimate very similar to what these two other studies find. The randomised evaluation of the Balsakhi remedial education programme in India was carried out simultaneously in two different locations (Mumbai and Vadodara) by two different implementing teams for exactly this reason, and, reassuringly, found more or less the same results in two locations.¹² These results suggest that extrapolating these results across large distances may not always be as bad as it might seem, but clearly many more replications need to happen.

Importance of Size

The problem of size arises because randomised experiments in particular tend to be relatively small scale, partly due to practical and financial constraints, but also in part because we want the experimental results before scaling up the programme. Other careful micro studies are less constrained, but the need to know exactly what is going on, which underlies good identification strategies, tends to limit the scope of these studies as well. Size is important for two reasons: One, which applies only to randomised evaluations, is that small programmes are easy to monitor carefully and therefore are not run like real programmes. While this is a concern, many of the more recent randomised evaluations are only small relative to nationwide programmes: The Balsakhi and deworming studies mentioned above; for example, each involved collecting data from tens of thousands of children and a number of other recent studies are even bigger. Moreover there is a lot of emphasis on making sure that the programmes that get evaluated are developed enough to be modular, in the sense that what gets implemented at every location is described by a simple and common protocol. This makes it easier to limit the involvement of the programme’s sponsors and the evaluators in the implementation process.

Size is also an issue because of what are often misleadingly called general equilibrium effects: Educating some more children in Vadodara or Mumbai is not the same as raising educational standards in the country as a whole – presumably if the number of educated people in the country goes up enough all sorts of other things will change. Wages paid to educated people will fall; the political clout of educated people will rise; others might see them and also start demanding more education; etc.

This is a very real problem, though obviously the import we need to give it depends on the question we are asking. If we are interested in how to get children to come to school, we might be okay in ignoring the effect of increased enrolment on wages 15 years hence, since parents probably do not think that far ahead. On the other hand, if we want to figure out how much we add to the future earnings of these children by making them go to school, it would be silly to not worry about what would happen to wages. Even the most committed experimentalist would want to do something.

II The Challenge to Theory

This is where, Mookherjee argues, we ought to be using more theory. Theory helps because what we really need is to make an assumption, and theory tells us what the right assumption would be. For example, we know from market equilibrium theory that the one number we need in order to assess the effect of the increase in education on wages is the elasticity of demand for skilled labour. Armed with this knowledge, we could go looking for an estimate of the elasticity and having found one (hopefully from an experiment or a quasi-experiment), could use it to calculate the true benefits from getting children into school.

Theory can also help us in solving the problem of scope. The conventional structural approach, which Mookherjee supports, involves fitting a model with a small number of unobserved parameters to the observed programme effects, and then using this model to make out-of-sample predictions.¹³ Conceptually, given that parameters of the structural model are estimated using the fact that the programme effect is different for people with different observable characteristics, this is not that different from a naïve approach, where we allow the programme effect to depend on

observables, and then extrapolate the results using what we know about the same observables in the new population. The advantage of the structural approach is that we have a theory to guide us about the kind of parametric restrictions we want to place on the data rather than having to guess at it. The disadvantage is that the theory may be wrong. The fallout of the behavioural economics revolution in economics is that we are no longer particularly sure of what the right theory ought to look like, especially inasmuch as decision problems are concerned. In particular, we are no longer secure in the presumption that utility functions and cost functions are somehow more stable and more universal than behavioural rules. Of course, as Mookherjee notes, we could also structurally estimate behaviour rules, but for that we would need a new body of theory. In the meanwhile it is not clear that using the existing theory always dominates simply assuming an ad hoc empirical specification, but perhaps it is best to use the two approaches in tandem, using one to check on the other.¹⁴

The bigger challenge to theory however comes from a different direction. The most important role of theory in development economics, and indeed in all the rest of economics as well, is to help us understand what are the right questions. The formulation of a testable hypothesis is only the final stage of this process, and one that is often left for the empirical researchers to do, since it often depends on the exact nature of the data. What is prior to that, and in some ways, even more important, is the ability of theory to locate the empirical results within a broader intellectual context and make us see why we ought to care. In this sense, a lot of the best empirical work of the last decade or so can be seen as a response to a body of theory developed in the previous decade that made us really understand the implications of living in a world where neither markets nor governments work perfectly.

What is unusual about the state of development economics today is not that there is too little theory, but that theory has lost its position at the vanguard: New questions are being asked by empirical researchers, but, for the most part, they are not coming from a prior body of worked-out theory. The most intriguing results from empirical research today, as I see it, are not the ones cited by BBM, but results like those of Bertrand-Karlan-Mullainathan-Shafir-Zinman (2004) (the decision to take a loan is at least as influenced by whose picture

is on the offer letter as it is by large differences in the interest rate),¹⁵ Karlan-Zinman (2004) (moral hazard in credit markets is a problem for men and not women, while for adverse selection it is the other way around),¹⁶ Duflo-Kremer-Robinson (farmers say that they do not buy fertiliser because they have no money, and do buy fertiliser if it is brought to them while they have money in hand, but will not go and buy it at the local store),¹⁷ etc. It is not just that we do not have a theory within which these results can be interpreted – it is not even clear how we would begin to build that theory.¹⁸ Indeed, the same goes for some less exotic results like Michael Kremer's finding that teachers teach better when students are given an incentive to do well (2004),¹⁹ but not when teachers themselves are rewarded if their students do well (2001).²⁰

The point is not that these phenomena are more complex than what we are used to and therefore need a more sophisticated theory. The problem is precisely that the correct explanation may not be particularly interesting or illuminating: Perhaps the borrowers in Bertrand-Karlan-Mullainathan just liked the picture and responded to it. Maybe the farmers in Duflo-Kremer-Robinson had never thought of the possibility of going to the store when they had the money (which is a few weeks before they need the fertiliser). Could it not simply be that the students in Kremer's study are better at getting the teacher to do what they want than the teachers are in getting cooperation from the students?

The challenge is to turn these explanations into a theory – something that would have implications for other things that we care about and pose new questions for empirical research. I believe that it can be done, but to do this we need to give up trying to defend the existing theory (which has been incredibly successful in many ways) against an onslaught of seemingly random results that are coming out of the field experiments. In Haavelmo's terms (as described by Mookherjee in the present issue) we are back at stage 1: We are gathering impressions that would eventually allow us to build a set of new theories that, one day, will define a "newer" development economics. ■■

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Notes

1 Deepa Narayan, *Empowerment and Poverty Reduction: A Sourcebook*, World Bank, Washington, DC, 2002.

- 2 Craig Burnside and David Dollar, 'Aid, Policies, and Growth', *American Economic Review*, September 2000, 90(4), pp 847-68.
- 3 William Easterly, Ross Levine and David Roodman. 'New Data, New Doubts: A Comment on Burnside and Dollar's 'Aids, Policies and Growth (2000)' ', mimeo Department of Economics, New York University.
- 4 Indeed, given all the identification problems with running this regression, there is no reason to presume that either result is actually correct. In other words, the real problem is that we have not learnt anything from the evidence. It therefore remains entirely possible that the MCA will end up doing much good in the world.
- 5 D Acemoglu, S Johnson and J A Robinson. 'The Colonial Origins of Comparative Development: An Empirical Investigation', *American Economic Review*, 91(5), 2001.
- 6 K Munshi (2003). 'Networks in the Modern Economy: Mexican Migrants in the US Labour Markets', *Quarterly Journal of Economics*, Vol 118.
- 7 E Miguel and M Kremer (2004). 'Worms: Identifying Impacts on Education and Health in the Presence of Treatment Externalities', *Econometrica*, Vol 72 (1): 159-218.
- 8 Glewwe, Paul, Michael Kremer, Sylvie Moulin and Eric Zitzewitz (2004), 'Retrospective vs Prospective Analyses of School Inputs: The Case of Flipcharts in Kenya', *Journal of Development Economics*, 74: 251-68.
- 9 E Miguel and M Kremer (2004), 'Worms: Identifying Impacts on Education and Health in the Presence of Treatment Externalities,' *Econometrica*, Vol. 72 (1): 159-218.
- 10 Bobonis, Gustavo, Edward Miguel and Charu Puri Sharma (2004), 'Iron Deficiency Anemia and School Participation', mimeo, University of California, Berkeley.
- 11 Hoyt Bleakley (2004), 'Disease and Development: Evidence from Hookworm Eradication in the American South', mimeo, University of California, San Diego.
- 12 Abhijit Banerjee, Shawn Cole, Esther Duflo and Leigh Linden (2004), 'Remedying Education: Evidence from Two Randomised Experiments in India', mimeo, MIT.
- 13 For an example of using a structural approach to analyse data from a randomised experiment, see: Orazio Attanasio, Costas Meghir and Ana Santiago (2003), 'Education Choices in Mexico: Using a Structural Model and a Randomised Experiment to Evaluate PROGRESA', mimeo, The Institute for Fiscal Studies.
- 14 For a version of an approach that compares structural estimates with a randomised evaluation, see: Petra Todd and Kenneth Wolpin (2003), 'Using a Social Experiment to Validate a Dynamic Behavioural Model of Child Schooling and Fertility: Assessing the Impact of a School Subsidy Programme in Mexico', mimeo, University of Pennsylvania. They however estimate the structural model without using the experimental data, and then use the experimental data just to confirm their structural results. The problem with this is that the non-experimental estimates may be hopelessly biased but sufficiently ill-estimated to still be consistent with the experimental results. It seems to make more sense to start

from the experimental results.

- 15 Marianne Bertrand, Dean Karlan, Sendhil Mullainathan, Eldar Shafir, Jon Zinman. 'Pricing Psychology: A Field Experiment in the Consumer Credit Market', mimeo, University of Chicago (2005).
- 16 Dean S Karlan and Jonathan Zinman, 2005. 'Observing Unobservables: Identifying Information Asymmetries with a Consumer Credit Field Experiment', Working papers 911, Economic Growth Centre, Yale University.
- 17 Esther Duflo and Michael Kremer and Jon Robinson (2004), 'Understanding Technology Adoption: Fertiliser in Western Kenya, Preliminary Results from Field Experiments',

mimeo, Poverty Action Lab, MIT.

- 18 For a discussion of why none of the existing theories help a lot in understanding the Duflo-Kremer-Robinson results on fertiliser adoption, and the challenge it poses for development thinking, see Esther Duflo (2003), 'Poor but Rational?' forthcoming in Abhijit Banerjee, Roland Benabou and Dilip Mookherjee (eds), *Understanding Poverty*, Oxford University Press.
- 19 Michael Kremer, Ted Miguel and Rebecca Thornton (2004), 'Incentives to Learn', NBER WP 10971
- 20 Daniel Chen and Michael Kremer, 'An Interim Report on a Teacher-Attendance Programme in Kenya', mimeo, Harvard University.

Goldilocks Development Economics

Not Too Theoretical, Not Too Empirical, But Watch Out for the Bears!

RAVI KANBUR

In this brief note I would like to set down some of my thoughts on the issues raised in this symposium. My perspective is to see development economics through the lens of mainstream economics. My conclusion is that the balance between theory and empirics is an ongoing process, in economics no less than in development economics. No doubt the pendulum will swing this way and that, and each swing will bring about its own correction. The balance will tend to be restored. But the really big issues for development economics are also the big issues for economics as a whole – namely, those that arise from our adherence to methodological individualism in a framework of “rational” choice, and the use of overly simplistic economic analysis in policy-making. These issues are neither solely theoretical nor empirical. But they are fundamental to economics and thus to development economics. Hence the title of this note. Goldilocks in the bears’ den found the right bowl of porridge for herself – not too hot, not too cold, just right.¹ She found the balance on porridge, but slightly neglected her fundamental predicament.

Theory versus Empirics?

The late David Champernowne, a British mathematical economist of rare

distinction, was once asked how much mathematics is necessary in economics. “The amount of mathematics I know”, was his answer.² There was a time when “mathematics” was associated with “theory” in economics. This is no longer the case. Mathematical techniques are as used in theoretical as in empirical economics, the techniques of mathematical statistics being prominent in the latter. The ground has shifted, since significant proficiency in mathematics and statistics is now the requirement in the best graduate economics degree programmes in the world. Rather, it would seem, the major divide is now between using these techniques in theoretical versus empirical exercises. “The amount of theory I do”, might now be Champernowne’s apocryphal answer to the analogous question for modern times.

In economics generally, the last 15 years have seen a major shift away from the prominence that was accorded to theory in the preceding decade and a half. One indicator of this is the Clark medal of the American Economic Association, awarded every two years to the most outstanding American economist under the age of 40. Of the last seven awards, only one was awarded to a pure theorist.³ Of the seven before these, five were awarded to pure theorists.⁴ This shift is reflected in the shift in graduate programmes, and appointments in top universities.