Research in Practice at the World Bank

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October, 2007

The overall objective of research at the World Bank should be exactly the same as the Bank's overall objective: to fight poverty through sustainable and inclusive processes of economic development. Here "poverty" is taken to be a broader concept than the standard income-based measure based on an international poverty line (such as the number of people living below "$1 a day"); it embraces both relative-income poverty in specific country-settings and dimensions of poverty not captured by incomes, as conventionally measured. Similarly, "economic development" is more than economic growth.

The Bank's main instruments for fighting poverty are development lending, policy advice to developing countries and its participation in global initiatives. The challenges for research in supporting these instruments span the entire gamut of development policy, including access to finance, human resource development, rural and urban infrastructure, redistribution and insurance, macroeconomic stabilization, trade reform and environmental sustainability.

The Bank's full-time researchers are mainly found in the Development Research Group (DECRG) within the Development Economics Vice-Presidency. The 100 or so researchers in DECRG are embedded within a large development agency (with a global staff of about 8,500) that is highly oriented toward the practice of development—with a deep commitment to fighting poverty in the world. What is the role of research in the practice of development at the Bank? How can that role be served better?

The relationship between research and practice

In thinking about how the Bank's research efforts can help in the institution's overall mission one must start with a realistic view of the relationship between research and practice. Experience suggests that it is unusual to find an unambiguous one-way track from specific research results to concrete policy actions. Good policy making for fighting poverty draws on inputs from many sources, including research, and is tailored to country circumstances. Not every important piece of development research has an immediate and clear policy implication. Good research is rarely so conclusive in its policy findings, although the ambiguities often fade in time, in the light of accumulated knowledge.

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There are some inherent tensions in the relationship between researchers and practitioners, stemming from their different roles in serving the same objective. Development practitioners often have an advocacy role, as they strive to improve peoples' lives. This can be in tension with the perspective of researchers who are more inclined to critically question the foundations of any policy position—to see if it really does improve peoples' lives. Balancing advocacy and rigor can be difficult, although it is usually clear when one has gone too far in one direction. Even when research findings are properly presented (with all the caveats acknowledged), practitioners keen to demonstrate impact are sometimes tempted to highlight research findings selectively; or to engage with researchers selectively, such as when practioners only put forward their best projects for serious evaluation. Sometimes researchers have to point out such biases, and doing so does not always make them popular with practitioners.

There is also a danger that researchers, eager for impact, overreach in their policy recommendations. A few researchers have at times been swayed by the publicity, and even fame, that can come when high-profile advocacy is disguised as research. This can undermine the credibility of the researcher, and can also jeopardize the credibility of other Bank researchers.

While the long-term interests of the Bank are not well served by its researchers turning into advocates, they are well served by having researchers engaged in the process of deciding on the most defensible policy stance, and engaged after, to maintain critical scrutiny on that policy stance.

Another source of tension in the relationship concerns the role played by what can be termed "policy rules." It is not always practical to adapt development policies to each context. Understandably, practitioners look for "best-practice" generalizations to guide action in diverse settings with limited information. This often runs into conflict with a researcher's perspective, which emphasizes the uncertainties and contingent factors in policy choices, which also points to the importance of context to outcomes and the need for better data. In principle one might develop a complete mapping from every possible circumstance to every policy, but that is rarely feasible in reality. So the tension is inevitable between practitioners looking (often in vain) for robust rules to guide action and researchers pointing to the caveats and unknowns.

At the same time, researchers need practitioners. Without the prospects of implementation in practice, much development research would be rather sterile stuff, of limited interest to the types of people attracted to doing research in the social sciences. I doubt if there is any researcher in DECRG who was not attracted to the job (in part at least) by the prospect of turning research into practice.

Recognizing the complexities and occasional tensions in the relationship between research and practice, researchers can play a crucial role in both <u>learning from past policies</u> and <u>thinking critically about future policies</u>. Without research, the conceptual foundations for policy making will be weak; there will be very little new knowledge or data to inform policy decisions; there will be little or no innovation; and we will know too little about what succeeds and what fails in the fight against poverty. Without research, failed policy orthodoxies will often persist without the critical inspection needed to show that they have failed. And successful policies will be dropped for the wrong reasons.

Realizing the potential for research to inform practice requires that practitioners have a realistic appreciation of the potential benefits from research and that the costs to them are not too high. Increasing the perceived benefits is largely about making sure that research addresses relevant questions and that it provides credible answers. The costs to practitioners of accessing research findings depend on our publication and dissemination efforts.

How researchers can help practitioners

A good research program for the Bank reflects the main researchable issues in the Bank's current and (likely) future work across all sectors. Here I want to point to two broad cross-cutting types of research that can have real impact.

First is evaluative research, which rigorously assesses whether development policies are effective, and under what circumstances they tend to be more effective. This is broader than impact evaluation, and embraces both "micro" interventions in specific sectors and macro policies, and it includes both *ex ante* and *ex post* evaluation.

The Bank's potential for evaluative research is a (seemingly) unique feature of the institution within the development community. The Bank's practitioners search continually for operational solutions to pressing development problems; the Bank's researchers have the training and skills needed to provide the conceptual and technical tools that can inform that search, and to help learn from our success and failures. How can we assure that the potential is realized?

Evaluative research must start with a clear understanding of the problem that a policy or project is addressing. Why is the intervention needed? What are the market, or governmental, failures it addresses? Researchers can often help in identifying the policy objectives (properly weighing gains across different sub-groups of a population, and different generations) and the relevant constraints, which include resource, information, incentives and political economy constraints.

This role of research in conceptualizing the case for intervention can be important when development policy making is captured by lobby groups, advocating narrow sectoral interests. The existence of trade offs between sectors (such as due to governmental budget constraints), spillover effects across sectors (costs and benefits to one sector from policies in another) and interaction effects (whereby attainments in one dimension influence the impacts of policies in another) beg for a broader perspective on the normative foundations of policy.

As is now widely appreciated, evaluative research must always assess impacts against explicit and relevant counterfactuals, such as the absence of the policy in question or some policy option. This requires sound evaluation designs, using good data and credible strategies for identifying causal impacts from those data — taking proper account of the likely sources of bias, such as when outcomes are only compared over time for program participants, or when participants and non-participants are compared at only one date. This is familiar terrain for research economists. I would like to flag some issues that are possibly less familiar, and matter greatly to the impact of evaluative research.

Most importantly, to maximize impact, evaluative research must be driven by interesting and important questions. This may seem obvious, but too often it is not the question that is driving the research agenda but a preference for certain types of data or certain methods; the question is then found that fits the methodology, not the other way round. We then run the risk of ending up with research findings on questions of secondary importance.

Starting with the question, not the method, often points the researcher toward types of data and methods outside the domain traditionally favored by the researcher's own disciplinary background. For example, some DECRG research economists trying to understand persistent poverty and the impacts of antipoverty programs have been drawn into the theories and methods favored in sociology and psychology. Good researchers, like good detectives, assemble and interpret diverse forms of evidence in testing empirical claims.

Good evaluative research must also recognize the importance of context. Heterogeneity in impacts is to be expected. Unless we understand the role of context, the research will have weak external validity, which is key to scaling up lessons from research. Contextual factors include the circumstances of participants, the cultural and political environment, and the administrative context. For example, a recent evaluation by DECRG researchers of a large World Bank lending operation found low overall impact, but considerable heterogeneity, in that different types of households benefited more than others; the policy implication is that choosing different beneficiaries would have greatly increased the project's overall impact. Furthermore, not all sources of heterogeneity are observable, and participants and stakeholders often react to factors unobserved by the researcher, confounding efforts to identify true impacts using standard methods.

Although external validity is highly desirable for evaluative research, it can be hard to achieve. We naturally want research findings in specific settings to have a degree of generalizability, so they can provide useful knowledge to guide practice in other settings. Thus empirical researchers need to focus on questions that tell us about <u>why</u> a policy or program has impact. Yet too often impact evaluations are a "black box"; under certain assumptions, they reveal average impacts amongst those who receive a program, but say little or nothing about the economic and social processes leading to that impact. And only by understanding those processes can we draw valid lessons for scaling up, including expanding to other settings.

Research that tests the theories that underlie the rationales for policy or program intervention can thus be very useful in practice. Even field-hardened practitioners do what they do on the basis of some implicit model of how the world works, which rationalizes what they do, and how their development project is expected to have impact. Researchers can perform a valuable role in helping to make those models explicit and (where possible) helping to assess their veracity. For example, it was once routinely assumed that project aid financed exactly what it claimed to finance. Research has pointed to a degree of fungibility in development aid. Donors probably do not fund exactly what aid recipients claim they have funded (although there is some evidence that external aid sticks to its sector). While academic work has provided important theoretical insights on the fungibility of development aid, the more fundamental empirical research that has been conducted on this topic probably would not have been possible without the kind of close interaction between research and practice found within the Bank. There are many other examples in which research has played a crucial role in questioning the received orthodoxies of development policy. That fuels a deeper understanding of the real-world factors that impinge on the Bank's success and helps identify the most effective leavers for the institution.

Research aiming to evaluate policies and programs must also be operational, in that it is feasible in the contexts in which the policies and programs are introduced. Since we are typically dealing with the policies of governments, the ethics and political economy of feasibility inevitably constrain research options. For example, in evaluating public safety net programs it is sometimes impossible to set up a baseline survey, since one cannot reasonably delay the program for such a purpose; data collection methods must adapt accordingly, and this can be costly. There is also a potential, but under-explored, role for "evaluative monitoring" of development projects, whereby the tools of project monitoring are modified or extended to allow more rapid feedback to operational staff on the impacts of the program.

None of this makes evaluative research any easier, particularly for Bank lending operations. Even getting researchers invited to the table at the right time, and for a representative sample of operations, can be very difficult. And practical/logistic difficulties in designing and implementing sound evaluations abound. But the hurdles can be overcome.

The second type of cross-cutting research can be termed "methodological research." Practitioners carry a set of tools to their project and policy dialogues with governments, including data, measures and models. Researchers can usefully assess those tools and help improve them. Are the data sound? Do the measures used have a clear theoretical foundation? In particular, are the measures the most relevant ones to the specific policy problem? (The measures used should be adapted to the objectives and constraints relevant to each policy problem, though this is not as common as one might think.) Are the assumptions made in modeling both realistic and internally consistent?

Methodological research can help expand the tool kit routinely employed by policy makers and analysts; this includes the data collected and the methods used to analyze those data, including software. The Bank has become a major producer of development data, and researchers have played a crucial role. A number of the Bank's most successful data initiatives started as research projects. (The Living Standards Measurement Study, which started as a research project around 1980, with the aim of greatly improving household survey data in developing countries, is an example.) Theoretical work often points to new tools, some more useful than others. Choosing wisely is key; some tools are more promising for practitioners than others. Researchers can have great value to practitioners in demonstrating the usefulness of new analytic tools in real applications, often working with operational colleagues. Over the years, DECRG researchers have developed numerous software programs that have facilitated data analysis throughout the Bank and in client countries.

Choosing research questions

The dialogue with policy makers and advisors is vital to finding relevant research questions, and prioritizing the options. Many academic researchers draw their ideas solely from the work of other scholars. Bank researchers tend to draw more on inputs from non-researchers actively involved in making and thinking about policy in developing countries. Researchers at the Bank are in an enviable position to learn from practitioners, including in the field. This potential to learn from practice is reinforced by the opportunities for mission support and by the Bank's convening power. (Research staff in DECRG spend about one third of their time directly supporting the Bank's work outside DECRG, much of which is in direct support to Bank operations in analytic work and projects in developing countries.)

But not every relevant question that should be addressed by the Bank's researchers is currently being asked by practitioners. There are examples in which Bank researchers have explored issues that were not being asked in operations at the time, but became relevant later. For example, Bank researchers had pointed to concerns about whether deposit insurance was appropriate in countries with poorly developed financial institutions and weak regulatory environments; and the research findings eventually influenced Bank operations. Carefully-chosen "blue-sky" topics can have large pay offs; foresight is often needed to make the Bank's research relevant.

Moreover, not every question asked by a policy maker is a researchable question, or worth researching given what we know. To choose questions, we must know what has already been done on the topic—no use re-inventing the wheel—and what potential there is for adding significantly to current knowledge, such as through new data collection or new analytic methods.

A sound understanding of the relevant theory and methods is crucial for the credibility of the answers researchers give to the questions posed by colleagues in operations and client countries. Researchers need to continually up-date their knowledge. Too often researchers maintain essentially the same tool kit they picked up in graduate school. This stifles creativity and the ability to respond to new and interesting questions.

Advanced training for the Bank's researchers is thus crucial. Here the interaction with academia, notably through seminars and conferences, can serve an important role in maintaining the human capital of the Bank's researchers (and sometimes stimulating new directions for academic research). *Ad hoc* specialized training courses by visiting academics can also help. Maintaining the skills of its researchers is an investment for the Bank, not only in future research outputs, but in future training of Bank staff; the Bank's in-house researchers are an important, and much used, resource for maintaining and enhancing the skills of operational staff, through the Bank's internal training programs. For example, the Bank's researchers have pioneered numerous approaches to policy and program evaluation that later became standard in operational work, and were disseminated in part through training programs.

Publish or perish?

The publication process is an important screening and disciplining device for researchers. Publishing in refereed professional journals helps establish a researcher's credibility (although one should never assume that publishing in even the most "prestigious" journals is a perfect indicator of research quality, given the mistakes made in editorial processes). Publishing also helps DECRG attract and keep the best researchers (whose career options often lie outside the Bank, mainly in academia). And publications are an important dissemination channel, provided the right audience is reached.

However, the aim of DECRG is not to maximize published output, but to maximize our development impact. That depends on the relevance to real-world development problems of the questions we address and the credibility of our answers. Publications can certainly help facilitate impact, as do other research outputs, such as the data sets and software products we produce. But publishing is clearly not sufficient for impact.

Is there a trade-off between publishability and relevance? Thankfully, the fact of being policy relevant is not in itself an impediment to publishability in most journals, though any research paper that lacks originality, rigor or depth will have a hard time getting published. It is by maintaining the highest standards that we assure that relevant research is publishable, as well as being credible when carried to policy dialogues.

However, it must be acknowledged that the research questions that are most relevant to development policy overlap only partially with the set of questions and methodologies that are seen to be in vogue by the editors of the professional journals, at any given time. So Bank researchers will face a tradeoff between relevance and getting published. Provided that scholarly rigor is maintained, the cost to a researcher's CV of doing policy relevant research might not be high, but it would be naïve to think it is zero.

The key point is that the benefits to the Bank, and the development community more broadly, from an in-house research team depend critically on maintaining the quality of that team through recruiting and training <u>and</u> assuring that the incentives of individual researchers are aligned with the institution's overall development objectives. In short: rigor must come hand-in-hand with relevance.