Conceptual Challenges in Poverty and Inequality: One Development Economist's Perspective[*]

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Abstract

The last thirty years in the analysis of inequality and poverty, especially in developing countries, has seen two phases—a phase of conceptual advancement, followed by a phase of application and policy debate. Both phases were exciting and useful in their own way, but the applied phase has significantly exhausted the potential of the conceptual advances of two decades ago, and new advances have been few and far between. However, there is now a need, and an opening, for a new phase of conceptual advances, advances that will make use of shifting methodological terrain in mainstream economics, and that will answer emerging policy questions that would otherwise have no easy answers (or, perhaps, too easy answers).

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[*] Introductory comments at the Cornell Conference on Conceptual Challenges in Poverty and Inequality, April 16-17, 2002: http://www.arts.cornell.edu/poverty/kanbur/ConceptualChallenges.pdf. These comments were intended to kick off the discussion with some suggestions for conceptual challenges. The keynote speakers at the Cornell conference: Francois Bourguignon, Douglas Massey, Martha Nussbaum, Amartya Sen and William Julius Wilson highlighted many other areas where conceptual advances were needed. I am grateful to Kaushik Basu, Gary Fields, David Grusky and Dick Miller for discussions on this topic over the last year.

1. Introduction

It is a pleasure to welcome the distinguished participants, and especially our guests from outside Cornell, to this conference on Conceptual Challenges in Poverty and Inequality. The title encapsulates the organizers' intent for this multidisciplinary gathering. We would like to flush out the major conceptual issues that need addressing in the analysis of poverty, inequality and distributional questions generally, as an input into the pressing problems of policy design and implementation. Such an exercise, we feel, would help to set an agenda in this area for the social sciences and cognate disciplines, an agenda to which disciplines could contribute in their own particular way, singly or in concert. These opening comments reflect brief observations by one member of one sub-discipline. But they may help to indicate some of the issues that we might end up discussing in this conference.

2. The Last Thirty Years

I want to begin by a characterization, perhaps controversial, of the last thirty years of research on distributional questions in economics, development economics in particular. Somewhat arbitrarily, let me focus on the period beginning with Tony Atkinson's classic 1970 paper "On the Measurement of Inequality", and ending with Tony Atkinson and Francois Bourguignon's state of the art edited survey volume of 2000, "Handbook of Income Distribution." I would like to argue that this period can be divided, very roughly, into two periods: the 1970's through to the mid 1980's, and the mid-1980's through to the end of last century. In my view, the first fifteen years were a period of great conceptual leaps and ferment and were exciting for that reason. The second fifteen years, no less exciting, were the years of consolidation, application and fierce policy debates, especially on the distributional consequences of macroeconomic policies in developing and transition economies. Let me elaborate on this characterization.

2.1 The First Phase: Conceptual Ferment

There are at least four areas where the literature of the 1970's and early 1980's made basic conceptual advances or raised basic conceptual questions in economics—the measurement of inequality and poverty, the debate on utilitarianism, gender and intrahousehold issues, and social interactions in the generation of poverty. Let us take each one of these in turn.

It is difficult to overestimate the electric effect that Tony Atkinson's 1970 paper, or Amartya Sen's 1973 book "On Economic Inequality," or Sen's 1976 paper, "Poverty: An Ordinal Approach to Measurement," had when they appeared. They provided a way into conceptualizing and operationalizing value judgments on distributional issues, and by doing so acted as an antidote to a natural instinct among economists to avoid

distributional questions, an instinct that goes back to debates in the 1930s launched by Lionel Robbins's "The Nature and Significance of Economic Science". The appearance of these papers in the 1970's sparked a huge discussion on how exactly to incorporate distributional value judgments. In the measurement of poverty, the culmination of this process was undoubtedly the famous paper by James Foster, Joel Greer and Erik Thorbecke in 1984, "A Class of Decomposable Poverty Measures." This measure, which has some claim to be called the "Cornell measure" of poverty, has now become the workhorse of applied work on poverty the world over.

Amartya Sen's name is also associated with the second major category conceptual ferment of the 1970's and 1980s. This was the time when interaction with philosophical discourse enriched economist's perspectives on distribution dramatically. James Mirrlees's 1971 Nobel Prize winning paper, "An Exploration in the Theory of Optimum Income Taxation," is famous for many reasons, including the fact that it was an application of thoroughgoing Utilitarianism to the policy question of how progressive income taxation should be. The shortcomings of such Utilitarian fundamentalism were highlighted by Sen in a number of well-known works, including his 1987 book "The Standard of Living." But the 1970s were also the time of Rawls and Nozick. Nobel Prize winning economist Kenneth Arrow introduced Rawls to mainstream economists in terms they would understand—maxi-min strategies in the face of uncertainty (behind the Rawlsian "veil of ignorance" people would rationally support the constitution that aimed for the greatest good of the worst off since, "but for the grace of God", any one of them could be the worst off). New journals such as Economics and Philosophy were started, and journals such as Philosophy and Public Affairs had contributions from economists and philosophers.

The 1970s and early 1980s were also a time when economists began to make the conceptual leaps necessary to bring processes of social interaction to understand economic phenomena in general, and poverty and inequality in particular. Staying within the rational choice framework, but taking the issues of imperfect and asymmetric information seriously, George Akerlof, Michael Spence and Joe Stiglitz launched a body of work, which won them the Nobel Prize in 2001. Akerlof and Stiglitz, in particular, saw this framework as helping economists analyze such phenomena as the underclass in developed economies, or lack of investment in education by the poorest of the poor in developing countries. It was argued that in the presence of imperfect and asymmetric information, the market economy can produce multiple equilibria, some more efficient and more equitable than others, and that public action and intervention was necessary to move from the "bad" equilibria.

As a final example of the conceptual ferment on distributional issues in economics and development economics, gender and intrahousehold questions were put firmly on the agenda. The leading name here is again Sen; in the 1970s and early 1980s in a series of publications (with titles like "Family and Food: Sex Bias in Poverty") that brought home to economists that our "unitary" models of the household simply could not capture or explain the evidence on deprivation among females in developing countries.

While slow to get off the ground, this line of enquiry gradually blossomed conceptually and led on to applied and policy analysis.

2.2 The Second Phase: Consolidation, Application and Policy Debate

My contention is that from the early to mid-1980's onwards the conceptual ferment on distributional issues died down and we went into a phase of consolidation, application and policy debate. I do not in any way want to suggest that this second phase was less useful or less exciting—just that it was different.

The various inequality and poverty indices were systematically applied to data sets in rich and poor countries. Indeed, for developing countries this period was characterized by a significantly increased availability of household survey data sets. In Africa, for example, the first modern high quality nationally representative household survey data set dates from 1985 (for Cote d'Ivoire), and now there are more than a dozen countries that have at least one such survey—indeed for half a dozen African countries there are panels where the same households are surveyed two years in a row. This increase in data availability shows no sign of abatement, with the result that the measures developed in the 1970s and 1980s will have plenty of applications in the years to come. And each new survey will lead to empirical debates about the nature and extent of poverty and inequality in the country in question.

Similarly, the literature on intrahousehold and gender issues has progressed to consolidation and application. In 1995 a group of us wrote a paper entitled "Unitary versus Collective Models of the Household: Is it Time to Shift the Burden of Proof?" and gave an affirmative answer to our own rhetorical question. There is of course still strong resistance from the profession's basic reliance on the "unitary" model. But the debate is now on the details of this or that empirical test, not on whether factors such as intrahousehold bargaining between the genders in principle have a role to play. The asymmetric information literature is now part of standard graduate courses, and basic texts in development economics, such as that by Kaushik Basu, "Analytical Development Economics," very much use this perspective to frame much of the discussion of underdevelopment. Finally, the interaction between economic and philosophical discourses has also "normalized", in the sense that much Kuhnian "normal science" is seen in the papers in the new journals that were founded two decades or so ago. But some sort of an equilibrium has been reached. While all economists would know exactly what was meant by the term "Rawlsian objective function", "big ideas" in philosophy do not seem to animate graduate students in the same way as they did twenty or thirty years ago.

It is not that there has been no ferment on distributional issues in economics and in development economics in the last fifteen to twenty years. But it has been not so much on the conceptual as on the policy front. In the wake of the oil price shocks of the 1970's, many developing countries in the 1980s adopted—or, depending on your point of view, were forced to adopt by a cartel of International Financial Institutions—programs of "structural adjustment". These programs, primarily introduced in Latin America and in

Africa, contained the key elements of "the Washington Consensus"—including opening up economies to trade and capital flows and reducing the role the state in the economy. The fall of the Berlin wall in 1989 ushered in "transition" to market economies for countries of Central and Eastern Europe and the Former Soviet Union. These economies also adopted, or were forced to adopt, similar policy packages. Finally, in the late 1990s the world was hit by a series of financial crises, which many laid at the door of these same policies, especially deregulation of financial markets and flows. All of the above have now perhaps been subsumed under a general (and generally unhelpful) catchall heading of the debate on "globalization."

The debates of the last fifteen years in development economics have crystallized around the consequences of these policies and these developments, particularly for poverty and inequality (see my 2001 paper, "Economic Policy, Distribution and Poverty: The Nature of Disagreements"). The conceptual advances of the first fifteen years, particularly in the measurement of poverty and inequality, have of course been put to good use, especially as new data sets have become available. The debate has been fierce, with the term "Washington Consensus" acquiring the status of a term of abuse in some quarters. But, including the general and vague "markets versus state" issue that figures large in most exchanges, it has hardly led to or involved strikingly new conceptual questions—at least not from the economic perspective. The economic questions that abound in these debates—Is economic growth good for the poor? Is trade openness equitable and efficient? What exchange rate regime leads to least unemployment? Is international capital cartelized around the leadership of the Bretton Woods Institutions?—important as they are, do not seem to call forth major conceptual advances in the core of economics any more than normal. Fiercely debated? Yes. Conceptual Ferment? No.

3. A New Conceptual Ferment? Some Possibilities.

In my view, the last fifteen years have not been one of conceptual ferment in the economic analysis of poverty and inequality—distributional analysis more generally. This is certainly true compared to the fifteen years before, when new questions and new techniques, and new perspectives from the interaction with philosophy, framed a lively decade and a half. The last decade and a half has also been exciting, but in a different way—through application of the previous advances, but also through a highly charged policy debate.

Will the next fifteen years resemble the last fifteen, or the fifteen before? It is unlikely that the policy debates will disappear, although some of the passions may calm as at least some consensus develops. But what about conceptual ferment? Where will that come from? In this section I tentatively suggest three candidates for topics or questions in the distributional area that may well spark the conceptual excitement of three decades ago—measurement of poverty with differential mortality rates; behavioral economics foundations of poverty and inequality analysis; and dealing with multidimensionality of poverty and the standard of living. Like all good questions, these are not new. They have

been asked before, even recently. But in my view they are examples of questions that could trigger the conceptual advances and debates of the next decade or two.

3.1 Poverty and Death

As AIDS cuts its destructive swathe through Africa, its distributional profile may be changing. Increasingly, it is becoming clear that it afflicts the rural poor, and these are the ones who will be least able to afford the pharmaceutical drugs necessary for controlling its symptoms. Without getting into a debate on the epidemiology of the disease, I would only ask us to consider the possibility that the poor will die disproportionately from this disease in Africa in the decades to come. It would be a monstrous assault on our fundamental intuitions if these deaths were not recorded on the negative side of the ledger in any sort of social assessment. And yet the fact of the matter is that the most commonly used family of poverty measures, the "Cornell measure" discussed above, would *decrease* if the poorest person died as a result of poverty.

The technical-mathematical reason for why this happens is simple and obvious—removing the poorest person from the count lowers poverty as measured. But fixing this problem is not simply a technical matter. Somehow or the other we wish to keep track of the information that a poor person has died because of poverty in the past, in assessing the poverty of the present. But poor people also die from factors that are nothing to do with poverty—in the sense that with these factors mortality is not above the average for the population as a whole. So, in a sense, we have to keep track of all lives lived and why they died. And how far back do we go? From AIDS to the Partition of India and Pakistan? The Great Fire of London? The Black Death?

Of course practical considerations of data will soon put a stop to whimsy in empirical work. But the issue raised here is not empirical. It is conceptual. Current conceptualizations of poverty measurement focus (somewhat unthinkingly) on those currently alive, whereas adult death rates from AIDS will force us to consider the lives extinguished of those who have just died. Our conceptual tools do not seem to be adequate to the task. While we might get considerable help from an earlier literature (which goes back to the 1970s and early 1980s) on Utilitarianism and population policy (many will recall the debate over Derek Parfit's "repugnant conclusion"), I think new tools will have to be developed to address these concerns, and several deep questions may be broached in the development of these tools.

3.2 Behavioral Economics, Development Economics and Distributional Economics

Surveying the development of the last few years in economics, the journalist Louis Uchitelle of the New York Times (February 11, 2001) wrote of the late 1990's that "Behavioral economics had finally arrived: a discipline that for a half-century had built its theories on the rigid assumption that people acted with rational, unemotional self-interest had formally recognized that human beings had another, feisty, side to them…And if the behaviorists prevail, the mainstream view of a rational, self-regulating

economy may well be amended and policies adopted to control irrational, sometimes destructive behavior." This journalistic recognition went hand in hand with professional recognition. With last year's award of the America Economic Association's Clark Medal--a biennial award to the best American economist under 40 that is proving to be a safe predictor of future Nobel Prize awards—to Matthew Rabin of Berkeley, Behavioral Economics can be said truly to have come in from the cold. The work of a generation of scholars working in this vein, including Cornell's Bob Frank and (formerly Cornell's but now) Chicago's Dick Thaler has in turn been honored by the newfound recognition given to Behavioral Economics.

Interestingly, while the behavioral economists were toiling away to question the assumptions of basic rational choice theory, development economists and distributional economists, including myself, were rushing to embrace the central tenets of microeconomic analysis, to move their sub disciplines (kicking and screaming, as they saw it) into the "modern" era. In development economics, the insights and analysis of Arthur Lewis and Gunnar Myrdal were dropped and sometimes disparaged. In the policy realm, simple competitive supply and demand economics was the foundation for the "Washington Consensus policy package" discussed earlier. In the analytical realm, development economists searched for "microfoundations" through "rigorous" models. True, the best among them converged on the models and insights that flowed form the imperfect information paradigm of Akerlof and Stiglitz, but these did not depart from the rational choice frame in any strong sense. In a companion article to the Uchitelle piece, Roger Lowenstein describes Dick Thaler's work approvingly as follows: "Thaler and a trio of colleagues went on to document that cabdrivers stop working for a day when they reach a target level of income". But the young Turk development economists of the last twenty years found the same assumptions about peasants or informal sector workers in developing countries, by anthropologists and older development economists, to be "irrational behavior" and therefore not worthy of incorporation into their models.

The same observations can be made about economists working on distributional matters. As I have argued in my 2002 paper, "Economics, Social Science and Development," mainstream economics has been characterized by a combination of distributional sensitivity and an appreciation of the "the genius of the market", the latter flowing from the basic economic models based on rational choice and competitive markets. Those measuring poverty have, implicitly if not explicitly, taken a rational choice perspective. This is captured most vividly in the standard way of operationalizing well-being in empirical work—a simple sum of expenditures on consumption goods. There are some adjustments, to be sure, for public goods. But there are none for public bads. Thus, for example, a ceteris paribus increase in expenditure on addictive goods like cigarettes would count as an increase in well-being. If this increase happens for poor households, measured poverty would decline! And this is not just an out of the way example—the biggest increases in smoking are occurring in the developing world, especially in Asia.

At the very least, the smoking example highlights a paradox—at the same time that the World Bank issues reports about the disastrous consequences of smoking among the

poor in developing countries, its poverty measures must treat this increase as a contribution to reduction in poverty. But, at a deeper level, the example issues a conceptual challenge to development economists and distributional economists. We have had a good run into the bosom of mainstream economics by adopting its rational choice precepts. But the mainstream itself is being questioned by behavioral economics. What does this mean for measurement for the poverty, and for the analysis of anti-poverty policies?

3.3 How to Handle Multidimensionality of Poverty and Inequality?

The standard way economists think of and operationalize the standard of living is in terms of income (or monetary value of consumption). Measures of inequality and poverty are also thus primarily income based. In 1990 the late Mahbub ul Haq introduced the well know Human Development Index (HDI) which is a weighted sum of three components—income, literacy and life expectancy. The way Amartya Sen tells the story, he told Mahbub that the conceptual foundations of such a measure were very weak (sure enough, within weeks of the measure coming out many economists, myself included, attacked it on these grounds). But Mahbub responded that such a measure would raise issues of health and education on par with income in a way that nothing else could. As Sen has acknowledged, and as I acknowledge, Mahbub was right. The annual publication of the HDI is now an eagerly awaited event and invariably leads to debates within a country through comparison with "competitor" countries (US versus Canada, India versus Pakistan, Ghana versus Cote d'Ivoire, etc). Its benefits in terms of raising awareness at the highest policy levels has been incalculable, and it has been an integral part of the policy debates discussed in the previous section.

But it still remains true that the conceptual foundations of the HDI are weak. And this is because the conceptual foundations for handling multidimensional poverty and well-being are not yet strong enough to give confidence in the deployment of operational measure such as the HDI. If each of income, literacy and health improve, then we could perhaps declare an overall improvement. But what if there are movements in opposite directions? How are they to be aggregated to come up with an acceptable answer? And what is this thing that aggregation leads to? Or should we start from the meta level and define something into which each of these feeds as a component? Once again, Sen has provided a lead here in terms of his ideas on "capabilities". But it would be fair to say that these ideas have not penetrated into the mainstream of poverty analysis among economists—where simple estimation of the "Cornell measure" based on income/expenditure is still very much the rule. Bringing in education, health and risk as key ingredients of well-being is happening slowly, but separately, dimension by dimension, or simply as a subsidiary supplement to the income based measure. Bringing in other dimensions such as "voice" integrally seems a long way off. Even in their rational choice frame, perhaps especially in this frame, economists have not been as successful as they might wish to be in conceptualizing and then operationalizing the simultaneous evaluation of different dimensions of well-being, despite the remarkable efforts of some. There is a just a thought that perhaps releasing ourselves from the

straightjacket of rational choice, and moving to a more behavioral frame, might help in this endeavor.

4. Conclusion

I have argued that the last thirty years in the analysis of inequality and poverty, especially in developing countries, has seen two phases—a phase of conceptual advancement, followed by a phase of application and policy debate. Both phases were exciting and useful in their own way, but the applied phase has significantly exhausted the potential of the conceptual advances of two decades ago, and new advances have been few and far between. However, there is now a need, and an opening, for a new phase of conceptual advances, advances that will make use of the shifting methodological terrain in mainstream economics, and that will answer emerging policy questions that would otherwise have no easy answers (or, perhaps, too easy answers).

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