VIEWPOINT

Promising Research Directions in the Development Field and Christian Economists' Role

Julie Anderson Schaffner, Tufts University (MA).

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My first task, as I understand it, is to say something about the directions for research in my field that I think will be especially fruitful in the next few years. In the most general terms, my field is the Economics of Development. My research falls on the micro side of that vast subject, and focuses primarily on labor markets in Latin America. Most of my work has some applied econometric component. I will organize my comments around three general themes regarding the sorts of research directions that look especially interesting.

Theme 1: Interesting research in the next few years will take policy more seriously than has typically been the case in past research. By "taking policy more seriously" I mean at least two things. First, I mean taking policy implications more seriously as the goal of research. That is, designing research projects that are intrinsically linked more closely and usefully to specific policy questions. (I don't mean just adding tenuously related paragraphs on "policy implications" to the concluding sections of papers.) This is a good time for such research for several reasons: we are still in an epoch of dramatic policy reforms in developing economies and economies in transition. In at least some places, policymakers either have been academics themselves or are receptive to inputs from academics. But in some fields, there is a big gap between the set of questions policymakers would like answers to and the set of questions the academic literature has been addressing. (This came out quite clearly at a conference on frontiers of research on economic policy reform held at Stanford's Center for Research on Economic Development and Policy Reform last fall.) At the same time such research is becoming much more feasible, because repeated cross section, and even occasionally panel, datasets from household and firm surveys are increasing in quality and availability, and governments are making policy changes that provide some variation from which to learn (with caveats and care, of course) about policy effects.

An example of a topic that is ripe for more specific, useful research relates to job security legislation. We need to ask much more specific questions than just whether "flexible" labor markets are good or bad for growth. Policymakers seeking to make labor markets more flexible do not write laws that declare "labor markets are now more flexible." They often make specific changes to many details of complex laws, while retaining the law's basic structure. They need to know whether they should start with changes in the average size of severance payments, the way the size of severance payments is linked to tenure on the job, the definition of "just cause" for dismissal, the length of the training period during which workers can be fired at will, or some other detail. The recent emergence of household surveys with job tenure information, over periods of time during which major reforms in job security legislation have taken place, has made it possible to start examining some of these questions.

The second sense of "taking policy more seriously" is: taking the details of policy, and of the institutions that enforce and shape them, more seriously as inputs to research. Here I'm thinking of researchers really steeping themselves in the messy, and often difficult-to-compile, details of laws and institutions. Some interesting research advances will be made by researchers who make a significant investment in learning the regulations, and who then find clever ways of exploiting some of the details of the legislation and institutions to develop strategies for identifying important empirical effects. For example, oddities of eligibility requirements that cause two fairly similar groups to be affected very differently by a policy change might generate something approaching a "natural experiment." Or, variation across provinces within a large country in labor court precedents regarding "just cause" for dismissal might allow a researcher to estimate effects of changing some job security legislation parameters. Finally, careful look at specific values taken by some parameters in wage or job security policy might lead researchers to suspect spikes at particular values in wage or job tenure distributions, which they can attempt to identify and to use in looking for policy effects.

Theme 2: Interesting research in the next few years will build bridges between the macro and the micro literatures. The divide between macro and micro literatures is much larger in the development field than in economics as applied to the advanced countries. Some bridging of this gap will take the form of more disaggregated studies that seek to open up black boxes that recent macro literatures have identified as important. For example, cross-country regressions suggest that countries with better education stocks grow faster, but that is not much of a guide to policy. We do not know the best ways to get kids to school or to make sure that something productive happens while they are in school. Exciting work involving social experiments is going on here (much of it directed by Paul Glewwe and Michael Kremer). There's room for much more.

Data also indicate that labor productivity is lower in developing countries, even after controlling for physical capital per person and for formal schooling stocks. This suggests to me that we should look at the shop floor to see how employers are trying to bring workers into the production process. What do we know about employment arrangements, compensation practices, use of Total Quality Management, on-the-job training, etc.? How are these related to levels of productivity? Are there barriers to training, or to the use of various incentive contracts that boost productivity or reduce turnover?

Another possible culprit explaining low labor productivity is "poor institutions." It is widely agreed that good courts and well defined property rights are essential to well functioning market economies, but that, too, is a large black box. Economists need to start taking a much closer look at court systems.

Endogenous growth models specify simple functions relating productivity advance to a few key variables in the economy. Can we open up that black box? There is already some very interesting work using firm-level panel data from various developing countries to assess, for example, whether productivity advance within industries is generated more by births and deaths of firms or by changes within firms (see the recent book by James Tybout and Mark Roberts). Such evidence should be used to discipline the range of theoretical models about growth.

Other research will bridge the gap between the macro and micro literatures by using micro methods but moving beyond study of individual or household behavior on just one side of the market. It will shed more light on the structure of markets, sectors or the macro economy. An obvious example of the need for research on market structure relates to market power. It is funny that, despite the common belief that small domestic markets give more scope for market power in protected developing economies, there is surprisingly little micro study of market power concerns. Studies of market power must be especially important for discussions of the regulatory frameworks that must accompany privatization. I take another example of this kind of research from a recent paper by Jonathan Morduch. He pointed out that much research on health care in developing countries seems to assume away the private sector. It compares the bang for the buck of big hospitals versus smaller, dispersed primary care centers, but does not ask much about the structure of the market, and what it would look like in the absence of intervention. Public-private interactions are important to policy analysis, because if public primary care centers merely crowd out comparable private centers, then government money might better be used elsewhere.

Moving up from the individual to market level is also important in the study of labor market issues. Many micro labor studies of developing countries take as given that there are large "informal" sectors, which are defined with reference to self-employment versus wage employment, firm size and whether or not the employer is effectively covered by labor legislation and other regulations. Studies consider how education and other characteristics affect sector choice, how wages differ across them, etc. But to me the really interesting question is: why are rates of self-employment so high and why are typical firm sizes so small? The answer to this is probably closely related to the explanation for the low labor productivity I mentioned earlier. To make headway here, I think we will need to improve the theoretical base for empirical studies of developing country labor markets.

A final research area that looks potentially useful is related to recent theoretical work by Caballero and Hammour, and others. This work starts with simple micro observations about difficulties in contracting between employers and employees (such as the need for specific investments in employment arrangements) and builds up from them implications for macro performance over the business cycle or in response to policy reforms. I would love to see more brainstorming about how micro data could be used to assess the importance of the critical assumptions, to determine whether the underlying contracting problems are indeed more severe in developing countries, and to draw out implications for policy.

Theme 3: The final theme I will simply state without elaboration: interesting research will make much more use of the tools of industrial organization, regulation, public economics, comparative institutional analysis and mechanism design, rather than the heretofore more common specializations for development economists of labor, demography, and money and banking.

My second task is to say something about the unique role, if any, that Christian economists have to play in carrying out research of the sorts I have just described. "Unique" is a strong word that I don't feel comfortable applying to Christian economists' role here. Still, it is useful to think about the relationship between Christian faith and how one inserts oneself into this research agenda. I came up with the following statement: "As a result of God's working in their lives, on average, Christians should be better equipped and motivated than other researchers for some characteristics of good research in these areas." Let me expand on this highly qualified statement.

When I say "some characteristics of good research" I have in mind research that is steeped in knowledge and understanding of legal and institutional detail (which often means that the researcher spent time in the field or spent time cultivating relationships with colleagues who live in developing countries) and targeted at answering useful questions. I am also thinking of researchers who are enterprising in the acquisition of data. Good datasets are much more accessible now than a few years ago, but to answer many interesting questions, researchers will need to develop relationships with Census Bureaus that allow them to add a few questions to ongoing household or firm surveys. Or they may need to cooperate with NGOs to collect new data. I also am thinking of researchers who are very careful in the use of imperfect data, paying attention to details of how questions are asked, how variables are constructed, sampling concerns, missing data, and measurement error, and who double- and triple-check all their calculations. Finally, I am thinking of research papers that exhibit intellectual honesty and transparency, not sweeping certain regressions that would make the story less pretty under the rug, and not passing off as trivial assumptions that are crucial and highly debateable.

Except perhaps for the last characteristic, these all require significant investments of time and energy. Only occasionally will these investments generate large, fairly immediate private returns. (They might have such returns, if, for example, attention to detail pays off in a really neat strategy for identifying an important policy parameter. But most of the time the private rewards are small, and probably off in the future after a body of solid work along these lines has developed.) In some sense, though, the social returns are higher, because these things are necessary to avoid misleading policy makers, and doing them is, in my opinion, just plain right.

When I say we should be "well equipped" for research with these features, I mean both that we should be more observant of the need for such investments and that we should have resources to draw on that make us willing to undertake the investments with energy, patience and joy.

Why do I think we "should" be well equipped for undertaking these investments with energy, patience and joy? The answers are obvious to members of ACE. We believe in an all-powerful God who holds our future in his hands, who loves us dearly and who gives our life meaning: we don't have to worry that our lives will be ruined if our research takes too long or does not produce an exciting result. We have also been commanded to do our work (whatever it may be) heartily for the Lord, rather than men, knowing that it is the Lord Jesus Christ whom we serve: we can be intellectually honest because Jesus values it, even when analysis of worldly costs and benefits argues against it. Furthermore, we desire to be more like Jesus and to bear the fruit of the Spirit, including love. And I think that caring about the people affected by the regulations we analyze, or who are involved in the markets we study, helps make us especially observant of potentially important details. Finally, we have the family of believers around the world, which can be of great practical help in making field work feasible, enjoyable and productive (spiritually as well as professionally). I certainly have found this to be the case.

The final qualification in my statement that I should explain is "on average." If we were to characterize all economists, Christian and non, by some index of the energy, patience and joy that they bring to such research, we would find that there is dispersion in both populations, because people differ in personalities and experience, as well as beliefs. I think we should discover that while the distributions for the Christians and others overlap, the mean for the Christians is higher. And it should be higher not just because of endogenous selection of high-index people into the camp of believers, but because of the intrinsic effect of God's grace, which takes us from wherever we started on the energy-patience-joy spectrum and moves us to the right. ■