Development Economics and Method: A Quarter Century of ABCDE

[Opening Remarks at the ABCDE 2014, Washington]

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Introduction

It gives me great pleasure to welcome all of you to the Annual Bank Conference on Development Economics (ABCDE) 2014.

The first time I attended an ABCDE was when Stanley Fischer used to do my job. A letter arrived, quite unexpectedly, in my Delhi mailbox inviting me to attend the ABCDE in Washington. The World Bank would cover all my expenses and, not just that, I was not given any specific task, like that of writing a paper or commenting on one. Several members of the eminences grises of the profession were at the conference and I remember feeling rather tongue tied. So, taking advantage of the fact that I did not have a specific brief, I hardly spoke during the two days. I later figured that if you measured the World Bank’s expenditure on different participants in terms of the amount spent for each word uttered, I was the most highly-paid person at that conference.

I made up for that a little in 1992, when Larry Summers was the Chief Economist, and I was invited once again from Delhi, this time to comment on Paul Romer’s paper (Romer, 1993). And I will make up for this today, since the Bank did not have to spend on my travel and I do intend to say a few things.

I am aware that opening remarks can be a bit tedious because people have no idea about the content and how long they will last. I think it was Phillip Larkin, who had said preferred to read poetry instead having it read because that way he knew how far he was from the end. To cut out that uncertainty let me say at the outset that I will speak for 20 minutes, making some introductory remarks about ABCDE and our journal, the World Bank Economic Review (WBER), which will publish the proceedings of this conference, and then make a few remarks on the theme of the conference.
On ABCDE and WBER

This is a special year for us for two reasons. It is the 25th anniversary of the ABCDE, an initiative that was an unlikely venture when Stanley Fischer, and Dennis de Tray (his director of research), launched it in 1989. The stated goals at the time were “to improve member country and World Bank policymaking by enhancing the knowledge base”, and to “open up the Bank to outside ideas and problems, and if possible, to help shape the research agendas of those outside the Bank who were also thinking about development.” (Fischer, 1998). One can read either humility or arrogance from these stated objectives: from the start, the Bank was modest enough to involve outsiders in its thinking, and leverage intellectual resources wherever available, but also keen to influence the global research agenda on development. When I was outside the Bank I did not like the Bank trying to influence the global agenda. However, I like it now.

That same year (1989), a World Bank Regional Chief Economist, who would later become well-known, John Williamson, summarized under the label ‘Washington Consensus’ views that were supposedly held in the policymaking circles of Washington. In an interview in 2000, Joseph Stiglitz pointed out that it was not quite Washington, but Washington “between 15th Street and 19th Street” (Stiglitz, 2000).

It was the end of the 1980s, a difficult decade for developing countries—especially those in Latin America and Sub-Saharan Africa. There had been a debt crisis in Latin America, a decline of GDP per capita in Sub-Saharan Africa, and the Berlin Wall was about to fall. While the East Asian miracle was in full swing, it is understandable that the focus of the dominant themes of policy discussions in Washington was on the need for macroeconomic stabilization and structural reforms. Unfortunately, and contrary to Williamson’s original intent, the Washington Consensus became synonymous with market fundamentalism.

At the 10th anniversary of the ABCDE, held in Washington on April 20-21, 1998, then World Bank President Jim Wolfensohn noted the general belief that the Washington Consensus needed to be expanded and broadened.
I am pleased that, 25 years on, we have plans both for a special ABCDE, and also to revisit and take stock of the Washington Consensus. The world economy has changed dramatically since 1989 and the big issues on the global agenda have evolved too. Over the years the ABCDE has covered many themes in economics, from transition in socialist economies, the economics of military expenditures, and urbanization, to institutions, geography, and entrepreneurship.

A companion venture, The World Bank Economic Review, was launched in 1987 (two years before the first ABCDE), and has anticipated and accompanied these intellectual changes and pursued investigation into similar topics all along. Its stated goal has always been to encourage and support research in the field of development economics. It is now a widely-read scholarly development economics journal.

Contrary to some other journals supported by development institutions, the WBER accepts submissions from inside and outside the World Bank and they are subject to the same refereeing process and quality standards. And it has been our policy not to interfere with the scientific independence of the published papers.

Let me use this occasion to thank the outgoing editors, Elisabeth Sadoulet and Alain de Janvry, and to express my welcome to the new editor Andrew Foster. I am absolutely delighted that someone of the calibre of Andrew agreed to take this on and I expect the journal to scale new heights under his stewardship. I am pleased to announce that after a lot of negotiation, we have virtually finalized plans to have WBER be published in English, as usual, but also in Chinese.

On the Role of Theory in Development Economics

Let me now turn to this year’s conference and its theme. There are prodigious writings nowadays, in newspaper columns, on web journals, and popular magazines, offering easy solutions to our problems. Reading these I often feel quite inadequate. How come these people have so much to say and such clear solutions to offer when I find myself caught in a dilemma about these
matters—how to cut unemployment, how to control exchange rate fluctuations, how to stop inflation? At such times, I have to derive consolation from Thomas Mann’s observation that writing is much harder for the writer than others. I translate this to mean economics is harder for the serious economist than others.

Economics is what it is today because of the staggering intellectual breakthroughs, starting with Adam Smith in the second half of the 18th century, and all the way till now, with some towering achievements along the way such as Cournot’s seminal work in 1838, Herman Heinrich Gossen's book in 1854—this would remain undiscovered during his life time--, the magical last decades of the 19th century with major works by Walras, Jevons, Pareto, and Marshall to name just a few. The march continued through the 20th century with major research published by Keynes, Samuelson, Hicks, Von Neumann, Myrdal, Arrow, Debreu, and others. Let me leave it at that because to move to more recent times is to make enemies by naming, or more correctly, by not naming.

It is a fact of life that the greatest research in history, be it physics, mathematics or economics, did not happen because of the researcher’s urge to help society. The best research, for good or for bad, came from the innate human desire to discover patterns in the chaos of statistics and data or the passion for deductive reasoning that enabled people to expend huge amounts of energy and concentration to uncover connections between seemingly disparate facts. This pursuit has benefited society greatly, but the benefit occurred as a byproduct of this thirst for aesthetics. I wish the world was different, but it is not. The good policymaker respects this and devises ways to tap into this energy source in order to enhance social welfare and human well-being. The flowing river may not have an interest in generating electricity. But the river’s zest to flow can be tapped to create electricity and well-being.

While all the early pioneers were drawing on facts and knowledge of the world, most of the facts were rudimentary, and impressionistic. The heart of their work was theory. They tried to deductively explain different parts of the jigsaw puzzle of economic life. This is what has made economics the exciting discipline that it is today.
The excitement of theory was so great that there was a tendency to overdo it, to while away our time on ever-more abstruse mathematical constructions. The need for better statistical work, better data and more systematic evidence was given short shrift. Fortunately, this has been changing over the past two or three decades; and we have seen a flurry of activity in empirical economics never witnessed before. This has played a transformative role in economics in general, but specifically in development economics. Simple regression analysis, the method of randomization, and the analysis of big data have been transforming development economics (Banerjee and Duflo, 2009; Deaton, 2010; Ray, 2014; Varian, 2014). This is truly welcome and has the potential to leave its mark on human well-being, growth and development.

But there is a risk that this euphoria will once again have us carried away. We are seeing, especially in policy circles, these new empirical findings being quickly waved in front of our noses and treated as ground for doing whatever the policymaker wants to do. What is important to realize is that when we say that policy should be evidence-based, both words are important—“evidence” and “based”. We must not fall into the trap of evidence-waved policy. To see this mistake, consider the commonly-heard policy refrain: “Recent data shows 90% of jobs were created by the private sector. Therefore, we have to rely on the private sector for creating jobs.” The “therefore” is wrong. If it were not wrong, we would also have to go along with the Soviet economist who having studied Russian data in the 1980s wrote: “Recent data shows 90% of all jobs were created by the state. Therefore, we have to rely on the state for creating jobs.”

This is why we need the discipline of deductive reasoning, economic theory and also commonsense.

Krugman (1995) had explained the marginalization of development economics for many decades by the methodological choice made by many early development economists to reject the drive toward rigor, to ignore the pressures to produce mathematically-founded analyses, and adopt instead a loose verbose style in the name of pragmatism.
There are two reasons why we have to avoid the pitfall of only-theory or only-empirics. Since in today’s world the latter is the present and imminent danger, let me concentrate on this.

There are many truths that are discovered much more efficiently and convincingly by deductive reasoning, even though we may be able to close in on them empirically as well. I am referring here to logically unfalsifiable claims and tautologies, but they are so interesting and not-obvious that their discovery is well worth the pursuit. Take, for instance, Pythagoras’ theorem on right-angled triangles. If we insisted that all research had to be rooted in data and statistics, we may have forced Pythagoras to collect actual right-angled triangles and do elaborate measurements. He may have inched towards the same conclusion but there would be many criticisms; there would be the demurrers pointing out that his right-angled triangles were drawn from the Mediterranean region, that too disproportionately from the island of Samos, and from Croton in Southern Italy, and as such not a random draw from all available triangles in the world. Is there any guarantee, they would argue, that this would hold true of right-angled triangles in the artic or the Southern hemisphere? What about the right-angled triangles of tomorrow? Those were not part of the population from which his sample was drawn.

The magic of deduction, be it Pythagoras’s theorem or Arrow’s impossibility theorem, is that it cuts through all those criticisms in one fell swoop.

I am not saying that this should be the only method used. I am aware that by this method we would not be able to get to truths that cannot be deduced but must be inferred by induction. Knowledge that has to be acquired by induction can never be established beyond any doubt but it still plays a very important role. That an apple suspended in mid-air will fall down is something that even a child “knows” but this knowledge is nothing but induction, based on witnessing thousands of objects suspended in mid-air falling, and our intuition, which suggests this will continue to happen.

I have recently argued (Basu, 2014) that intuition and commonsense are not skills that we can dismiss out of hand. They are probably rooted in human
evolution. Species that intuited wrongly—believing, for instance, that apples suspended in mid-air will remain there--have perished. And for this reason our intuition must be respected and even treated as an ingredient in scientific inquiry and in the design of policy. What we need is a blend of theory, empirical investigation and reasoned intuition to get to knowledge and policy.

The recent rise in the use of randomization in development economics has greatly enhanced our knowledge. But in that enthusiasm, claims have been made on causality that don’t stand up to scrutiny. This is not the occasion to go into the details of this but I may be able to demonstrate the serious nature of the problem with an illustration. Suppose researchers come to your village, draw a random sample of inhabitants and establish that the injection of a green liquid into the arm enhances IQ, and has no adverse side effect. Will you trust that result and agree to be injected with this? To show the difficulty, let me fill in some details. Suppose this village is actually Eden, populated mainly by snakes and other weird creatures and may be just one more person like you, called Adam. In other words, the random draw on which the experiment was done was mainly snakes. You will have good reason to consider this experiment largely irrelevant for you.

Suppose you hear from a neighboring village, called Washington, where a biased trial was done using the same injection and where half the creatures injected died. But the creatures in Washington all happen to be human. You would be much more likely to trust that result despite its bias and be even more convinced that the proper random experiment done on your own population is less relevant to you than the biased study in a neighboring village. What you just used is intuition and a little bit of deduction based on this intuition.

This is a long and hotly debated subject. Let me leave it here by observing that to get to usable policy, we need to combine good statistical methods with theory and reasoned intuition. None of these three ingredients is dispensable. I hope I will come out wiser on how you mix and match at the end of this two day conference.
Acknowledgements

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References


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