

## Remarks on Bank Research Evaluation

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1. Overall, the report is excellent. It highlights the strengths of the Bank's research. Indeed, its predominant role in development research is so strong that, were it involved in the production of an ordinary commodity, it might be accused of anti-trust violation, dominating an industry. This dominance gives the Bank special responsibilities. In setting the research agenda, it can lead some important topics to be under researched. The evaluation rightly points out that some of the Bank's research in the past has been "advocacy research," falling far short of normal academic standards. It is a short sighted strategy, for when the weaknesses in the research are uncovered; it compromises the credibility of the positions taken.
2. In these respects, the Bank is much better today than it was some twenty five years ago, in the years following the departure of Hollis Chenery, who had done so much to establish the World Bank's global reputation in research. In those years, much of the research was "advocacy research" (designed not to enhance understanding of what was going on, but to bolster the case for particular positions), those that did not toe the party line felt uncomfortable, and were forced out or left. The integrity of the Bank's research programme has been restored, and this has been a major achievement of recent Chief economists. DEC has been open to new ideas, and has played an important role in introducing new ideas into the Bank. Its study of the East Asia Miracle was a landmark in rethinking development strategies (though the implications of that study for further research and for policies advocated by the Bank were not adequately followed-up). The Bank has led the profession in research on corruption, and in the use of information disclosure as a way of controlling pollution.
3. Even in areas where its results have been questioned, like aid effectiveness, by asking the question are there circumstances in which aid is more effective, it helped redefine the development debate.
4. A crucial, if not the main problem, in the research program in recent years stems, I believe, from the lack of adequate financial support. This has limited hiring, and it is through new hires that any research program is enlivened, broadened, and kept up to date. There needs to be a substantial commitment of additional resources.
5. A vastly underutilized resource is the Bank's evaluation department, which focuses more on the evaluation of processes than on outcomes. The report is correct in emphasizing the gains to be had—both for research and evaluation—if more projects were designed with an explicit objective of evaluating the consequences of different features. (The report may, perhaps, put too much stress on this; it is key for the Bank to keep a balanced research portfolio.)

6. The report does not emphasize as much as I would the gaps in the research portfolio, and the difficulties as Chief Economist of getting those gaps filled. During the East Asia crisis, three questions constantly recurred: (a) What is the effect of capital market liberalization? Does it lead to more growth, or more instability? (b) What is the effect of raising interest rates? Does it lead to a stabilization of the exchange rate, or just to deeper economic downturns? When does tightening monetary policies in a crisis “work”? (c) What makes a country more vulnerable to an economic crisis? Little of Bank or IMF research was devoted to answering these vital questions, and too much of the research that was done seemed directed at justifying the actions taken. While these policies were at the center of discussion, there was little research in or out of the Bank, and in such circumstances; there is an especial need for the Bank to be able to quickly direct resources to answer such questions. Its current structure simply does not give it the ability to do so. How this can be done should be a central question that Management should address. Here are a couple of suggestions:
  - a. The Chief Economist should have a discretionary budget, say of 5 to 10%, of the overall budget, with which to contract outside research or to “buy time” of staff.
  - b. All research projects should have a life time of no more than 5 years, so that at least 20% of the funds would be up for reallocation every year. (This proposal suffers from a problem faced by other “market” mechanisms at the Bank. Staff have to constantly sell themselves. Besides contributing to a sense of insecurity, it means that resources are used in “selling” rather than in “producing.” One of the advantages of outside contractors is that they have a more diversified source of funding. If they fail to find a project that meets the Bank’s needs, they can look elsewhere.)
7. There are also important gaps in its longer run research programs. The Bank has recently emphasized the importance of private sector development, but has almost no research on corporate governance, competition policy, bankruptcy law, industrial policies, or tax policies that would help define what pro-growth policies might look like. This is partly a legacy of the so-called Washington consensus: if it is simply accepted that industrial policies are bad, the question of how best to design industrial policies is not going to arise. Tax policy has been ceded to the IMF; but the IMF has particular perspectives, that may not be consistent with the models or concerns upon which the Bank centers its attention.
8. These problems are not surprising: successful research takes big issues and breaks them down into smaller pieces. Most researchers are focusing on the pieces. But in the process, the big picture often gets lost. A key question, not adequately addressed in the Review, is how to get new issues on the table, how to reset the agenda. This is particularly difficult in periods in which there is limited hiring. (See point 6 above.)

9. But there is another problem: management of research is, in many ways, different from management in other areas. Research managers tend to have views and perspectives on what kind of research should be conducted—often related to their own professional interests. Those promoted to research managers in DEC typically have little desire to leave DEC, and research management is often their comparative advantage. But the inability to change research managers limits the ability to shift research foci. One recommendation: no one should maintain a position as a research manager for a period of more than, say, six years. There needs to be created a position of “senior” research scholar/fellow, with the status and pay of a research managers. A senior research scholar/fellow could return to a position of research manager after, an interlude of, say six years.
10. The focus on advocacy research often impaired learning about what would promote development. Most notable in this respect was research on the transition from Communism to the market, research which has particularly not held up well. Leaving out China and Vietnam, the real success cases, in most of the statistical studies obviously raised questions about the generality of the findings. The question should have been, when does privatization lead to greater efficiency? Does it depend on the pace of privatization? Other characteristics of the economy? The form of privatization? Key policy questions—such as whether it made sense to restructure before privatization, or to ensure that there were good corporate governance laws in place—were not addressed.
11. This is a general theme: the belief that certain policies always worked meant that the more relevant questions of when the policies worked were not addressed. Trade liberalization may work on average, but no country views itself as average. Each country wants to know whether in its circumstances is it likely to work. For instance, there is evidence that growth benefits are not likely to occur when the unemployment rate is high or where the binding constraints lie in the infrastructure or the financial sector
12. There is a third general problem, less serious at the Bank than at the Fund, but still a serious impediment to policy relevance: the standard models taught in graduate schools in the U.S. and Europe are of limited relevance for developing countries. They assume perfect information, perfect markets, no unemployment, etc. These assumptions are questionable even in advanced industrial countries, but they are particularly inappropriate for developing countries. The report questions the extensive use of CGE models. I agree. But more disturbing is that virtually all of the research uses full employment models, making the results of questionable relevance, e.g. in a country with 25% unemployment.
13. Because the basic models are often so inappropriate, there is a great need for the Bank to sponsor more fundamental research into the kinds of models that are more appropriate for developing countries. This lacuna was not sufficiently stressed in the Review.
14. The synergies between graduate education and research (the economies of scope) explain the success of the research universities; they play a central role

in the Innovation System of the most successful countries. That was why, during my tenure as Chief Economist, I explored the possibility of creating a World Bank Graduate School of Development (WBGSD). There were institutional and financial impediments that could not be overcome in the short time I served as Chief Economist, but I believe that the idea requires continued exploration. Simply put, graduate education in the advanced industrial countries—where most of the Bank researchers receive their training—is focused on the economics of the advanced industrial countries. Few learn the lessons from comparative economic development. Few begin by studying the distinctive features of emerging markets. The lowering of transportation and communication costs provides new opportunities that were not possible a decade ago, e.g. linking the WBGSD with a number of graduate programs around the world, with students spending part of their time in their home school, part of their time at WB headquarters, and most of their time in the field.

15. The report highlights problems in gathering and managing underlying statistical data. It seems to me that this is a question that needs to be resolved at a higher level: the UN should probably take responsibility for the overall management of statistical data, a kind of Central Statistical Agency. There is a danger in operating agencies being responsible for data—evident in IMF growth projections, but evident even in estimates of inflation. At the same time, there needs to be a close link between the keeper/managers of data and the users of data, and the Bank (or the Fund) may argue, in certain limited cases, for the maintenance of their own data banks.
16. I share some of the concerns with the flagship reports. But the review does not fully understand the role these reports play in stimulating discussions of development policy around the world. The reforms I would advocate differ slightly from those emphasized in the Review:
  - a. There should be less emphasis on “best practices” and more on “alternatives,” “trade-offs,” “uncertainties” and the conditional nature of advice.
  - b. There should be more “authored” chapters, less “Bank policy” statements: authored chapters would allow a clearer and more forceful statement of policy perspectives, as opposed to the watered down consensus statements that too often dominate such reports.
  - c. The flagship reports contribute to the advocacy style of research; the reforms I suggest would help alleviate this problem.
  - d. I disagree with the view that the “macro” (Global Development Prospects) and “finance” reports be eliminated, though they may be consolidated. It is important for the Bank to comment on current events, and the perspective of the Bank can be markedly different from that of the Fund. Fund forecasts are of limited value, because they have to be consistent with their policy stances. These reports have made major contributions to the policy debate.
17. I agree that one of the ways in which the Bank promotes development is to encourage better research around the world, and in particular in developing

countries. That was why I instituted guidelines for encouraging joint research with those in developing countries and worked to create the Global Development Network. But given the dominant role that the Bank has in development research, there is a real need for care. There is a risk that the Bank uses its financial resources to divert research towards the agenda which it is pushing, and away from concerns of those in the country; or that it push the current development fads in the developed world. It can lead to a strengthening of an Orthodoxy. Some of these fears are surfacing in the context of Bank supported research in Africa, as brought out forcefully in a recent meeting of the IPD Africa Task Force in Manchester, attended by Prime Minister Meles, among others. The GDN Africa Growth Project had put forth a set of views on the constraints or influences on growth in Africa which was strongly challenged, especially by African participants not associated with the Growth Project. Special efforts to maintain diverse perspectives will be required, and probably will have to be institutionalized.

18. I strongly agree with the recommendation of strengthening the visitor scholar program. I also agree that the Bank should consider setting up networks of research/scholar programs (like NBER and CEPR) on particular topics.