

New Empirical Development Economics: Remarks on Its Philosophical Foundations

Author(s): Kaushik Basu

Source: *Economic and Political Weekly*, Vol. 40, No. 40 (Oct. 1-7, 2005), pp. 4336-4339

Published by: Economic and Political Weekly

Stable URL: <http://www.jstor.org/stable/4417229>

Accessed: 21-10-2017 21:03 UTC

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# New Empirical Development Economics

## Remarks on Its Philosophical Foundations

KAUSHIK BASU

### I Introduction

The methodology of scientific research is largely a matter of intuition and knack. As a consequence it is hard to think of formal criteria for evaluating methodology. This must be one reason why the philosophy of science has made so little progress compared to science. The same is true of economics. The subject has made huge gains. But the skirmishes that have been fought about its methodology – for instance, the debate based on Friedman’s famous paper (1953) – have done little to enlighten us. Nevertheless, there is value in occasionally pulling ourselves back and asking whether the method that we use to advance knowledge in a particular discipline is right. Dilip Mookherjee’s masterly essay, based on a symposium at Cornell University, does precisely that. It is an exercise in introspection and an invitation to practitioners to take stock of the methodology of development economics.

There may be special value in taking up this invitation because development economics has witnessed an upsurge in innovative empirical research. The new empirical development economics<sup>1</sup> is a remarkable achievement. It is not surprising that, once its method was properly understood, there was an explosion of research using its techniques [by, for instance, Esther Duflo, Raghavendra Chattopadhyay, Edward Miguel, Simon Johnson, Michael Kremer, Daron Acemoglu, James Robinson, Kaivan Munshi, Rohini Pande]. But I have some critiques to offer of this method, which overlap partly with the criticisms put forth by Bardhan (2005) and Mookherjee (2005), and goes beyond to some rather philosophical matters. I shall in this essay concentrate on what, in Dilip Mookherjee’s paper, is described as Stage 4 research and a little on his late Stage 3 research.

One reason for the popularity of this new method<sup>2</sup> is the precision of its findings and because, once a result is discovered by this method, we fully understand what it is that has been discovered. Let me consider here one particularly elegant paper written in this mode, namely, Chattopadhyay and Duflo (2004). One of the results reported in this paper is the following: In West Bengal, having a woman head a panchayat has made a difference to what the panchayat does; it leads, for instance, to the better provisioning of water in the village.

In a lot of empirical research, there is the risk that reality may be the reverse of what is being claimed. For example, suppose that in areas with poor water-supply, women (whose traditional job is to fend for the household’s water supply) are so preoccupied with the task of fetching water that they cannot participate in local panchayat politics. This could easily create the impression that if women participate in panchayat politics then this leads to the better provision of water. In this case the deduction would be wrong; the causality runs the other way around. What is remarkable about this new empirical development economics is that by using the fact of an exogenous randomisation or the method of carefully-selected instrumental variable it has found a way to get around this problem.

Findings like the above one are extremely valuable; they tell us important things about the way the world is. A popular view, often encountered in newspapers, is that, in a traditional society like India’s, having a woman head a panchayat is of no real consequence because she would invariably do her husband’s bidding or be bulldozed by the male members of the panchayat. The study by Chattopadhyay and Duflo tells us, compellingly, that that is not so. The new empirical development economics aspires to results of this kind. Where this method applies (and as Mookherjee reminds us, it does not apply everywhere) it is an excellent method to use.

If despite this, I have criticisms of the new empirical development economics, it

is clearly not about the method itself but about what we do with it and how we interpret it. And on this there are many popular misconceptions that need to be countered.

Furthermore, it should be stated at the outset that I have some skepticism about all human endeavour to acquire knowledge. Hence, the limitations that I point to here apply much more widely than my focus of attention in this essay, to wit, development economics. I use the new empirical economics as a peg for my argument precisely because it constitutes some of the best research going on in the field.

### II Prediction and Policy

First, it must be recognised that the new method, at least by its own criterion of what constitutes correct inference, does not help us predict the future. Put differently, suppose we apply the strictness of criterion that this new method demands of empirical work, and then ask what we can predict based on this new method? It is important to understand that the answer is “very little”.

Suppose a researcher, studying the effect of aspirin, administers a very low dose of the medicine (150 mg, say) to a random sample of people found walking in the streets of Delhi and finds that, if a person with a headache takes aspirin, he typically (meaning, let us say, in 90 per cent of the cases) benefits from it. Let me call this the “research result” (RR). Now suppose you are asked what would have been the effect of giving 150 mg of aspirin to a random selection of people in Delhi who were lying down with a headache? Based on the RR could you say that they would have benefited from it? The answer clearly is no; you cannot say much, since this sample does not belong to the population on which the original study was done.

Let us now consider how we may use the RR for forecasting. If we next year give the people of Delhi, who are walking around, 150 mg of aspirin, could we expect that those with a headache would benefit from this? Strictly speaking, the answer is no. The people of next year’s Delhi are not the population from which the RR was derived. Also the state variables may be different. The weather may be warmer, there may be more suspended particulate matter in the air, and so on. So to use the RR and to predict the future is like doing

a study of the effects of administering aspirin to people who are walking and then presuming that it will apply to those who are lying. If we are fussy about proper randomisation for our study and take the view that we should not accept the wisdom of samples drawn in a biased manner or from the wrong population, we should also take the view that we cannot say anything about the future.

This, in turn, means that we cannot make policy prescriptions, since those are always recommendations for the future. This does not mean that the initial RR is useless. If a person told you that he had (at the same time that the aspirin research was being done in Delhi) selected people randomly from those walking in Delhi and given them 150 mg of aspirin, you would rightly be able to guess that 90 per cent of those with a headache would have benefited from this. The trouble arises with going over from yesterday's population to tomorrows.

One may try to counter this by arguing that between yesterday and tomorrow there is no fundamental difference and so no reason to expect a relation that was true yesterday to be not true tomorrow. But the difference between yesterday and tomorrow is not just a matter of time. Between yesterday and tomorrow there can be war and pestilence; between yesterday and tomorrow can be 9/11, altering the way world politics is conducted; between yesterday and tomorrow we can have a warmer globe.

One may respond to this further by saying that wars and pestilence do not make a difference to the human physical constitution and so we would expect the aspirin result to carry over from yesterday to the future. This is perfectly reasonable but in making this argument we are immediately conceding the role of intuition. We are combining our statistical finding with our prior "knowledge" that, for matters of health, knowledge acquired from one population can be carried over to another. We may hesitate to do this about the role of women in panchayats but feel confident about aspirins. This brings us to precisely the point I would endorse: These statistical findings are not useless for prediction but they have to be combined with unscientific intuition for them to be considered useful. We cannot reject the unscientific and claim that our method has predictive power.

Our intuition or our unscientific judgment comes in gradations. Before aspirin

is tried on anyone, we may have no faith in aspirin; once we know it has worked on people last year we have more faith in it next year. And the faith grows inductively. For that matter the fact that it has worked on people walking may make us inclined to believe somewhat that it will work on people lying down. But note that there is nothing objective about these beliefs. As I will argue later, what we so often take to be features of the world are actually propensities of the mind.

Let me move on from the subject of prediction and assume (maybe for reasons of intuition just discussed) that time cannot make a difference to the subject matter that is being researched. So a relation found to be valid today will be valid tomorrow – this is an axiom. In that case we can of course predict the future but there remains another fallacy that we can fall into if we are not careful in interpreting the results of the new empirical development economics.

Notice that, if we draw a person at random from among the Japanese who happen to be walking in Delhi,<sup>3</sup> can we then use the above research result and say that he will benefit (meaning with 90 per cent probability) from 150 mg of aspirin in case he has a headache? The answer is no, because this person was not drawn at random from the same population as the original research. He was drawn from the Japanese walkers in Delhi, which is different from the walkers in Delhi.

The stark case where this would be so is if 10 per cent of Delhi's walking population was Japanese, and it was the case that 150 mg of aspirin helped a walking person if and only if he or she was not Japanese. In this case the RR would be true and, at the same time, a randomly-chosen Japanese person would not benefit from aspirin.

This in turn leads us to the following troublesome question. Suppose you know of the RR (and this is guaranteed to hold over time). Now you are walking down Janpath in New Delhi with a headache, and wondering if you will benefit from 150 mg of aspirin. The answer depends on whether you can be thought of as a random draw of a person from the walking population of Delhi. It seems reasonable to me to treat the answer to be no. For one, you could be a Japanese in which case you are drawn from the population of Japanese walkers in Delhi. Hence, you cannot draw any policy lesson for yourself from the research result. This is a rather worrying predicament. It means that whenever I want

to use a research finding (based on proper controlled experiments) for my own treatment, strictly speaking, there is little reason for me to have faith in the result, since I am not a random draw from the population.

Am I using too demanding a criterion for what constitutes knowledge? Maybe, but since this new method in development economics is itself based on a very strict criterion of what is statistically correct, those who view this kind of correctness as essential should go along with my skepticism.

### III Knowledge and Evolution

The skepticism about the acquisition of knowledge expressed in the above section is troublesome. Despite my own inclination towards skepticism, I am aware that one must not have an unbending adherence to it (that would be a contradiction anyway for a skeptic). One possible mistake that both the skeptic and the practitioner of the new development economics have to guard against is that of denying that there may be other modes of acquiring knowledge.

To understand this, consider the number of things that we learn from poorly-controlled experiments or, for that matter, no experiments. A growing child learns soon enough that a frown implies displeasure and a smile implies approval that a slap hurts and a massage soothes (especially neck massage), that when a person cries she is sad and when she laughs she is happy.

Suppose this child's father stops the child each time she makes an inference, by asking whether she was sure that she was making the deduction from a proper random sample and not merely from her experience with those she happens to bump into in her everyday life. And suppose he asks her to discard any knowledge not picked up from properly randomised experiments. Surely this child will turn out to be a very ill-informed adult. The fact of the matter is that the knowledge that we human beings carry in our heads is disproportionately from wrongly conducted experiments and from biased samples. The knowledge that we have from scientifically-conducted studies – things like, 80 mg of aspirin a day can cut the risk of a heart attack by half and oatmeal reduces cholesterol – are a tiny fraction of what we know.

This is indeed a puzzle: How do we know so much given the atrociously-biased methods we use through life to collect information? Of course, we make errors. My son, whose first experience of the world outside of India was Belgium, for a long time after our return used to point to any white person he saw in Delhi and say confidently, "Belgian". It took considerable effort on my part to persuade him of the sampling bias that underlay his deduction.

There are three responses we can have to this puzzle. One is to try to show how, even if each person uses a biased sample, by the act of pooling our individual information, as we human beings do, the biases tend to cancel out and for the large part correct themselves. This would be an interesting research agenda in probability and information theory (even though it would still leave open the question of how we can predict about tomorrow from our information of yesterday).

But if this theoretical exercise turns out to be futile (and till such a result is proved it seems reasonable to proceed as if it were not true), then there are two possible positions that we can take. One is to claim that we human beings actually do not know much. Much of our knowledge is chimera – a mere illusion of knowledge. Many religious traditions and also some irreligious philosophers take such a view. There is a long Greek tradition of this.<sup>4</sup> Historically, the most famous is the philosopher, Pyrrho (4th century BC). Pyrrho did not write down any of his philosophy because he was skeptical about its value (though, of course, he could have been equally skeptical about the value of not writing and written a lot, like Bertrand Russell, also a skeptic,<sup>5</sup> did). It is believed that he went with Alexander's army to India and returned humbled because in India he had met "sadhus" who not only did not write but did not even speak. Legend has it that he heard one of his teachers asking for help, having fallen into a ditch; but he walked away calmly because he could not be sure that the teacher would be better off outside the ditch than in it. The teacher in this case happened to be Anaxarchus, a philosopher who held many similar views to those of Pyrrho. After he was heaved out of the ditch by some others and safe, it is believed that his greatest praise was for Pyrrho whom he had seen walk past with complete sangfroid [Diogenes Laertius 1925].

Maybe it was to counter these extreme versions that the later Greek philosopher, Carneades (2nd Century BC), stressed that, behaviourally, a skeptic need not be any different from a non-skeptic.<sup>6</sup> It may however be recalled that Carneades himself caused some comical problems by arguing one day in favour of justice and another day against it, since he felt committed to neither view. In fact, the Greek physician and philosopher, Sextus Empiricus (2nd century AD), took the view that the main consequence of skepticism was the tranquillity of mind achieved from resigning oneself to the futility of the quest for knowledge.

A second position, which is not incompatible with the above one, is to take an evolutionary view of knowledge [Basu 2000]. This is to admit that we do not know how we know things but, if it is the case that a person's knowledge or (at a more meta level) the facility for acquiring knowledge is inheritable, then the people who have wrong beliefs or knowledge about the way the world works (those, for instance, who think that the frown on the face of the person approaching them menacingly, knife in hand, indicates friendliness) would perish in the long run. So the people we see around know that an apple released in the air will fall downwards and know that the knife being brought down on a person will kill him. The fact of these people being around means that they and their ancestors have survived the weeding process of evolution. The ones who knew the wrong things or did not have the facility for learning correctly from nature are just not around anymore. By this theory, there is no right way to acquire knowledge. Nature is too idiosyncratic for that. But some minds are synchronised with our idiosyncratic nature and some minds are not. The people we see around us, by virtue of the fact that they exist, have minds that are in synchrony with nature.

My own belief about the puzzle of knowledge lies somewhere between the skeptical and the evolutionary claims. The trouble with this is that unlike others, who have strong views on what is the right method, I do not have any. I therefore find it difficult to take a clear position on debates like whether we need more theory or more empirics. I have some faith in our intuition. 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). And the same is true with theoretically-derived results. Some feel right; some do not. I would be tempted to give in to the feeling. Hence, the issue is not between theory and empirics.<sup>7</sup> We need to do both as correctly as we can and then use our intuition to select the ones we want to live by and base our policy recommendations on those and, most importantly, keep our fingers crossed behind our backs, when doing so.

#### IV A Comment on Causality

Economists have a propensity to find the lack of evidence of causality in other people's research. We complain about how other people's empirical papers demonstrate correlation but not causality. Such language is fine, as long as we realise that there is no real way for demonstrating causality. Indeed, there is no reason to believe that there is anything objective in nature called causality.

It may sound baffling at first sight but it is much more robust to maintain that causality lies in the eyes of the beholder. That is, we human beings have it hardwired in us to think in terms of causality. This can be a useful feature of our mind. It allows us to be more sure-footed than we would be if we could not live as if causality existed.<sup>8</sup> And, as we saw in the last section, there is reason to believe that what our mind takes to be causal is reasonably dependable because it has come to be reasonably well synchronised with the way nature actually works, thanks to thousands of years of human evolution. Hence, it is useful for us to feel that a particular relation is causal (and hence dependable in the future) and not merely a matter of correlation. But even on this we can make large mistakes. I will end on this cautionary note by constructing an example of how we can be led astray in our interpretation of probabilities.<sup>9</sup>

Suppose that there was a ritual in the world whereby in each city the mayor tosses a coin 20 times at the start of each millennium. So the last time this was done was on January 1, 2000. Let us suppose in Washington a researcher wanted to see if there was reason to believe that there was a particular bias in coin tosses in Washington and he discovered that in fact all the 20 tosses had yielded heads. He

would be tempted to publish a paper entitled, "Heads Bias in Washington," and may have speculated if having too many senators or too many lobbyists caused this. In case he had no experience of the rest of the world but had experience of coin tosses by others in Washington, he may have thought he had made the discovery of how the city's mayor is head prone. It is indeed very surprising to get 20 heads in a row. It does seem to say something to us.

Now return to the problem and think of the larger picture. Given that this ritual is followed in thousands and thousands of cities, the probability is very high that a sequence of 20 heads will occur somewhere. So the fact that this has occurred somewhere is of no interest whatsoever. It reveals nothing beyond what we already knew from our rudimentary knowledge of probabilities.

Let me digress for a moment to deal with a possible technical objection. A critic may say that before publishing the paper the author should have collected more information so as to decide if what happened was just pure chance. He may for instance collect data on another 20 tosses of the coin in Washington. But surely that will not change anything because we can pretend that this has already been done. It could, for instance, be that he had first collected information on the mayor's first 10 tosses and, seeing that all were heads, asked the mayoral office to send him the data on the next 10 tosses. He was then astounded to find that the next 10 were also heads and therefore wrote the paper. Therefore, we can think of larger and larger number of tosses. As long as there are more and more cities where the experiment can be done, we can construct the same, logically-equivalent story.

To return to the example, what we have are two propositions. The Washington researcher's feeling that he is onto something interesting seems justified. The outcome that he has seen has too small a probability if the probability of each coin toss by the mayor of Washington landing a head is half. So he is right in concluding that the probability of a head is much more than half. On the other hand, we also know from our larger global perspective that there is actually no special information in what happened in Washington. A string of successive heads occurring somewhere is very high, even if each coin toss has a probability half of getting a head.

The analogue of this problem is that researchers the world over are studying different phenomena. They publish only what seems unexpected. Since the expected does not get published, we never get the larger global picture (like the data on coin tosses from across the world) and so think we have stumbled upon knowledge when, in fact, we have not.

Am I making a mistake somewhere? 

Email: kb40@cornell.edu

## Notes

[I am grateful to Talia Bar and John Roemer for reading the essay, making helpful comments and criticisms and to Ted O'Donoghue for a very useful suggestion.]

- 1 Like some of the most interesting concepts in life (consider "expressionist art" or "Victorian manners") "new empirical development economics" is easier to identify than to define. I will, broadly, take it to refer to the recent research which uses controlled randomisations or carefully selected instrumental variables to reveal causal links between economic variables that are of particular interest to developing countries.
- 2 I refer to this as the "new method" given its relative newness in development economics. The method itself is not new – it has been used for some time in epidemiology, for instance.
- 3 For those planning to travel to Delhi, I should clarify that this gives a somewhat exaggerated impression of Delhi's cosmopolitanism.
- 4 An interesting summary account occurs in Long (1998).
- 5 Russell was however troubled by the fact that conventional skepticism was not just a philosophy of doubt, but what it never should have been – "dogmatic doubt".
- 6 "We Sceptics follow in practice the way of the world, but without holding any opinion about it" [Bevan 1950, p 52]. This, interestingly, amounts to a critique of behaviourism. But I take behaviourism to be an easy target, summed up (no doubt in somewhat of a caricature) in the observation, allegedly made by Bertrand Russell, that there is no way to tell the difference between a mathematician asleep and a mathematician at work.
- 7 The limitations that I discuss in this paper for empiricism should not be read as endorsement of theory as the instrument of choice for understanding the way the economy works. Theory can help us sort certain deductive complications but may not be able to do much more [see Appendix to Basu 2000]. For an excellent and persuasive essay on skepticism in the context of theory, see Rubinstein (2004).
- 8 The fact that human beings tend to pick up patterns beyond what is actually there has been documented in psychological experiments [Tversky and Kahneman 1971]. They report on several interesting findings. For instance, when

people are asked to guess what a random sequence of coin tosses will yield, they produce sequences in which heads are closer to 50 per cent than turns out to be the case in reality. This is especially true for short sequences. It could however be argued that this tendency on the part of human beings may have evolutionary survival value.

- 9 The philosophical foundations of probability have a history of confounding not just philosophers and statisticians but also economists, most famously John Maynard Keynes. Indeed, it may be Keynes's early encounter with probability theory that led to some of his strong views on the empirical method that Mookherjee discusses in his paper.

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