Theory or Empirics in Development Economics

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Since I agree with Dilip Mookherjee on the substance of his arguments, much of what I'll say is supplementary or annotative.

In the last two decades there has been a rejuvenation of development economics, and in this the leadership has been provided by a group of empirical researchers mostly in the US. In this period there has been an easier availability of detailed disaggregated, often household-level, data (for example, LSMS data of the World Bank for several countries and census data in many countries, apart from the Indian NSS, ICRISAT and NCAER data). Large memory capacities in our desk computers and sophisticated statistical analysis software have made the handling of gigantic datasets easily manageable, with a speed that still amazes some of us who, when we started our own empirical work, had small Facit machines to be hand-cranked for calculations (including inverting reasonable-sized matrices in the most labour-intensive way), and for large jobs had to wait for days in the queue in the main large-frame computer for the whole campus. Also the influence of labour economics, where applied econometrics had advanced in a major way a few years earlier, has been substantial in allied fields like development. Much more meticulous attention is now paid to finding appropriate identification strategy in our econometric analysis of causal explanations in a way that makes some of the earlier empirical work in development (including my own) look more cavalier. As empirical work has become more respectable in development economics, and sophisticated empirical strategies (including that of random evaluation) explore new horizons, whole armies of graduate students and young faculty are now excited by the field, and nearly 90 per cent of the papers presented in development seminars in the US (including mine at Berkeley) are now mainly, and proudly, empirical.

This is a big change from the days when the brighter students in development would not dirty their hands with the (inevitably messy) data, when the intricate theorems in optimum growth, general equilibrium, imperfect information, or game theory beckoned alluringly. The question that Mookherjee posits suggests that the pendulum may now have swung too much in the other direction. As someone who has dabbled both in theory and empirical work, my inclination is to agree with him and also point, as Mookherjee does, to some of the limitations of the newly fashionable empirical approach, without doubting its valuable contributions. I shall, however, qualify this with some special remarks on the empirical work on Indian development at the end.

In order to slay the dragons of "endogeneity" and "reverse causality" that threaten the validity of inference in many of the empirical exercises researchers are forever in search of clever "instrumental variables". Once one hits upon a clever "instrument", then the rest is considered relatively secondary, and you crank out your triumphant causal explanation from the two-stage regressions. In this we are often careless about the many pitfalls of instrumental variables, fail to examine the larger meaning of the identifying assumptions, and jump to conclusions. We forget that an instrumental variable even if it has satisfactory statistical properties does not by itself give us an adequate causal explanation or a satisfactory testing of a theory. Let me take the example of an article which is probably the most widely cited in the recent literature on how development depends on institutional quality: Acemoglu, Johnson and Robinson (2001) – AJR analysis. It so happens that I am often on the same side as these authors when it comes to underlining the importance of institutions in development economics, but I cannot go along with the way the profession seems to have lapped up their results as authoritative in quantifying the effect of institutions, and over-interpreted their significance. In their search for an instrumental variable that exogenously affects institutions but not income directly, they imaginatively picked a historical variable, colonial settler mortality, and used it in their first-stage regressions, and the predicted institutions then explained inter-country differences in per capita income in an econometrically cleaner way than has been the case before in this literature. Their presumption is that the mortality rates among early European settlers in a colony determined whether the Europeans decided to install resource-extractive or plundering institutions there, or to settle and build European institutions, like those protecting property rights which help long-run investment and development.

Importance of Institutional Quality

Even ignoring the usual questions about the quality and comparability of data that afflict such cross-country empirical exercises, the fact that some of the early colonies (say in Latin America) were run by Europeans (from Spain and Portugal) at a time when they did not have those property rights institutions quite in place even at home, and the fact that there are many developing countries which largely avoided colonisation (for example, China, Thailand, Mongolia, Ethiopia), this particular exercise has many substantive and methodological problems:

(a) Are property rights institutions the only type of institutions that matter for development? What about participatory and accountability institutions, or institutions of investment coordination? Bardhan (2005) quantifies the effect of some participatory and accountability institutions in explaining the cross-country differences in human development indicators.

(b) As Przeworski (2004) points out, the procedure of instrumenting recent institutions by referring to some old historical fact is flawed because institutions change over time. An instrument for the initial institutions need not be a valid instrument for the current ones. If good institutions are more likely to survive in more affluent countries, then institutional quality today is still endogenous with respect to income.

(c) Albouy (2004) corrects some flaws in the AJR settler mortality measure and shows that when the revised data are used, the AJR analysis suffers from a "weak instrument" problem. (In general in some of the recent use of the instrumental variable approach it is often ignored that a weak instrument can be worse than no instrument). AJR, however, has contested Albouy's corrections.

(d) The disease environment in the 18th or 19th century may be correlated with that today, and the latter affects current incomes directly as well as through its effect on institutions.

An econometrically cleaner method of establishing causality is through random evaluation, which, borrowing a method from medical experiments, has become increasingly popular in development economics, as Mookherjee has noted. This is a very promising development and some researchers have made a creative use of this, including getting involved with NGOs in the field, as participant observer/researcher and influencing their survey design to generate robust statistical estimates. One of the best examples of work in this genre is Miguel and Kremer (2004), on the impact of deworming drugs among Kenyan school children; this is one of the cleanest empirical assessments of externalities, which development economists always talk about but find hard to measure. But I share some of the general doubts expressed by Mookherjee on the methodology in general. By nature these experiments are much too microscopic, and there is a danger of missing "the forest for the trees". A rush to generalisations from these experimental results will be unwarranted, as they ignore macro- or political-economy or general-equilibrium effects of a programme when they are extended to a larger scale, and the whole is usually more than a sum of the parts. How reliable will it be to generalise the treatment effects of an NGO or government programme, when it is implemented nationwide (as in the case of PROGRESA in Mexico, Grameen Bank in Bangladesh, or self-help groups in micro-finance in India)?

And after all there is more to development economics than more precise programme evaluation or the impact study of a particular intervention. I can see that the latter is particularly important for some administrators and loan givers; I can also see why international financial institutions (like the World Bank) can benefit from the results of these studies on their many projects, which are currently often crudely evaluated. But the task of development economists is beyond writing more effective policy manuals; at least some of them should think about larger structural and conceptual issues. Our improved identifying strategies and controlled experiments have not made us any wiser in deciphering the mechanisms through which certain outcomes are generated (the "why" and the "how") and the social dynamics that are involved, and without these our causal explanations are weak, for all the precision of our tools or the statistical significance of our estimates. In fact, we are sometimes so obsessed with the precision of these tools, that we dismiss potentially insightful exercises that do not pass the standards of our econometric vice squads, and we often let the best be the enemy of the good.

In fact, our preoccupation with accurate quantification often takes us away from the more important causes of a phenomenon and we concentrate on variables that are better measured but may be socially and economically less significant, reminding one of the oft-repeated charge against economists, who look for the missing keys not in the dark place where they lost them but where there is more light. We tend to work with a thin conceptual menu and a large box of precision instruments. In contrast, sociologists and anthropologists spend much less time on honing these instruments, and more on a richer understanding of the processes, relations and dynamics.

Directions for Theoretical Research

In calling for a more balanced portfolio of research Mookherjee has briefly cited some of the directions which theoretical research can fruitfully take. Let me add a few more to his list. We don't know enough about the relationship between factor market imperfections and the social norms, and how this relationship evolves with changing demographic and technological circumstances; how economic processes and community institutions interact; how we can go beyond the existing partial-equilibrium models of oligopoly to generate a viable theory of factor prices and income distribution in a generally oligopolistic economy; an integrated theory of vertical product differentiation, firm heterogeneity, marketing economies of scale and international competition; the nature of transitional dynamics (as opposed to comparative statics) as we move away from a traditional low-level equilibrium; how economic processes get moulded when the state is weak and distant, the legal system is poorly enforced, and there is an "oligopoly" of violence (as opposed to the "monopoly of violence" that Max Weber ascribed to the state); and so on.

Also, in the recent theoretical and experimental work in economics on departures from self-interested maximising behaviour not enough attention has been paid to special behavioural issues that arise in poor countries. For example, the ideas emanating from the growing literature on fairness and reciprocity in individual economic behaviour need to be integrated with the idea of "moral economy" that historians and sociologists have talked about both with respect to European and Asian cases: for example, Rude (1964) and Thompson (1963) cite cases of peasant *jacqueries* where peasants in their rage against high prices sometimes looted the granary of a merchant but then paid him what they considered a "fair" price; Scott (1976) has cited cases in colonial Vietnam when peasants rebelling against landlords seized the harvest but paid the landlord what they considered a "fair" crop share. The interesting angle from the anthropological literature on this is that traditional notions of fairness are often asymmetric: (a) in acts of reciprocity or gift exchanges between two parties payments are not supposed to balance, and (b) moral economies often have double standards depending on the domain of the moral community (a peasant who is meticulously fair-minded in his transactions with fellow members of the community has no scruple in cheating people whom he considers outsiders). Our theoretical literature, while beginning to handle normative and weak-willed or time-inconsistent behaviour, is yet ill-equipped to tackle another type of social action which sociologists have pointed attention to: problem-solving interaction with others in which our ends and means co-evolve, with ends discovered and transformed in the process.

The psychological literature on cognitive dissonance and internalisation of severe constraints has obvious relevance to the way the poor behave: as Sen (1984) reminds us, "many of the inequities of the world survive by making allies out of the deprived and the abused", and as Appadurai (2004) reminds us, the poor may lack "the aspirational resources to contest and alter the conditions of their own poverty". The constraints they internalise are not just the ones they themselves face, but their parents, peers and neighbours have faced. There is much to draw here from the sociological literature on role models and peer effects and the growing socio-economic literature on neighbourhood effects (mostly relating to poverty traps in US inner-city ghettos). This also relates to the role of group pride, group anxiety and other culturally sanctioned collective processes which deeply affect our preferences (particularly in developing countries suffused with ethnic, caste, or religious affinities), and are yet largely ignored by economists. This has implications for incentives and organisations in labour markets – as Akerlof and Kranton (2005) have emphasised, but also for our theories of collective action (which are dominated by simplistic free-rider presumptions). It raises a larger methodological issue as well. Methodological individualism which undergirds most of economics (including the recent attempts to depart from postulates of self-interested maximisation) may not be an appropriate principle when issues of group dignity and autonomy supercede individual-oriented motivations.

Dearth of Careful Empirical Work in India

Finally, while I may agree that there is too little emphasis on theory compared to the newly fashionable, though highly valuable, empirical exercises, this is mostly in relation to development economics as practised in the west. I have to qualify my agreement in relation to development economics as practised in India, where I believe the old brahminical tradition of high premium on theory still persists. We have a fascination for the esoteric intricacies of a theoretical problem, much less for the sweat, toil and tears involved in grubby empirical work. In particular, there is a dearth of careful empirical work on many vital issues of the Indian economy. While there is a massive amount of largely descriptive empirical work published every year (including in the pages of the *EPW*), a great deal of it would have improved in explanatory power if more attention were paid to the identification strategies and selection issues that worry econometricians. This also would have stimulated the need for better data collection and for trying out experimental methods in the field, involving real farmers, real workers and real entrepreneurs. There is a great need for more empirical work in India on the structure and practices of enterprises in the vast informal sector (more than 80 per cent of even the non-agricultural labour force is employed there), on interrelationships among firms in a given industry, patterns of industrial evolution with the use of firm-level data, on how our marketing and informal insurance institutions work both in agriculture and outside, work place practices, recruitment mechanisms and wage-setting institutions in industry, trade, and the service sectors, on patterns of technology diffusion, etc. We have a relative abundance of household data on consumption and employment. This has fuelled the endless debates on measurement of poverty and inequality over the last four decades. Yet to this day we know very little on inter-generational mobility, which is probably the most important aspect of inequality in an extremely hierarchical society like ours, and there are very few attempts at collecting the requisite longitudinal surveys of families. Compared to other countries, we have much less solid empirical work on the necessary restructuring of our pension system or the building of a comprehensive health insurance system outside the formal sector. Fewer good economists are now working on Indian economic history. Just to give one example, there is now a theoretically-informed economic-institutional history of long-distance trade and credit in the Mediterranean and western Europe in the early modern period (for example, the work of Avner Greif). Similar work cries out to be done for India, with its rich history of long-distance trade and credit, of "hundi" and "hawala", and how the caste panchayats and other multilateral reputation mechanisms enforced the rules of conduct. I could go on and on. EPW

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